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RELATIONSHIPS BETWEEN ALGEBRA, DIFFERENTIAL EQUATIONS  
AND LOGIC IN ENGLAND, 1800-1860.

MARIA PANTEKI

A thesis submitted in partial fulfilment of the  
requirements of the Council for National Academic  
Awards for the degree of Doctor of Philosophy

Middlesex University

December 1991

To Tasos and Tsitsi.

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## **Abstract.**

This thesis surveys the links between mathematics and algebraic logic in England in the first half of 19<sup>th</sup> century. In particular, we show the impact that De Morgan's work on the calculus of functions in 1836 had on the shaping of his logic of relations in 1860. Similarly, we study Boole's background in D-operational methods and its impact on his calculus of logic in 1847.

The starting point of the thesis is Lagrange's algebraic calculus and Laplace's analytical methods prominent in late-18<sup>th</sup> century French mathematics. Revival in mathematical research in early-19<sup>th</sup>-century England was mainly effected through the diffusion of Lagrange's calculus of operations -as further developed by Arbogast, Servois and others in the 1800's- and of Laplace's theory of attractions.

Lagrange's algebraic calculus and Laplace's methods in analysis -particularly on functional equations- were considerably developed by Herschel and Babbage during the period 1812-1820. Further research on the foundations of the calculus of operations and functions was provided by Murphy, De Morgan and Gregory in the late 1830's.

Symbolical methods in analysis were further extended by Boole in 1844. Boole was followed by several analysts distinguished for their obsession in further vindicating these methods through applications on two differential equations which originally appeared in Laplace's planetary physics.

We record the main issues of De Morgan's logic and their mathematical background. Special reference is given to his logic of relations and its connection with his foundational study of the calculus of functions. On similar lines we study Boole's algebraic cast of logic drawing consequently a comparison between his two major works on logic. Moreover, we emphasize his epistemological views and his evaluation of symbolical methods within logic and analysis.

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## Chapter 0.

### Introduction.

#### 0.1 Outline of the thesis.

Chapter 1 provides a brief background in French mathematics during the period 1770-1830. We start our account with Lagrange's Mécanique Analytique [1788] and Laplace's Mécanique Céleste [1799-1825] and consequently we discuss those issues of Lagrange's analytical techniques which had particular impact on English mathematicians.

Lagrange's assumption that any function can be expanded in a Taylor series, together with his cast of Taylor's theorem in symbolic form, had a considerable impact on Laplace, Arbogast, Français and Servois during the period 1770-1810. English work on symbolic algebras actually originated in the development of an algebra of differential operators effected by these analysts. The chapter ends with a review of operator techniques applied by Fourier, Poisson and Cauchy in the 1810's and 1820's and with a brief account of semiotica, as featured in Condillac [1780]. The latter's influence on the eminent textbook writer Lacroix is recorded.

Various reforms took place in Irish, Scottish and English universities and institutions during the first two decades of the 19<sup>th</sup> century. Of most importance was the reform at Cambridge effected by members of the Analytical Society from 1813 onwards. Herschel, Babbage and Peacock translated in 1816 the abridged textbook of Lacroix on the calculus, producing four years later a volume of examples: both books formed standard part of the Cambridge curriculum up to the early 1840's.

Herschel and Babbage actively worked on the calculi of operations and functional equations during the period 1812-1820. Herschel drew on Laplace's method of generating functions, Arbogast's method of the separation of symbols of operation from those of quantity and Brinkley's work on Lagrange's theorem. He contributed substantially in the calculus of finite differences and the calculus of operations.

Babbage closely collaborated with Herschel during that time. Influenced by Monge, Laplace and Herschel, he tried to further generalize the existing methods for the solution of functional equations. Constantly drawing on analogies from algebra, Babbage proposed his own general method for the solution of functional equations.

Chapter 2 opens with an account of the reforms in British universities during the first two decades of the 19<sup>th</sup> century and then focuses on Cambridge. Our study of Herschel's and Babbage's work concludes with a study of the latter's paper on the influence of signs on mathematical reasoning published in 1827.

Chapter 3 is devoted to a study of the transitional period 1820-1840 in Cambridge University. We comment upon the Tripos exams and on the diffusion and development of physical astronomy by Airy, Whewell and Murphy. Finally we focus on the foundational studies on the calculi of functions and operations by De Morgan and Murphy in 1836 and 1837 respectively. De Morgan's article on the calculus of functions, [1836], included a study of abstraction and generalization, as well as of the properties of inverse functions and it had a strong influence on his shaping of his logic of relations around 1860.

Two years later, Gregory proposed a foundation of the method of separation of symbols consequently applying this method for the solution of differential, finite difference and functional equations. His work, limited to commutative operations, was extended by Boole for the symbolical solution of differential equations with variable coefficients in 1844.

Chapter 4 deals largely with Boole's "On a general method in analysis" [1844]. Besides Gregory, lesser known figures, such as Greatheed, Gaskin, Bronwin and Ellis are noted, particularly in connection with the earth-figure equation, which had originally appeared in Laplace's Mécanique. Gaskin was the first to provide a solution by symbolical procedures. We cover the period 1839-1845, focusing on the integration of the earth-figure equation as the motivation par excellence for English analysts to develop D-operational methods in analysis.

Boole's general method in analysis attracted a remarkable interest among his contemporaries. During the period 1846-1863 we

notice an abundance of papers on the calculus of operations by Bronwin, Hargreave, Donkin, Carmichael, Graves, Russell and others. These analysts extended Boole's method so that it tackled hopefully the whole theory of differential and integral calculus. They focused on the ordinary differential equation which describes the figure of the earth and the partial differential equation, called after Laplace, on the attraction of spheroids.

The latter equation presented with numerous problems. Whewell and Murphy provided a study of the Legendre functions called by the former as "Laplace coefficients". Hargreave integrated the equation of Laplace's coefficients in finite form in 1841. But it was Boole's method that proved more efficient for its symbolical solution in 1846-7.

In chapter 5 we discuss the various extensions and applications of Boole's general method during the period 1846-1863. We acknowledge minor figures such as Curtis and Williamson working on the earth figure equation, as well as Carmichael's treatise on the Calculus of operations [1855]. The chapter includes a brief review of the contributions of Boole, Donkin, Hargreave and others on the integration of the Laplace equation in finite symbolic form, and of the work of Boole's and Carmichael's followers in the calculus of operations up to 1863. Links between the development of the calculus of the operations and the rise of invariant theory by Cayley and Sylvester at that time are also noted.

The revival of logic in early-19<sup>th</sup>-century England was due mainly to Kirwan, Whately, Bentham and Hamilton. The former two perceived analogies between logic and mathematics, the latter two independently introduced the issue of the quantification of the predicate. A dispute between De Morgan and Hamilton on this issue in the mid 1840's had a double effect: on one hand it motivated De Morgan to modify Hamilton's system and thus enlarge and elaborate traditional Aristotelian logic; and on the other hand it motivated Boole to cast traditional logic in a mathematical form.

The two logicians instituted different aspects of algebraic logic. Despite his appeal to mathematical methods and analogies, De Morgan's logic was in large traditional. His main innovation

though, was his calculus of relations directly influenced from his study of abstraction and of inverse functions as in his article [1836]. Boole's application of symbolical methods was far bolder than that of De Morgan's. Conversion and syllogism were treated strictly mathematically. Moreover, Boole investigated deeper than De Morgan the links between the two sciences. An outcome of our research is the illustration of Boole's view that his algebraic calculus of logic and symbolical algebra form two independent parallel branches of a wider calculus of symbols.

Chapter 6 opens with a brief account of the revival of logic in England during the period 1807-1841. We next focus on the main issues of De Morgan's investigations in logic from 1831 up to 1860, namely: the double copula, the composition of relations, the numerically definite syllogism, the quantification of the predicate and the form-matter distinction. The rest of this chapter deals with the influence of his article on functions [1836] in the final shaping of his calculus of logic in 1860.

Chapter 7 focuses on Boole's early development of algebraic logic. First we give an account of his life, his metaphysical and educational concerns and a brief outline of his work on the calculus of operations and logic during his life-time. We next deal with a study of the direct or indirect influences of his general method in analysis, as in his [1844;1845d] on construction of his Mathematical analysis of logic [1847a].

Chapter 8, devoted also to Boole, opens with a study of the largely ignored transitional period 1847-1853. Then follows a study of his Laws of thought [1854] together with a comparison of this work with his [1847a]. We cover additionally Boole's evaluation of symbolical methods within logic and mathematics during the period 1854-1860, concluding with a critical comparison between Boole's and Gräy's epistemological doctrines.

Chapter 9 contains the conclusions of our investigation. We discuss the reception of Boole's and De Morgan's logic by their contemporaries and followers during the period 1860-1900. We also deal with the reception of symbolical methods within analysis and physics. Our overall aim is to explain the interest of English and Irish analysts in symbolical methods and evaluate the success of their applications in a variety of domains such as

pure mathematics, physics, chemistry and logic. In the course of this chapter we will point out certain observations which are worth of notice as a basis for further research in the future.

## 0.2 Literature review; the novel aspects in this thesis.

English mid -19th- century algebra and logic have been considerably studied in our century. The most important works on the former are [Clock 1964, Nagel 1935, Nový 1968, 1973 and Pycior 1981, 1982a, 1983, 1987]. De Morgan's logic of relations is discussed particularly in [Hawkins 1979, Merrill 1977, 1978, 1990 and Prior 1962] and Boole's calculus of logic is extensively studied in [Corcoran 1980, Hailperin 1976, 1981, 1984, Styazhkin 1969 and Van Evra 1977] as well as in standard books on the history of logic. Certain of the works mentioned above aim at a remodelling of De Morgan's and Boole's calculi of relations and logic upon rigorous, modern standards. But hardly anyone views the exact genesis of their logic or considers their mathematical background.

As far as the history of the calculi of operations and functions is concerned we have above all to mention Pincherle [1912] and Cooper [1952] for their acknowledgment of largely ignored French, English and Irish analysts who contributed to their development. In particular, Cooper's brief paper is an interesting attempt to provide a background of operational methods, now often linked only with Heaviside for his application of them in physics [see also Petrova 1987].

More recent papers on the calculus of functions and operations have been published by [Dhombres 1986, Koppelman 1971 and Youschkevitch 1977]. Particularly on Babbage we also have [Dubbey 1978]. However, Herschel's and De Morgan's work is only lightly touched upon. Koppelman's work formed the main motivation for this thesis. She refers to the most important English and Irish analysts who contributed to the development of the calculi of functions and operations, focusing on its impact in the genesis of abstract algebra in mid-19<sup>th</sup>-century Britain. A further study of this impact on Boole was carried out by [Laita 1977].



Based upon the information provided by Koppelman, important aspects of the history of the matter under study emerged that were surprisingly omitted in her work. Observing the emphasis of mid-19<sup>th</sup>-century analysts on the earth-figure equation and the Laplace equation I posed several questions: Why were these analysts so much obsessed with operational methods? Which is the proper evaluation of symbolical methods in mathematics? How far did their contributions prove useful in the solution of differential equations of physics?

Most of these questions were provided with answers through a consultation of standard textbooks on differential equations in Cambridge University, such as Boole [1859] and Forsyth [1914], of treatises on physical astronomy and particularly on the Laplace coefficients, such as Todhunter [1873, 1875] and Whittaker [1952] as well as of reports to the British association for the advancement of science during the period 1850-1880. I was justified for the importance of my research in the earth figure equation particularly when through Forsyth I became acquainted with Glaisher [1881]. In his article, long enough to stand on its own as a treatise, Glaisher incorporated instances of most of the methods known for the solution of this equation which he had earlier shown that is deducible from the widely known Riccati equation.

The major novelties of the thesis consist of the following: minor or major information concerning unknown aspects of Babbage's and Herschel's research; De Morgan's work on the foundations of the calculus of functions; connections between Boole's operational methods and logic; the comparison between Boole's two main books on logic; a comparison between Boole's and Gratry's epistemology; clarification on certain issues in De Morgan's logic and particularly the links between his foundation of functions and his logic of relations.

The study of the aspects mentioned above was considerably helped by the consultation of the Babbage-Herschel and Boole-Cayley correspondences held in the Royal Society Library. In the course of our study certain minor figures, such as Sarrus, Kirwan, Gaskin, Solly and others, have been rescued from (near) oblivion.

### 0.3 Structure of the thesis; citations.

The thesis consists mainly of nine chapters. Follow the endnotes and the bibliography where the dates of all people known are provided. Each chapter is divided in 8-10 sections. By [5.4] we refer to the fourth section of chapter 5. No subsections are used. Formulae are denoted accordingly as (53.2), i.e. the second formula of section 5.3. Endnotes are numbered 1, 2, 3, ... in each section and cross-reference to them in text is provided as [5.3, (9)], i.e. endnote 9 of section 5.3.

Normally, references to the bibliography are given as [Koppelman 1971,180,fn 2], 1971 the date of publication, 180 the page number, and 2 the footnote to which we refer. If we have two publications of the same author in the same year we use [Smith 1982a,10]. Often, instead of the page number we may provide, for reasons of simplicity, the number of the article, [De Morgan 1836,art.6]. If obvious from the context, the name of the author will be omitted, e.g. we may have [1836,art.6], De Morgan implied as the author.

Books and manuscripts are cited with their abbreviated name and code number respectively. For example, Boole's Mathematical analysis of logic [1847a] is cited [MAL,6] instead of [Boole 1847a,6]. Further, his manuscripts with code number  $W_3$  are cited as [ $W_3$ ,3] (if pagination is available). In all such cases we will explain in text or in an endnote the mode of citation used. My own insertions into quotations are enclosed into square brackets. Often an endnote will serve as a means for cross-reference to a specific citation in text.

Problems arise some times as to the date-reference of certain books, journals of encyclopedias. For example, the first edition of Boole's Differential equations appeared in 1859, and by [1859] we will thus generally refer to this book. But if we provide a quotation from a later edition we will clarify this point and use, for example in this case, [1877, 380]. As a rule, all papers are cited by the date of publication of the volume of the journal where they appeared. However, particularly with Encyclopedia Metropolitana, we pay attention to the fact that all

volumes are "officially" dated as 1845. Whenever available, we cite an article by the actual date of its publication, as in the case of [De Morgan 1836].

#### 0.4 Acknowledgments.

I am indebted to Nikos Kastanis, my colleague in the mathematics department of Aristotle University of Thessaloniki, who initiated me in the history of mathematics. Both Nikos and Vasilis Kalfas, professor of epistemology in the University of Crete, reinforced my decision to delve into the history of logic by arguing on the importance of the history of science, a subject neglected in Greek Universities up till recently. Thanks to my former MSc.supervisor on logic, Dr.W.Hodges of Queen Mary College, London, I got in touch with Dr.Ivor Grattan-Guinness of Middlesex Polytechnic who at once encouraged my plans and undertook the supervision of my thesis on the history of mid-19<sup>th</sup>-century algebra and logic in England. Professor K.Lakkis of my department unfailingly supported my plan and stood by my side; thanks to him I was awarded a scholarship to study in London for four academic years.

In London I was warmly received by Ivor, Mr.Allan Findlay, my second supervisor in Middlesex Polytechnic, and Niccolo Guicciardini who has now published his thesis on 18<sup>th</sup>-century English fluxional calculus as [1989] under Ivor's supervision. Ivor constantly helped in a variety of ways. He revealed to me all the subtle clues of historical research, guided me to all secret corners of the numerous London libraries, encouraged my participation in lectures and seminars and motivated stimulating contacts with historians all over the world. All his advices proved to be invaluable, not only in the gradual shaping of my thesis, but in enriching my whole life!

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I want to thank the librarians of the Senate House and of University College for permission to use De Morgan's manuscripts. I am also greatly indebted to the librarians of Trinity College of Dublin, Cambridge University and Aberdeen University for providing me with information on rare books and manuscripts. I had also access to the voluminous Babbage-Herschel correspondence, Boole's manuscripts and rare books and journals in the Royal Society Library in London whose librarians were tireless in responding to my innumerable requests. I would like to acknowledge personally Nina Cohen, Sally Grover and Alan Clark for their friendly encouragement. Sally went through part of my work correcting syntax mistakes, while Alan was an expert in digging up bibliographical and biographical information on neglected scientists. Sally and Alan willingly undertook to help me while I was writing up my thesis in Greece by providing valuable material and letters which filled me with joy. Very few librarians combine their personality, culture, professional standards and respect for intellectual work.

The key to all these precious contacts was Ivor. He stood by my side for seven full years and I suspect no Ph.D. student ever held such a voluminous and substantial correspondence with a supervisor as we did! My friend Alison Walsh, successor in Ph.D. research under his guidance, was very keen in doing some extra work for me in the British Museum Library the last couple of years. All my friends showed a touching enthusiasm in my work requesting to see it when ready; Harris deserves of special notice here.

Many thanks go to Maria Halatsi and Telemachos Stamkopoulos for their efficiency in word processing and for their tireless corrections of my voluminous drafts. I take the full responsibility for any errors left as the time can never be enough for such a bulky work.

This thesis, however, would never had been realized had it not had been for the understanding and collaboration of Professor Lakkis who freed me from the worry of limited time. Above ALL, I want to thank my parents who went through real sacrifices all these years; the least I can give in return to their devoted love is to devote my thesis to them, aware that there are things in life which can never be fully acknowledged.

Hoping I have not committed any serious omission and thanking my friend Athena for preventing me from prolonging this best part of my thesis to a thesis in itself.

Maria Panteki

## Chapter 1

### Background in French mathematics : 1770-1830.

#### 1.1 Introduction.

Theoretical physics of the boldest and most difficult sort has been forced to abandon images and the whole notion of visual and motor representation. In its effort to subjugate its vast domain -to unify its laws and make them independent of the place, time, and movement of the observer- it has no other guide than the symmetry of its formulas<sup>(1)</sup>.

Paul Valéry.

The main feature of late-18<sup>th</sup>-century terrestrial and celestial mechanics in France was the prominence of analytical methods over geometrical methods. The major figure responsible for the algebraic cast, not only of mechanics but also of the calculus, was J.L.Lagrange. His notation and mathematical methods were to influence considerably certain of his contemporaries, as well as the mathematical thinking of English and Irish analysts in the period 1800-1860.

The language of Lagrange's algebraic calculus was characterised by a tendency for abbreviation, symmetry and unity. Moreover, the calculus of operations, which largely originated in his works, was to be greatly admired at the turn of the century in both France and England as a useful tool for discovery through the possibility it offered for abstraction, generalization and unity of methodology.

Silently influenced by Lagrange, another figure to be acknowledged for the mathematization of mechanics and for the introduction of new analytical methods, is P.S.Laplace<sup>(2)</sup>. It was mainly through his work on physical astronomy that interest in mathematical and physical research in early-19<sup>th</sup>-century England revived. In particular, his theory of attractions and of the figure of the earth had considerable impact on the development, not only of theoretical physics, but also of the symbolical methods for the solution of differential equations which were to flourish abundantly in mid-19<sup>th</sup>-century England. However, Laplace himself avoided Lagrange's use of symbolic arguments, as we shall see below.

It is a known fact now<sup>a</sup>days that French research in the theory of the figure of the earth and in differential equations developed almost side by side in the 1740's<sup>(3)</sup>. The interaction between these two domains contributed in the further development of each one. However, the nature of this interaction in 19<sup>th</sup>-century England has not yet been thoroughly studied.

The scope of this chapter is to provide a brief background of the most significant theories which were developed in late 18<sup>th</sup>-century France as necessary for our study of the growth of operational methods in analysis and of mathematical logic in England during the period 1800-1860. For this reason we introduce first Lagrange's Mécanique Analitique<sup>(4)</sup> [1788] in which his calculus of variations appeared prominently together with instances of his calculus of operations [1.2]. Then follows Laplace's Mécanique Céleste [1799-1825] prominent for the author's skillful use of expansion in series in his theory of attractions. Above all we show the proximity between this theory and the earth-figure equation which became important for its role both in physical research and in the development of operational methods in England [1.3].

We focus next on the solution of differential and functional equations acknowledging the contributions of Lagrange, Monge and Laplace [1.4]. In the next section we cover the early treatment of Lagrange's theorem important for the analogy between exponentiation and differentiation indices which it brought to light. Laplace's proof of it, by his method of generating functions, and Arbogast's cast of it in symbolical form are mentioned [1.5].

Consequently we acknowledge Brisson, Français, Servois and Sarrus for their contributions in the calculus of operations in 1800-1822. Servois introduced the concepts of commutativity and distributivity, whereas Sarrus extended Servois's theory for inverse operations the study of which -being of most importance for the development of the calculus of operations- was omitted by Servois.

As it is well known, around 1820 Lagrange's algebraic calculus -based on the Taylor expansion of functions- was replaced by Cauchy's limit-based calculus. In the course of this program, Cauchy, resuming Brisson's unpublished work on the application of symbolical methods for the solution of differential equations, rigorously founded the calculus of operations. Cauchy's work had

no immediate impact on the English<sup>(8)</sup>. Of more important impact though, was Fourier's solution of partial differential equations in symbolic form in 1822. Finally, Poisson is also acknowledged for his contributions in partial differential equations in the 1820's [1.7].

The last section is devoted to the epistemology of Condillac and Condorcet at the turn of the century and its impact on the eminent textbook writer S.F.Lacroix. Among the French philosophers and scientists of that period, Degerando and Carnot are also mentioned for their subtle influence on English algebraists such as Babbage and De Morgan [1.8].

## 1.2 Lagrange's Mécanique Analitique [1788].

With Lagrange, mechanics is for the first time allegedly deprived of any geometrical or physical considerations. The book avoids diagrams; and physical concepts, such as pressure, tension or forces of constraint, are replaced by purely mathematical equivalents. The two principles of physics that formed the basis of his work were D'Alembert's principle and that of virtual velocities<sup>(1)</sup>.

From the point of view of methodology, Lagrange's innovation was his peculiar application of the calculus of variations. The invention of his notation "gave distinctness and permanence to the new method, securing it from being confounded with the other infinitesimal methods, to which it is in some degree similar". Through elaborating and extending the new science of analytical mechanics by his application of variational principles, Lagrange is "justly reputed the inventor of the calculus of variations"<sup>(2)</sup>.

His first account of the calculus of variations was given in his "Essai" [1762]. He remarked that the increments which functions receive in consequence of a change in their form are quite analogous to the increments they receive in consequence of a change of their independent variables. To distinguish this difference he denoted the former by  $\delta$  and the latter by  $d$ . This analogy furnished him with an ability to tackle problems of maxima and minima<sup>(3)</sup>.



In 1774 Lagrange was to notice another important analogy: between indices of differentiation and exponentation. Though this observation dates, like the calculus of variations, quite earlier<sup>(4)</sup>, it was upon what was to be called "Lagrange's theorem" that he founded the calculus of operations. This theorem was introduced as follows:

$$(12.1) \quad \Delta^n u = (e^{\frac{du}{dx} + \dots} - 1)^n,$$

where  $n$  integral number [1774, 194; see also 1.5]

Variational principles and operator methods were combined in certain instances of Lagrange's mechanics. For example he introduced operator laws such as

$$(12.2) \quad \delta dx = d\delta x$$

[Fraser 1985; Grattan-Guinness 1979, 84].

One of the best well-known of Lagrange's contributions to mechanics is his equations -now named after him- for the motions of a dynamical system. Starting with the basic problem in the calculus of variations, that is to determine the conditions that render the integral

$$(12.3) \quad J = \int_{x_1}^{x_2} f(x, y, \frac{dy}{dx}) dx$$

a maximum or a minimum (provided with the possibility to distinguish between the two), he derived the equation

$$(12.4) \quad \frac{df}{dy} - \frac{d}{dx} \left( \frac{\partial f}{\partial (dy/dx)} \right) = 0.$$

Called later as the Euler-Lagrange equation, (12.4) formed a necessary, though not sufficient, condition for the problem (12.3). Next he deduced his motion -equations, first in rectangular coordinates,  $x, y, z$ ,

$$(12.5) \quad \frac{d}{dt} \left( \frac{\partial T}{\partial x} \right) + \frac{\partial V}{\partial x} = 0 \quad \text{etc},$$

and then in generalized ones,  $q_1, q_2, q_3$ ,

$$(12.6) \quad \frac{d}{dt} \left( \frac{\partial T}{\partial (dq_1/dt)} \right) - \frac{\partial T}{\partial q_1} + \frac{\partial V}{\partial q_1} = 0,$$

where  $x = x(q_1, q_2, q_3)$  etc,  $V$  the potential function and  $T$  the kinetic energy given by

$$(12.7) \quad T = \frac{dx^2 + dy^2 + dz^2}{2dt^2} \text{ (5)}.$$

Lagrange aimed to prove, via his two motion equations and his variational methods, the principle of the conservation of living forces.

$$(12.8) \quad T + V = \text{const.}$$

and the least action principle

$$(12.9) \quad \left( \int T dt = \text{max or min} \right) \longleftrightarrow \delta \int 2T dt = 0 \text{ (6)}.$$

In fact he managed to provide a very long and complicated proof only for (12.8) by means of (12.5).

Lagrange's terrestrial mechanics was highly influential on his contemporaries. However, historians are not surprised by the difficulty experienced by his followers in grasping his ideas. For Lagrange espoused the fundamental principles of his work without verification, aiming mainly to vindicate them by illustrating their fertile applications [Mach 1902, 436]. Moreover, his notations were difficult to follow and for this reason we have avoided citations from his original work.

What we want to stress is the abstract nature of his procedures, his tendency for generalization and the symmetry of his forms. The impact of Lagrange's Mécanique in Britain was not immediate but considerably important. In 1841 Boole provided a simple and elegant proof for (12.8)-(12.9) based upon the formulae (12.6) [Boole 1841b]. It is important to notice that through the motivation provided by Lagrange's combination of variational and operational methods, Boole contributed substantially in the development of the calculus of operations [chap. 4].

The "first really competent presentation of Lagrange's idea" is attributed to the Irish J.H. Jellett [see (7) below]. In his elementary treatise on the calculus of variations [1850], Jellett adopted closely Lagrange's style treating his subject in the context of the calculus of operations. Jellett's operational methods were of considerable influence on his country-fellow R.

Carmichael<sup>(7)</sup>. The latter is acknowledged for his original application of operational calculus on partial differential equations [chap. 5].

### 1.3 Laplace's Mécanique Céleste; the equations of Laplace's coefficients and of the figure of the earth.

Laplace's Mécanique consists of 5 volumes. Volumes 1 and 2, published in 1799, deal respectively with the motions of heavenly bodies in general and with the mathematical foundation of planetary physics. Volumes 3 and 4, published in 1802 and 1805 respectively, focus mainly on problems regarding comets and perturbations of planets. Volume 5, published in parts during 1823-1825, bears a strong influence from Fourier's theory of heat. The theory of attractions and of the earth's shape are included mainly in Volumes 2 and 5 which we will make use of in this section<sup>(1)</sup>.

The characteristic feature of Laplace's early work on mechanics is his adoption of the view that all physical phenomena could be described in terms of Newtonian, inter-particular forces. He believed that through this approach the results would be clearer than through Lagrange's alternative analytical approach. No diagrams appear in his work either, and the state of equilibrium is equally emphasised as by Lagrange<sup>(2)</sup>.

In his authoritative presentation of the theoretical aspects of mechanics, Laplace's mathematization of this topic was less abstract and general than that of Lagrange<sup>(3)</sup>. Most important problems in physical astronomy were presented in the form of a differential or finite difference equation. Laplace's favourite method, adjusted each time to the specific problem involved, was that of his "generating functions" [1.5]. Particularly in dealing with the theory of attractions, as we shall see, he assumed an unknown function to be expandible in infinite series (convergence taken for granted). Then, by investigating the properties of the unknown coefficients involved, he arrived at an approximate solution of an equation. Quite often he would provide a solution without any explanations.

Given a spheroid of density  $\rho$  at point  $(x,y,z)$  and an external particle of coordinates  $(f,g,h)$ , the attraction  $V$  between them is

$$(13.1) \quad V = \iiint \frac{\rho dx dy dz}{[(f-x)^2 + (g-y)^2 + (h-z)^2]^{1/2}} .$$

It is easily shown that  $V$  is a solution of

$$(13.2) \quad \frac{d^2 V}{df^2} + \frac{d^2 V}{dg^2} + \frac{d^2 V}{dh^2} = 0$$

[1799a, Book 2, art 11]. This equation, known before Laplace, was and still is named after him. Laplace committed, though, a serious oversight in regarding (13.2) true also for internal particles. Poisson provided the correct equation for this case later, as we shall see below.

Laplace's virtuosity lies in his study of (13.2). By transformation in polar coordinates  $(r, \theta, w)$  and  $(r_1, \theta_1, w_1)$  by means of the relations  $r^2 = f^2 + g^2 + h^2$ ,  $\cos \theta = h/r$  and  $\tan w = h/r$ , (13.1) assumes the form

$$(13.3) \quad V = \iiint \frac{\rho r_1^2 dr_1 d\mu_1 dw_1}{[r^2 + r_1^2 - 2rr_1\gamma]^{1/2}} ,$$

where  $\cos \theta = \mu$ ,  $\cos \theta_1 = \mu_1$  and  $\gamma = \cos \theta \cos \theta_1 + \sin \theta \sin \theta_1 \cos(w-w_1)$ . Consequently, Laplace's equation (13.2) appears in the form

$$(13.4) \quad r^2 \frac{d^2 r V}{dr^2} + \frac{d}{d\mu} \left[ (1-\mu^2) \frac{dV}{d\mu} \right] + \frac{1}{1-\mu^2} \frac{d^2 V}{dw^2} = 0,$$

[1799b, Book 3, art 9] (4).

The integration of (13.4) was far from easy. Laplace assumed  $V$  to have the form

$$(13.5) \quad V = \frac{U^{(0)}}{r} + \frac{U^{(1)}}{r^2} + \dots ,$$

where  $U^{(1)}$  were "rational and integral functions" of  $\sqrt{1-\mu^2}$ ,  $\cos w$ ,  $\mu$  and  $\sqrt{1-\mu^2} \sin w$ . If  $U^{(1)}$  could be obtained, then  $V$  would be approximately obtained too. Substituting the expanded form of  $V$  in (13.4) he arrived at a partial differential equation in only two variables:

$$(13.6) \quad \frac{d}{d\mu} \left[ (1-\mu^2) \frac{dU^{(1)}}{d\mu} \right] + \frac{1}{1-\mu^2} \frac{d^2 U^{(1)}}{dw^2} + i(i+1)U^{(1)} = 0.$$

A very important observation that followed was that (13.6) held also true for the coefficients  $Q^{(1)}$  of  $T = (r^2 + r_1^2 - 2rr_1\gamma)^{-1/2}$ , if the latter was assumed to be developed in a series of the form

$$(13.7) \quad T = \frac{Q^{(0)}}{r} + Q^{(1)} \frac{r_1}{r^2} + Q^{(2)} \frac{r_1^2}{r^3} \dots$$

[1799 b, Book 3, art. 9].

Equation (13.6) is a very important one for its role in other domains of physics besides that of astronomy.  $Q^{(1)}$  became known as the "Laplace coefficients", and any other function that satisfies (13.6) was often called as a "Laplace function". Later authors attribute to them the name of Legendre for he was the first to isolate such functions and study their properties in pure mathematical context<sup>(5)</sup>.

Laplace proved various relations between the coefficients  $U^{(1)}$  and  $Q^{(1)}$  and then introduced the following theorem

$$(13.8) \quad -a(dV/dr) = 2\pi a^2/3 + 1/2 V,$$

assuming that a spheroid of radius  $r$  can be closely approximated by a concentric sphere of radius  $a$ ,  $(dV/dr)$  being Euler's notation for the partial differential [1799b, Book 3, art 10]. Laplace's demonstration of his favourite theorem (13.8), named after him, was regarded as implausible. However, the result is right, and it proved to be very important for its role in the theory of attractions.<sup>(6)</sup>

Laplace took the radius  $r$  of the spheroid in the form

$$(13.9) \quad r = a(1 + \alpha y),$$

where  $\alpha$  a very small quantity and  $y$  a function of  $\mu$  and  $w$ . Substituting  $V$  and  $(dV/dr)$  from (13.5) in (13.8) he deduced the formula

$$(13.10) \quad 4\pi a^2 y = \frac{U^{(0)}}{a} + \frac{3U^{(1)}}{a^2} + \frac{5U^{(2)}}{a^3} + \dots$$

Contemplating upon the form of (13.10), Laplace was led to the conclusion that  $y$  is expandible in a series of Laplace coefficients  $U^{(1)}$ . In other words, he proved indirectly the general

proposition that any function of  $\mu$  and  $w$  can be expanded in a series of Laplace coefficients. It thus follows that  $y$  has the form

$$(13.11) \quad y = Y^{(0)} + Y^{(1)} + Y^{(2)} + \dots$$

where all  $Y^{(1)}$  satisfy the equation of Laplace's coefficients (13.6) ('?'). Another interesting property that he proved was the orthogonality property of Laplace functions of different orders [1799b, Book 3, art 11-12] ('?'). The rest of Laplace's theory of attractions and his presentation of the theory of the figure of the earth depended upon a skillful use of the properties of various coefficients, such as  $U^{(1)}$ ,  $Y^{(1)}$  etc as given so far in this brief outline.

Proceeding to Laplace's deduction of the earth-figure equation, we would like to comment briefly on its interesting prehistory. It was Clairaut who provided first the differential equation which links the ellipticity and density of the earth in 1743. He regarded the earth as a rotating, universally gravitating mass of strata of fluids of varying densities. Two crucial questions puzzled him: 1) what is the ideal shape of this mass when equilibrium is effected? and 2) If the answer to the first question is "elliptical", what is the relationship between densities and ellipticities?

Clairaut founded his study on the plumb-line principle. Via geometrical considerations this principle led to an integro-differential equation which express the relation between ellipticities  $\rho$  and densities  $R$  of the strata in equilibrium:

$$(13.12) \quad 5r^2\rho \int Rr^2dr = \int Rd(r^3\rho) + Fr^3 + \frac{5}{2} A\rho r^3 - r^3 \int Rd\rho$$

[ Clairaut 1743, 262-275; see Greenberg 1979, 566-8].

Given the ellipticity  $\rho$ , the density  $R$  could easily be obtained [Clairaut 1743, 281-3; Greenberg 1979, 575-76]. However, the converse problem proved much more difficult. Assuming  $dr$  as constant, Clairaut differentiated (13.12) arriving at the original form of the so-called later "earth figure equation":

$$(13.13) \quad \frac{d^2\rho}{dr^2} = \frac{6\rho}{r^2} - \frac{2\rho Rr}{\int Rr^2dr} - \frac{2Rr^2}{\int Rr^2dr} \frac{d\rho}{dr}$$

By suitable transformations, he reduced (13.13) to a first-order differential equation which he noticed to be an instance of the "famous Riccati equation"

$$(13.14) \quad dy/dx + y^2 = X(x).$$

But as no method was known for the treatment of (13.14) in the general case, Clairaut further simplified the problem by assuming  $R(r) = r^n$ . Only under this assumption he was able to provide a solution of (13.13) when density  $R$  is given<sup>(9)</sup>.

According to Greenberg, Clairaut's treatment can be regarded as slightly anticipating the potential theory introduced 30 years later [1979, 608-9]. As far as the figure of the earth is concerned, Clairaut was followed by D' Alembert in 1749 and Legendre in 1793 [Todhunter 1873a, 264, 301; 1873b, 109-11, 118]. Laplace and Legendre had been rivals and therefore it is difficult to judge the degree of the latter's influence on the former. However, it was basically Laplace's procedure that had a great impact on most British physicists and so we provide an outline of it.

Assuming, like Clairaut, that the earth is an heterogeneous rotating mass of nearly spherical strata of varying densities  $\rho$ , Laplace assumed that the following law is obeyed

$$(13.15) \quad d\Pi/\rho = Fdf + F_1df_1 + \dots$$

where  $\Pi$  stands for pressure,  $F$  for force and  $df$  for its direction [1799b, Book 3, art 22; Pratt 1836, 541].

Laplace undertook first to prove that the shape of the earth is elliptical and consequently to determine its ellipticity. Based on (13.9), (13.10), the law (13.15) and the properties of  $Y^{(i)}$  determined so far in the third Book, he was led to the equation

$$(13.16) \quad \frac{4\pi a^i}{2i+1} \int_a^1 \rho d \frac{Y^{(i)}}{a^{i-1}} - \frac{4\pi}{3a} Y^{(i)} \int_0^a \rho da^3 + \frac{4\pi}{(2i+1)a^{i+1}} \int_0^a \rho d(a^{i+3} Y^{(i)} + a^i Z^{(i)}) = 0$$

for  $i \geq 1$ , where  $Z^{(i)}$ , determined via the coefficients of  $T$ , satisfies the equation (13.6) [1799 b, Book 3, art 29]<sup>(10)</sup>.

Based upon the properties of  $Z^{(i)}$  [see (10) above] and by differentiation of (13.16) under certain conditions, he reduced the above equation to a simpler form devoid of  $Z^{(i)}$ :

$$(13.17) \quad \frac{d^2 Y^{(i)}}{da^2} = \left[ \frac{i(i+1)}{a^2} - \frac{6pa}{\int \rho da^3} \right] Y^{(i)} - \frac{6pa^2}{\int \rho da^3} \frac{dY^{(i)}}{da}.$$

Now, the integral of this equation, observed Laplace, will give the value of  $Y^{(1)}$  with two arbitrary constants which are rational and integral functions of order  $i$  of  $\mu$ ,  $\sqrt{(1-\mu^2)}\sin w$  and  $\sqrt{(1-\mu^2)}\cos w$  that satisfy equation (13.6) [1799b, Book 3, art 30].

Equation (13.17) is the original, general form, of the earth figure equation. Notice that if  $i=2$ , and  $Y^{(2)}$ ,  $a$  and  $\rho$  are replaced respectively by  $\rho$ ,  $r$  and  $R$ , we get instead Clairaut's equation (13.13) mentioned above. Legendre and Laplace proved in different ways that in the context of the figure of the earth, equation (13.17) has no admissible solution, unless

$$(13.18) \quad Y^{(1)}=0, \quad i \neq 0, 2$$

holds true<sup>(11)</sup>.

According to (13.18) the radius  $r$ , given by (13.9) and (13.11), assumes the form

$$(13.19) \quad r = a + \alpha a(Y^{(0)} + Y^{(2)}),$$

hence, given that the surface of every strata is elliptical, then the external surface of the spheroid is elliptical too, as Laplace remarked. Through another complicated process he proved that

$$(13.20) \quad Y^{(1)} = hU^{(1)},$$

$h$  a function of  $a$  and  $U^{(1)}$  independent of it [1799b, Book 3, art 30; see also (13.5), (13.6), (13.11) above].

Now, it suffices to determine  $h$ , which Laplace names later in volume 5 "ellipticity". Since  $Y^{(0)}$  can be arbitrary [see (10)] it suffices to determine  $h$  and  $U^{(2)}$ . Then, by means of (13.20) and (13.19) the radius of the earth can be determined. Substituting the value of  $Y^{(1)}$  from (13.20) in (13.17), he arrived for the case  $i=2$  to the equation

$$(13.21) \quad \frac{d^2h}{da^2} - \frac{6h}{a^2} \left[ 1 - \frac{\rho a^3}{3 \int \rho a^2 da} \right] - \frac{2\rho a^2}{\int \rho a^2 da} \frac{dh}{da} = 0.$$

Having determined, first  $Z^{(2)}$  and consequently  $U^{(2)}$  through a complicated procedure, it remained to solve the above equation. Given  $h$ , the density  $\rho$  is easily obtained, he observed. But, as with Clairaut's case, the converse problem could not be treated by known methods, remarked Laplace. Notice, once more the equivalence between (13.21) and (13.13).



Reconsidering this problem in 1825, Laplace provided a further simplification for the equation (13.21). As the law of densities he took

$$(13.22) \quad d\Pi/d\rho = 2k\rho, \quad k \text{ constant.}$$

Now, the generalized equilibrium equation was obtained in the form

$$(13.23) \quad \frac{d\Pi}{\rho} = -4\pi \frac{da}{a^2} \int \rho a^2 da$$

[1825, Book 11, art 6].

Solving the former equation for  $\Pi$  and substituting its value in (13.23) he obtained

$$(13.24) \quad \frac{d\rho}{da} = -\frac{n^2}{a^2} \int \rho a^2 da, \quad n^2 = 2\pi/k.$$

Further, by putting  $\rho_1 = \rho a$  and next by differentiation of (13.24), he arrived at

$$(13.25) \quad d^2\rho_1/da^2 + n^2\rho_1 = 0.$$

Solving next (13.25) for  $\rho_1$ , he arrived at

$$(13.26) \quad \rho = (A/a) \sin an + (B/a) \cos an.$$

Now, since the density can not be infinite in the centre, where  $a=0$ , Laplace claimed that  $B$  has to be 0. So, the new law for densities was simplified in

$$(13.27) \quad \rho = A/a \sin an.$$

By rearranging the terms of (13.21) at the right-hand side and replacing  $d\rho/da$  from (13.27), he arrived at

$$(13.28) \quad \frac{d^2[h \int \rho_1 a da]}{da^2} - \frac{6}{a^2} \left[ h \int \rho_1 a da \right] + n^2 \left[ h \int \rho_1 a da \right] = 0.$$

"It is easy to see", he claimed (as usual), that (13.28) is satisfied by

$$(13.29) \quad h \int \rho_1 a da = H\rho_1 \left(1 - \frac{3}{n^2 a^2}\right) + \frac{3H}{n^2 a} \frac{d\rho_1}{da},$$

with  $H$  an arbitrary constant. Moreover, he remarked, since  $(d^2\rho_1/da^2) = -n^2\rho_1$ , we are provided by means of (13.29) with the value of the ellipticity  $h$ :

$$(13.30) \quad h = -H \left( \frac{3}{a^2} + \frac{n^2 \tan an}{an - \tan an} \right),$$

without giving any further explanation [1825, Book 11, art 6].

In fact, it would have been easy to cast the solution (13.29) of (13.28) in a more convenient form by replacing  $p_1$  by  $A\sin(ax+B)$  which follows from (13.26). Thus, the general solution of (13.28) would be given in two arbitrary constants, C and B, in the form

$$(13.31) \quad h \int p_1 da = C \left[ \left(1 - \frac{3}{n^2 a^2}\right) \sin(an+B) + \frac{3}{na} \cos(an+B) \right],$$

[Todhunter 1873b, 337-8] <sup>(12)</sup>.

The usual <sup>a</sup> form in which the earth-figure equation, (13.28), was presented <sub>A</sub> few decades later in works of mathematical context was

$$(13.32) \quad \frac{d^2 y}{dx^2} + n^2 y = \frac{6y}{x^2}, \quad \text{where } y = h \int p_1 da \text{ and } x = a.$$

In fact, (13.32) is a particular case of the general equation involved in the shape of the earth, (13.17). If we assume  $dY^{(1)}/da$  to be very small, and if given the density  $p$  we replace the known quantity  $(6pa)/(\int p da^3)$  by  $n^2$ , then (13.17) assumes the form

$$(13.33) \quad \frac{d^2 y}{dx^2} + n^2 y = \frac{i(i+1)}{x^2} y.$$

Notice that for  $i=2$  we have (13.32).

In volume 5 of his Mécanique, while dealing with the theory of heat, Laplace came across the equation

$$(13.34) \quad \frac{d^2 q}{dz^2} + kn^2 q = \frac{i(i+1)}{z^2} q,$$

where  $k, n$  constants, which belongs to the same class as (13.33). According to his method of generating functions, he assumed  $q$  to have the form of an infinite series. By substituting this form in (13.33) he obtained recursive relations between the coefficients involved in the form of the solution <sup>(13)</sup>.

In fact, Legendre was the first to deal with the class (13.33) in 1793, providing a solution devoid of any demonstration [Todhunter 1873b, 118]. Earlier in volume 5, Laplace vaguely mentioned Legendre in connection with the latter's research on the figure of the earth [1825, Book 11, art 6].

Laplace's theory of attractions was considerably elaborated and developed by Lagrange, Legendre, Poisson and others and had considerable application within ~~electricity~~ and magnetism and other domains of physics besides physical astronomy<sup>(14)</sup>. In 1813 Poisson proved that when the particle is inside the attracting body, Laplace's equation (13.2) obtains the form

$$(13.35) \quad \frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = -4\pi\rho.$$

He also proved directly that any function of  $\mu$  and  $w$  is expandible in a series of Laplace coefficients<sup>(15)</sup>. In 1823 Poisson solved in functional definite integral form an equation which includes (13.33) as a particular case [see 1.7]. However, English analysts worked independently of Poisson's definite integral method during 1837-1850.

The diffusion of Laplace's Mécanique in early-19<sup>th</sup>-century Britain had a considerable impact on the development of theoretical physics, as well as on the growth of mathematical methods, particularly of symbolical methods for the solution of ordinary and partial differential equations. Ivory, Brinkley, Airy, Whewell and Murphy contributed in the development and diffusion of Laplace's theory of attractions and of the figure of the earth during the period 1800-1840. Pratt's treatise, which followed closely Laplace's procedures, deserves of mention too [see 2.1-2.3; 3.1-3.3].

Gaskin was acknowledged as the first to provide with a solution of (13.32) in 1839. R.L.Ellis tackled the general form (13.33) in 1841 followed by Boole in 1844. The latter was followed by Bronwin, Hargreave and others. All these analysts aimed inventing symbolical methods for the solution of a wider class of differential equations, (13.33) being a particular case. [see chapters 4 and 5]. The equation of Laplace's coefficients presented more difficulties and thus subtle methods for its integration were suggested. Hargreave was the first to provide a solution in finite form in 1841. Boole applied symbolical methods in 1846-7 followed by Hargreave, Donkin, Carmichael and others [see chapter 5].

Our research on the methods of solution of the equations (13.6) and (13.33) mainly focuses on the respective work of

English and Irish analysts of mid-19<sup>th</sup><sup>-century</sup> in pure mathematical -in fact symbolical- context. However, the success of these methods can not ignore their actual efficiency within physical context. Impressed by the obsession of these analysts to focus mainly on classes of equations important for their role in physics, and particularly on the two equations examined in this sections, we felt it necessary to offer a brief background of their original formulation and study.

We wanted to stress the great difficulties with which physicists were confronted in the early stage of their study as well as the link involved in their original formulation. Both (13.6) and (13.33) share the factor  $i(i+1)$  in common. We also note that both equations are satisfied by the coefficient  $Y^{(1)}$ . Moreover, the connection between the earth-figure equation (13.13) or (13.32) with the Riccati equation deserves to be noticed.

#### 1.4 Differential and functional equations in late-18<sup>th</sup>-century France.

By mid 18<sup>th</sup> century, ordinary differential equations had formed an independent discipline of study. This was mostly due to the applications of the differential calculus to physical problems. The theory of tides and of the earth's shape were among the earliest significant motivations for development of the integral calculus in general. Riccati, Clairaut and Fontaine were among the first contributors in the theory of ordinary differential equations<sup>(1)</sup>. Significant contributions in the study of ordinary and partial differential equations were provided also during the period 1730-1760 by Euler, D'Alembert and the Bernoulli brothers<sup>(2)</sup>.

Around 1750 the focus of interest was switched from ordinary to partial differential equations, for the latter proved to be more prominent in problems of mechanics. Moreover, the former equations most often occurred as by-products of the latter<sup>(3)</sup>. Lagrange, Monge and Laplace contributed substantially in the development of partial differential equations during the period 1760-1790. The integration of these equations presented new difficulties. A vivid example is the equation of Laplace's coefficients (13.6).

Our interest, however, lies not in the theory of the integration of partial differential equations, but in the genesis of a new branch of analysis which sprang out of the study of the form of their solution. The determination of the arbitrary functions in the solution of partial differential equations, according to the initial conditions of the physical problem, was a main motivation for the study of functional equations. D'Alembert and Euler were the main founders of this new discipline in the 1740's and 1750's. They were soon followed by Lagrange, Monge and Laplace in the 1760's and 1770's. The two latter were to influence significantly the development of functional equations by Herschel and Babbage in the 1810's [chapter 2].

In what follows in this section, we will first comment upon the most common methods which French analysts used for the solution of differential equations as well as upon the form of solutions they preferred. This commentary, together with our review of the earth-figure and the Laplace equations in 1.3, will form our main background in the classical theory of differential equations necessary for our study of symbolical methods in chapters 4 and 5. We will next focus on the theory of functional equations, as shaped particularly by Monge and Laplace. We are very selective in the topics discussed, for the subject of differential equations is too vast to be covered satisfactorily in so limited space. Above all, we would like to stress the fact that largely we will concentrate in this thesis on differential and functional equations in one variable.

We start our historical account with Riccati, who was acknowledged, as we saw in 1.3, by Clairaut as the pioneer in the reduction of second-order ordinary differential equations to first-order ones. While working in the theory of acoustics in 1724, Riccati reduced a complicated second-order non-linear differential equation to an instance of

$$(14.1) \quad dy/dx = a_0(x) + a_1(x)y + a_2(x)y^2.$$

This equation was named after Riccati by D'Alembert in 1763. The most classic form under which the "Riccati equation" was studied is the following:

$$(14.2) \quad dy/dx + ay^2 = bx^m.$$

Riccati simply pointed out that there were integrable cases for (14.2). A further study was carried out by D'Alembert, Euler and

the Bernoulli brothers [Kline 1972, 483-4; Vessiot 1910, 125).

A great interest was aroused in 18<sup>th</sup> and 19<sup>th</sup>-century analysts for the Riccati equation. The main reason for this was the fact that many physical problems led to second-order, partial or ordinary differential equations reducible to cases equivalent to (14.2). We have noticed Clairaut's early observation on the similarity between the earth-figure equation, in the form (13.13), and the Riccati equation, as in (13.14). This similarity was established to be actually an equivalence by Liouville in 1841. Since the latter's study occurred around the time that English analysts applied abundantly symbolical methods for the solution of the earth-figure equation, I would like to give a brief sketch of the early-19<sup>th</sup>-century development of the Riccati equation in France.

Without loss of generality, (14.2) can be written in the form

$$(14.3) \quad dy/dx + y^2 = x^m.$$

If  $y = (1/u)(du/dx)$ , then the Riccati equation can be transformed to a linear second-order equation

$$(14.4) \quad \frac{d^2u}{dx^2} - x^m u = 0.$$

In 1813 Poisson provided a definite integral solution for (14.4). His investigations led to the remark that (14.4) is integrable when  $m$  has the form

$$(14.5) \quad m = \frac{4i}{2i+1}, \quad i \text{ integer} \geq 0$$

[De Morgan 1842c, 703-705; Glaisher 1871]. Finally, Liouville transformed (14.4) into

$$(14.6) \quad \frac{d^2y}{dx^2} = \left(A + \frac{B}{x^2}\right)y, \quad A \neq 0$$

proving that the necessary and sufficient condition for (14.6) to have a solution in finite form is:

$$(14.7) \quad B = i(i+1), \quad i \geq 0$$

[Liouville 1841, 13].

Now, under condition (14.7), equation (14.6) is equivalent to the earth-figure equation in its general form (13.33) or

$$(14.8) \quad \frac{d^2y}{dx^2} + n^2y = \frac{i(i+1)}{x^2}y.$$

Thus, any solution to be obtained in chapters 4 and 5 for the solution of the earth-figure equation, (14.8), can be transformed to a solution of the Riccati equation in either of the forms (14.4) or (14.3) (4).

Another historically important equation for its prominent role in physical astronomy is:

$$(14.9) \quad \frac{d^2y}{dx^2} + ky = f(x), \quad k \text{ constant.}$$

Euler dealt with (14.9) in his memoir on tides in 1739, applying for its solution the method of "variation of constants". He was followed by Daniel Bernoulli in 1741 [Kline 1972, 497-8; Vessiot 1910, 113, fn 166]. The integration of (14.9), as well as of a large family of second-order differential equations, depends upon that of equation

$$(14.10) \quad d^2y/dx^2 + ky = 0.$$

The latter equation is identical with (13.25).

Finally we refer to "Clairaut's equation",

$$(14.11) \quad y = \frac{dy}{dx}x + f\left[\frac{dy}{dx}\right]$$

which by differentiation leads to two factors, one of which gives the singular solution and the other the complete solution. Clairaut integrated (14.11) in 1734, but singular solutions had made their appearance earlier than that [Kline 1972, 476-7]. Clairaut and Euler independently introduced in 1740 and 1734 respectively the conditions under which an equation is a total differential [Kline 1972, 425, 476]. This method led to the theory of integrating factors. Another method widely used at that time was the method of separation of variables established principally by D' Alembert [Dhombres 1986, 132].

The problem of the deduction of a singular solution from a differential equation was first considered in its general form by Laplace in 1772. However, it is with Lagrange in 1776 that we have a reformulation of Euler's concept of the "complete solution" of a differential equation, as well as the interpretation of the singular solution as the envelope of the curves contained in the former one (8).

Lagrange generalised Clairaut's method of differentiation in order to solve the equation

$$(14.12) \quad f(x, y, y') = 0$$

of which (14.11) is a particular case. Applying the method of variation of constants in 1806, Lagrange obtained the singular solution of (14.12) [Fraser 1987, 45-47].

Lagrange contributed much to the theory of partial differential equations applying to them the method of total differentials. He also established the method of variation of constants as a standard method in 1774. Significant contributions in the study of partial differential equations and systems of differential equations were also provided by Monge, Legendre and Laplace in the 1770's (6).

Another important method is the series method, widely used by Newton in late 17<sup>th</sup> century. Assuming the solution of the ordinary differential equation to be represented as an infinite power-series, this method amounts to differentiation and substitution of this form in the given equation and thus to the determination of its coefficients. Euler and Leibniz made use of this method strictly in cases when the solution could not be provided in finite form [Greenberg 1979, 72-73, 152-155; Kline 1972, 379, 488-489].

In late 18<sup>th</sup> century infinite series were widely used as methods of approximation, particularly when problems of interpolation were involved. Lagrange and Laplace regarded power series as an extension of algebraic polynomials and often their convergence was taken for granted. The method of power series for the solution of differential equations was treated by Laplace in a sophisticated way, which gave rise to his method of "generating functions" in <sup>the</sup>early 1780's [see 1.5].

Particularly for the study of periodic physical phenomena, trigonometric series were developed. Euler, D'Alembert, Lagrange and Daniel Bernoulli worked in this area, but, when the latter suggested the representation of the functional solution of the partial differential equation (14.13) in the form of a trigonometric series, the others strongly objected to this approach. In fact, the representation of an arbitrary function in the form of an infinite trigonometric series was to be established only by Fourier in the 1810's [see 1.7].

The equation which gave rise to a long debate between the analysts mentioned above was the vibrating-string equation



$$(14.13) \quad \frac{d^2y}{dt^2} = \frac{d^2y}{ds^2},$$

one of the earliest partial-differential equations which appeared. D'Alembert provided in 1747 its general solution in the form:

$$(14.14) \quad y(t,s) = \psi(t+s) + f(t-s),$$

where  $\psi$  and  $f$  are arbitrary functions<sup>(7)</sup>.

Taking under consideration the initial conditions of the problem, the two arbitrary functions were in fact reduced to one. D'Alembert was consequently led to the functional equation

$$(14.15) \quad y(t,s) = \psi(t+s) - \psi(t-s) = \Lambda(t)\Gamma(s).$$

In 1750 he formulated a differential equation which, solved by the method of separation of variables, gave the forms of the functions  $\Lambda$  and  $\Gamma$ , and thus of the solution  $y(t,s)$  of (14.13), in arbitrary constants [Dhombres 1986, 130-132].

Euler, motivated by problems of geometrical origin, worked in the 1760's on functional equations in one variable. The functional method was also applied by D'Alembert, Lagrange, Monge and Laplace in mechanics in connection with the composition of forces according to the law of the parallelogram during the period 1760-1790 [Dhombres 1986, 133-150].

One of these problems in mechanics was reduced by D'Alembert in 1769 to the functional equation

$$(14.16) \quad 2\varphi(x) = \varphi(x+z) + \varphi(x-z).$$

Once more by differentiation he was led to the integration of the equation

$$(14.17) \quad \varphi''(x) = 0$$

which gives the form of  $\varphi$  as  $ax+b$ ,  $a$ ,  $b$  constants. By means of the same method Laplace tackled another equation concerning the composition of forces

$$(14.18) \quad [\varphi(\theta)]^2 + [\varphi(\pi/2 - \theta)]^2 = 1$$

[Dhombres 1986, 145, 148-150].

Equations of the form (14.16) were also to be considered by Poisson in 1806. D'Alembert's method of differentiation for their solution was used in certain instances in England up to the 1860's<sup>(8)</sup>. With Lagrange we have in 1762 an investigation in functional equations whose treatment was based upon the expansion of  $\varphi(a+ba^mx)$  in Taylor series. Lagrange's method did not make any considerable impact<sup>(9)</sup>.

The first systematic treatment of functional equations was carried by Monge, mainly in his memoirs [1773] and [1776]. In contrast with Lagrange's algebraic approach in the calculus, Monge introduced in the 1770's the language of geometry. Motivated by the current interest in partial differential equations, Monge realized that the intricacies involved in the determination of arbitrary functions in their solution demanded further consideration. The treatment of D'Alembert, Euler and Lagrange, he wrote, was inadequate [Monge 1773, 18].

In the introduction of his first memoir, Monge claimed that his own treatment was based upon the assumption of the perfection of analysis. That is, given  $\psi(x) = y$ , the determination of  $f$ , so that  $x = f(y)$ , is possible [Monge 1773, 19]. In other words, Monge assumed the existence of the inverse function of  $\psi$ , which he implicitly assumed to be unique. But he avoided mentioning anything more on this subject. As we shall notice later it was exactly on a similar assumption that Herschel built his brief theory of functional equations in 1813, motivated by Monge's following problem [2.4].

Let  $V$ ,  $\Delta$  and  $\psi$  be known functions; it is required to determine  $\phi$  so that if  $y = \Delta(x)$  is substituted in

$$(14.19) \quad z = \phi[V(x, y)],$$

then  $z = \psi(x)$ . The problem is thus reduced to the solution of the functional equation

$$(14.20) \quad \psi(x) = \phi[V'(x, \Delta(x))],$$

where  $V'$  equals  $V$  after the substitution of  $y$ . Monge let

$$(14.21) \quad V'(x, \Delta(x)) = u$$

from which according to his introductory statement,  $f$  can be found so that  $x = f(u)$ . Substituting now  $x$  in (14.20), equation  $\phi(f(u)) = \psi(f(u))$  is obtained from which it follows that  $z = \psi(f(u))$  [Monge 1773, 19-20].

As an example let  $z = \phi(x^2 + y^2)$  so that  $z = x^2/a$  when  $y = mx$ , where  $e$ ,  $m$  are constants. Equation (14.20) gives  $x^2/a = \phi(x^2 + m^2x^2)$ . Letting  $x^2 + m^2x^2 = u$ ,  $x^2$  can be determined in respect to  $u$  and, if substituted in the above equation, the form of  $z$  is readily found to be  $z = (x^2 + y^2)/[(m^2 + 1)a]$  [1773, 20-21].

Monge claimed next that he could provide a geometrical construction of problem (14.20) in the cases that the functions  $\Delta$  and  $\psi$  are continuous or discontinuous. At that time a function was regarded as "continuous" if it was specified by a single algebraic expres-

sion over its range of definition. Another mode of solution for problem (14.20) followed as a consequence of geometrical consideration [1773, 21-23].

Monge's geometrical considerations in connection with functional equations were not followed by Lagrange and Laplace and had no impact on English analysts. For this reason we have omitted any reference to them. However, the very first process upon which problem (14.20) was tackled is algebraic in character. We now proceed to mention a functional equation which appeared in his [1776]. In this memoir Monge's treatment is more general than before, but involves throughout geometrical constructions. The main reason why we mention this memoir, is that he reduces functional equations to finite difference ones, a method which were to become very popular in England.

Let  $\varphi$  and  $\psi$  be determined by

$$(14.22) \quad z = \varphi(u) + \psi(v),$$

under the conditions

$$(14.23) \quad z = f(x), \text{ when } y = F(x) \text{ and}$$

$$(14.24) \quad z = f_1(x), \text{ when } y = F_1(x),$$

where  $u, v$  known functions of  $x$  and  $y$ , and  $F, F_1, f, f_1$ , also known functions of  $x$ . Substituting  $y$  in (14.22), according to the conditions (14.23-24), Monge eliminated  $\psi$ . Via geometrical considerations, he was led finally to a first-order finite difference equation in  $\varphi$  which could easily be integrated [1776, 306-308]

At that time Laplace was working on finite difference equations, transforming variable differences into constant ones. Monge, realising that more complicated equations led to "un nouveau genre de calcul integral", referred to Laplace's memoir, [1776a], as a complement to his own treatment of finite difference equations [Monge 1776, 308-311].

In a third memoir, [1780], Monge focused on the determination of the complete solution of finite difference equations. By exponential transformations he was led to functional equations. Once more he was based upon complicated geometrical considerations. One of the problems he tackled has as follows: "Intégrer complètement l'équation  $\Delta y + Ay + B = 0$  [ $A, B$  constants], dans le cas ou la différence finie de la variable principale seroit 1<sup>o</sup> constante= $a$ , 2<sup>o</sup>= $a+bx$ ; 3<sup>o</sup>= $ax^n-x$ ". The transformation upon which the solution was based was  $Ay+B=e^u$  [1780, 357-60] (22).

Motivated by problems suggested by Monge in 1772, Laplace dealt with functional equations in the course of his memoir on finite difference equations which were involved in problems of probability [1776a]. The general problem solved was the determination of  $f$  such that

$$(14.25) \quad f[\varphi(x)] = H_x f[\psi(x)] + X_x,$$

where  $\varphi$ ,  $\psi$ ,  $H_x$  and  $X_x$  are all given functions of  $x$ .

Laplace let a new function  $u_z$  be determined as follows

$$(14.26) \quad u_z = \psi(x), \quad u_{z+1} = \varphi(x).$$

Solving the first of (14.26) for  $x$  we have

$$(14.27) \quad x = \Gamma(u_z) \text{ and } \varphi(x) = u_{z+1} = \Pi(u_z),$$

where  $\Gamma$  and  $\Pi$  known functions of  $u_z$ . The second equation in (14.27) is a finite difference one in  $u_z$  with  $\Delta z = 1$ . Its integration gives  $u_z$  as a function of  $z$ . Moreover, the first equation in (14.27) will give  $x$  as a function of  $z$ .

Calling  $L_z$  and  $Z_z$  the functions  $H_x$  and  $X_x$  respectively after the substitution of  $x$  as a function in  $z$ , the functional equation (14.25) obtains the form

$$(14.28) \quad f(u_{z+1}) = L_z f(u_z) + Z_z.$$

Further, denoting  $f(u_z)$  by  $y_z$ , (14.28) becomes

$$(14.29) \quad y_{z+1} = L_z y_z + Z_z.$$

In other words, the functional equation (14.25) is reduced to a finite difference equation in  $y_z$  [1776a, 103-104]. Laplace further observed that (14.29) can be solved easily according to the method provided earlier in that memoir [1776a, 74-75]. Laplace illustrated the method given above for problem (14.25) with solving the equations  $f(x^q) = f(mx) + p$  and  $[f(x)]^2 = f(2x) + 2$ , where  $q, m, p$  are constants [1776a, 104-108].

Instead of applying Laplace's method for the solution of these two equations I choose equation (14.18) for its prominent role in both mechanics and the development of the study of functional equations. Equation (14.18) written in  $x$  becomes

$$(14.30) \quad [\varphi(x)]^2 + [\varphi(\pi/2 - x)]^2 = 1.$$

It was nearly formulated as such by Laplace in his [1799, Book 1, art 1]. Though his method, as in his [1776a] was applicable to (14.30), Laplace followed, as we mentioned above, D'Alembert's method. In what follows we see Boole's application of Laplace's basic method for the solution of equation (14.30).

Let

$$(14.31) \quad [\varphi(x)]^2 = \psi(x), \quad x = u_z \text{ and } \pi/2 - x = u_{z+1}.$$

Further denoting

$$(14.32) \quad \psi(x) = v_t \text{ and } \psi(n/2-x) = v_{t+1},$$

the problem (14.30) is reduced to the system

$$(14.33) \quad u_{t+1} + u_t = n/2 \text{ and } v_{t+1} + v_t = 1.$$

The solutions of equations (14.33) are respectively

$$u_t = c_1(-1)^t + n/4 \quad \text{and} \quad v_t = c_2(-1)^t + 1/2.$$

By rearranging their terms and by division we find

$$v_t = 1/2 + C(u_t - n/4) \quad \text{or} \quad \psi(x) = [1/2 + C(x - n/4)] \quad (\text{see (14.31), (14.32)}).$$

Therefore, according to (14.31) we have

$\varphi(x) = [1/2 + C(x - n/4)]^{1/2}$ , in which  $C$  must be interpreted as a function of  $x$  which does not change if  $x$  becomes  $n/2 - x$  [Boole 1860, 225-227].

Laplace's method, as introduced through the solution of (14.25), was adopted by Herschel in the 1810's without any modification and remained a standard method up to the 1860's. The only contribution provided was a generalization of it<sup>(10)</sup>. However, the method that motivated Babbage to contribute in an original manner in the development of functional equations, was that by Monge illustrated above by (14.20) [chapter 2]. The work of Babbage and Herschel was further explored by De Morgan in his [1836] [chapter 3].

Monge's work is largely ignored today. It is particularly strange that Dhombres's extended historical survey on functional equations [1986] omits any reproduction of either Monge's or Laplace's treatment of functional equations<sup>(11)</sup>. The reason we discussed Monge's work amply was on one hand to rescue it from oblivion, and on the other to show its impact on Laplace and later on Herschel, Babbage and De Morgan. To conclude, I note that the most extended and useful historical account that I have found on Lagrange's, Monge's and Laplace's treatment of functional equations, was provided in De Morgan's article [1836, art 246-251].

## 1.5 Lagrange's theorem and the genesis of a new calculus: 1774-1800.

Lagrange's theorem,

$$(15.1) \quad \Delta u = e^{h \frac{du}{dx}} - 1,$$

where  $\Delta u$  is the increment of  $u(x)$  when  $x$  is replaced by  $x+h$ , is but a symbolical expression of Taylor's theorem of expansion. The formulation of (15.1) in 1774 involved combinatorial analysis and symbolical arguments. Besides the novelty of the symbolical notation, we notice also Lagrange's tendency<sup>n</sup> for generalization when he inferred from (15.1) that

(15.2)  $\Delta^n u = (e^{\frac{h}{dx} \frac{du}{dx}} - 1)^n$ ,  
 $n$  any integer, holds also true. This inference lacked, according to Lagrange, a direct demonstration. However, since (15.2) led to known formulae of expansion, he did not bother to further justify its validity [1774, 195].

The analogy between indices of exponentiation and those of differentiation or integration had been perceived by Leibniz in 1695 [Koppelman 1971, 158]. Through (15.2) this analogy was formally established. One of the results of Lagrange's theorem was the possibility of transform<sup>ing</sup> finite difference equations into differential ones. At the early stage, however, (15.2) and the respective formulae deduced from it for different values of  $n$ , were useful mainly in problems of interpolation.

In<sup>the</sup> late 1770's Laplace improved considerably on certain aspects of Lagrange's "nouveau calcul". Avoiding Lagrange's symbolic arguments, Laplace provided a direct, analytical proof of (15.2) in 1776. This proof was further simplified in 1780. In the course of these demonstrations Laplace was led to the discovery of a new analytical tool for demonstration, his generating functions. This method, introduced in 1782, was to be widely used by him for the integration of finite difference and differential equations. However, the third demonstration of (15.2), which he presented in his [1782] via his generating functions, lacked elegance as it involved the passage from finite to infinite.

Due to the link provided by (15.1) between finite and infinitely small differences, Lagrange's calculus offered a new angle for view<sup>ing</sup> the foundations of analysis. That is, one could regard the theory of finite differences as the fundamental one and from it deduce theorems of the differential calculus<sup>(1)</sup>. An instance of such a systematic arrangement was Laplace's method of generating functions. Another instance was Arbogast's calculus of

derivations incorporated in his [1800]. In the course of this calculus, by separating the symbols of operation from those of quantity, Arbogast gave explicit emphasis on operational symbols.

Laplace claimed in 1811 that his calculus of generating functions encompassed Arbogast's method of separation of symbols [1.7]. It is true indeed that these two methods largely led to the same results and in this sense they are equivalent. However, Arbogast's notation and principles rendered his method more direct than that of Laplace, and easier in applications. With Arbogast, (15.2) can be written as

$$(15.3) \quad \Delta^n u = (e^{h \frac{d}{dx}} - 1)^n u,$$

and (15.3) can be further generalized into

$$(15.4) \quad F(1+\Delta)u = F(e^{h \frac{d}{dx}})u,$$

for any algebraic or transcendental function  $F$  which applies only on the operations.

Whereas Laplace's method led to the development of the analytical aspect of the calculus of operations -including happy applications in combinatorial problems of probability and in the determination of definite integrals- Arbogast's method contributed in the development of its algebraic aspect. Both methods were admired by early-19th-century English analysts, but it was the latter that determined <sup>and</sup> the development of symbolical methods in the 1810's <sup>and</sup> 1840's.

In what follows we will provide first Lagrange's own demonstration of (15.1). Next we present Laplace's analytical procedures that led him to (15.2). Finally we will refer briefly to Arbogast [1800] -focusing on his technique of separation of symbols. In the course of this study we will notice remarkable differences between the procedures of Lagrange and Laplace. This section forms the main background in French operational calculus for our study of Herschel's work [2.3].

In his memoir "Sur une nouvelle espèce de calcul" [1774], Lagrange obtained first Taylor's theorem of expansion in the following way: let  $u$  be a function of  $x$ . If  $x$  is replaced by  $x+\xi$ , then by the known theory of series,  $u$  will obtain the form

$$(15.5) \quad u + \rho \xi + \rho' \xi^2 + \rho'' \xi^3 + \dots$$

where  $p, p', p'', \dots$  new functions of  $x$  "dérivées d'une certaine manière de la fonction  $u$ " [1774, 186]. If in (15.5) we replace  $x$  by  $x+w$ , then each of the functions  $u, p$  etc will receive a development similar to (15.5). Let the expression representing these developments be called  $S_1$ . If  $x$  is replaced from the very beginning in  $u$  by  $x+\xi+w$ , formula (15.5) will be as above, the only difference being that instead of  $\xi^n$  we will have  $(w+\xi)^n$ .

Let now all these binomials be expanded and call the final sum  $S_2$ . Since these developments must be the same, by comparison of the terms of  $\xi^n$  in  $S_1$  and  $S_2$  it follows that  $p=u', p'=u''/2, p''=u'''/2.3$  etc. [1774, 189]. The final step is to regard  $\xi$  in (15.5) as infinitely small. As a result  $\xi^2=\xi^3=\dots=0$ . The second term of (15.3),  $u'\xi$ , has to be the increment of  $u$ ,  $du$ . Hence, since  $\xi$  is the increment of  $x$ , we have  $du=u'dx$ . Inductively it follows that  $u'=du/dx, u''=d^2u/dx^2$  etc, or, Taylor's theorem

$$(15.6) \quad u(x+\xi) = u + \xi \frac{du}{dx} + \frac{1}{2!} \xi^2 \frac{d^2u}{dx^2} + \dots$$

in modern notation [1774, 191] (3).

Lagrange's reasoning, which leads from (15.6) to (15.1), consists of the following observations. Suppose we have  $u=u(x,y,z,\dots)$  and  $x,y,z,\dots$  are replaced by  $x+\xi, y+\psi, z+\zeta, \dots$  respectively. Then, the increment of  $u$ ,  $\Delta u = u(x+\xi+\dots) - u$ , will be given accordingly from (15.6). In fact, the general term in the right-hand side of the development (15.6) will be of the form

$$(15.7) \quad \frac{\xi^\mu \psi^\nu \zeta^n \dots}{\mu! \nu! n! \dots} \cdot \frac{d^{\mu+\nu+n+\dots} u}{dx^\mu dy^\nu dz^n \dots}, \quad [1774, 192].$$

Lagrange observes then that  $\frac{1}{\mu! \nu! n! \dots}$  equals  $\frac{M}{(\mu+\nu+n+\dots)!}$ ,

where  $M$  is the coefficient of  $x^\mu y^\nu z^n \dots$  in the expansion of  $(x+y+z+\dots)^{\mu+\nu+n+\dots}$ . [Indeed, take  $(x+y)^{\mu+\nu}$  then

$$M = \binom{\mu+\nu}{\nu}, \text{ hence } \frac{1}{\mu! \nu!} = \binom{\mu+\nu}{\nu} \frac{1}{(\mu+\nu)!}]. \text{ According to this observa-}$$

tion, the increment  $\Delta u$ , given primarily as a series with (15.7) as its general term, can also be obtained by the series with general term

$$(15.8) \quad \frac{(x+y+z+\dots)^n}{n!}$$



under certain symbolical transformations. That is, after the development of each nominator, we have to change  $x$  into  $\xi/dx$ ,  $y$  into  $\psi/dy$  etc. and then multiply each term by  $d^\lambda u$ ,  $\lambda$  being the sum of the exponentation indices of  $x, y, z, \dots$  in that term.

Now, the series with general term (15.8) can be written as  $e^{x+y+z+\dots} - 1 = e^x e^y \dots - 1 = (1+x/1+x^2/2 + \dots)(1+y/1+y^2/2 + \dots) \dots - 1$ . So  $\Delta u$  is obtained if after the multiplications we change the terms in the way suggested above. Finally, he considers the expression [1774, 193-194]

$$(15.9) \quad e^{\frac{du}{dx}\xi + \frac{du}{dy}\psi + \dots} - 1.$$

Developing (15.9) according to the powers of  $du$ , it only remains to change  $(du)^\lambda$  into  $d^\lambda u$  and the result will be according to the observations stated above,  $\Delta u$ . Hence, we arrive at Lagrange's theorem

$$(15.10) \quad \Delta u = e^{\frac{du}{dx}\xi + \frac{du}{dy}\psi + \dots} - 1,$$

[1774, 193-194].

Thus, (15.10) was obtained by means of Taylor's theorem, combinatorial observations and symbolical arguments. The latter amounts to switching from exponentation to differentiation indices and as a result Taylor's theorem is cast in symbolic form. Lagrange did not express any doubt whatsoever about the demonstration of (15.10). What he felt to be not so clear and rigorous was his step from (15.10) -taken in one variable- to the general theorem

$$(15.11) \quad \Delta^\lambda u = (e^{\frac{du}{dx}\xi} - 1)^\lambda,$$

where  $\lambda$  can be a positive or negative integer. In the latter case  $\Delta^{-1}$  is replaced by  $\Sigma$ ,  $d^{-1}$  by  $\int$  and so on. Lagrange was afraid that a "directe et analytique" demonstration of (15.11) would be very difficult. But, since *a posteriori* one could be reassured that (15.11) led to right results, he did not bother to investigate this matter any further [1774, 194-195] (4).

In the case that  $\lambda > 0$ , the right-hand side of (15.11) could be obtained directly according to Taylor's theorem. Expanding  $(e^w - 1)^\lambda$ , taking the logarithms of both sides and differentiating, he arrived at the formula

$$(15.12) \quad \Delta^\lambda u = \frac{d^\lambda u}{dx^\lambda} \xi^\lambda + A \frac{d^{\lambda+1} u}{dx^{\lambda+1}} \xi^{\lambda+1} + \dots,$$

where  $A, B, \dots$  are determined in terms of  $\lambda$ . It is important to note that he did not bother to determine the coefficient of  $\xi^{\lambda+n}$  in terms of  $n$ , but only provided the first 4 coefficients of (15.12) by means of recursive relations [1774, 195-197]. One of Laplace's improvements would be to find the formula that gives the general term of such expansions.

In the case of negative indices, Lagrange cast first (15.11) in the form

$$(15.13) \quad \left( \frac{du}{dx} \xi \right)^\lambda = [\log(1+\Delta u)]^\lambda,$$

where the development of the right-hand side of (15.13) was determined on lines similar to those that had led to (15.12). Replacing  $d^\lambda$  by  $\int^\lambda$ ,  $\Delta^\lambda$  by  $\Sigma^\lambda$ , in the case that  $\lambda$  was negative, he formulated various expansions. The special case for  $\lambda = -1$  was considered leading to a formula important for the calculation of areas of curves. He observed that Cotes, Stirling and others had given respective formulae only for a finite number of coordinates. Now, Lagrange provided a general formula whose coefficients obey a certain law [1774, 201-202]. But once more the direct determination of the coefficient of the  $n^{\text{th}}$  term was missing ('5').

Laplace undertook in his [1776b] to prove directly (15.11) for arbitrary  $\lambda$ . In fact, he was to arrive first at (15.12) and deduce from it (15.11) by letting  $u$  have a specific value. Laplace's procedure is more general and orthodox in its principles than that of Lagrange's. In fact, the main difference is that it is far less algebraic and more analytical. Induction is still a necessity though, and the steps omitted render the proof rather more obscure than that given by Lagrange. Finally, his technique does not rely upon the interpretation of  $(du/dx)^\lambda$  as  $d^\lambda u/dx^\lambda$ , an analogy necessary only for the final step from (15.12) to (15.11).

Let  $u_1$  equal  $u$  when  $x$  is replaced by  $x+a$ ,  $u=u(x)$  as above. Then  $u_1=u(x+a)=u+s$ . By differentiation relative to  $a$  we have

$$\left[ \frac{\partial u_1}{\partial a} \right] = \left[ \frac{\partial u_1}{\partial x} \right] = \left[ \frac{\partial u}{\partial a} + \frac{\partial s}{\partial a} \right] = \frac{\partial s}{\partial a},$$

hence  $s$  can be determined, and accordingly we have

$$(15.14) \quad u_1 = u + \int da \frac{\partial u_1}{\partial x}$$

[1776b, 316; my own insertions in square brackets] <sup>(6)</sup>.

From (15.14) "we see" that

$$(15.15) \quad \frac{\partial u_1}{\partial x} = \frac{du}{dx} + \int da \frac{\partial^2 u_1}{\partial x^2}$$

In other words, we differentiated (15.14) relative to  $x$ . Such clarifications, as well as the next steps, are omitted by Laplace. Presumably these are to integrate (15.15) relative to  $a$ , giving

$$(15.16) \quad \int \frac{\partial u_1}{\partial x} da = \int \frac{du}{dx} da + \int \left[ \int da \frac{\partial^2 u_1}{\partial x^2} \right] da,$$

since  $u$  is independent from  $a$ , and according to (15.14), formula (15.16) gives

$$(15.17) \quad u_1 = u + a \frac{du}{dx} + \int \left[ \int da \frac{\partial^2 u_1}{\partial x^2} \right] da.$$

So, by repeated differentiation relative to  $x$  and integration relative to  $a$ , we can arrive at the formula given by Laplace:

$$(15.18) \quad u_1 = u + a \frac{du}{dx} + ha^2 \frac{d^2 u}{dx^2} + h'a^3 \frac{d^3 u}{dx^3} + \dots$$

where  $h, h', \dots$  are independent of  $a$  [1776b, 316].

What Laplace proved thus was Taylor's theorem (15.18). However, he neither said so, nor did he bother to determine  $h, h', \dots$ . His aim being to arrive directly at (15.12), he reasoned as follows: since (15.18) provides us with a formula for  $\Delta u = u_1 - u$ , the same formula can be used to determine  $\Delta u_1$  if  $u$  is replaced by  $u_1$  in the right-hand side. We thus have

$$(15.19) \quad \Delta^2 u = \Delta u_1 - \Delta u = a \left( \frac{\partial u_1}{\partial x} + \frac{du}{dx} \right) + \dots$$

and by induction and the same process followed as with (15.14)–(15.18), we will have formula (15.12), or,

$$(15.20) \quad \Delta^n u = a^n \frac{d^n u}{dx^n} + qa^{n+1} \frac{d^{n+1} u}{dx^{n+1}} + \dots$$

[1776b, 317].

Laplace's procedure is far more general than that of Lagrange's. In none of the formulae (15.18), (15.19) or (15.20) did he try to determine the constant coefficients. For him importance lay in the fact that  $q$ ,  $q'$  etc in (15.20) are independent of  $u$ . Thus any function can be substituted for  $u$  without limitation of the generality of the result. Letting  $u=e^x$  in (15.20) we are led to  $\Delta^n u = e^x(e^a-1)^n$ . Expanding the factor  $(e^a-1)^n$  by (15.20) in powers of  $a$ , we will have finally

$$(15.21) \quad \Delta^n u = (e^{a \frac{du}{dx}} - 1)^n,$$

under the condition that, after the development,  $(du/dx)^n$  is replaced by  $(d^n u/dx^n)$ . For negative indices we have accordingly

$$(15.22) \quad \Sigma^n u = (e^{a \frac{du}{dx}} - 1)^{-n}, \quad [1776b, 318].$$

Laplace's second proof of Lagrange's theorem in his [1780] is a slight improvement upon his first. Partial differentiation is still prominent but the switch from it to integration is absent this time. It is very probable that in the course of this demonstration he perceived the germ of his concept of "generating function" which appeared in 1782(?) .

Let  $u=u(a)$  developed in powers of  $a$ :

$$(15.23) \quad u=v + aq_1 + a^2q_2 + \dots + a^nq_n + \dots$$

where  $u=v$  when  $a=0$ . By differentiation relative to  $a$  and by consequent replacement of  $a$  by 0, he determined  $q_n$  by

$$(15.24) \quad q_n = \frac{(\partial^n u / \partial a^n)}{n!}.$$

Hence, Taylor's theorem was produced in a manner far more direct than in his earlier proof or in Lagrange's proof [1780, 313-315].

Setting now  $u=u(t)$ ,  $u_1=u(t+a)$ , Taylor's theorem readily gives (15.18) where now we have  $t$  instead of  $x$ , and  $1/2!$ ,  $1/3!$ , ... instead of  $h$ ,  $h'$  etc. Formula (15.18) gives

$$(15.25) \quad \Delta u = a(du/dt) + a^2/2!(d^2u/dt^2) + \dots$$

Taking the second finite difference we have

$$(15.26) \quad \Delta^2 u = a\Delta(du/dt) + a^2/2!\Delta(d^2u/dt^2) + \dots$$

Substituting successively  $du/dt$ ,  $d^2u/dt^2$ , ... for  $u$  in (15.25) we obtain  $\Delta(du/dt)$ ,  $\Delta(d^2u/dt^2)$  etc. and replacing these values in (15.26) we obtain  $\Delta^2 u$ . Inductively we arrive once more at (15.20) under a slightly different procedure [1780, 316-18]. The

final step is exactly as in his [1776b]. The function  $u$  is replaced by  $e^u$  and theorems (15.21)–(15.22) readily follow [1780, 320].

Laplace proceeds next to determine the expansions of (15.21)–(15.22) for specific values of  $n$ . Of most importance is his deduction of the development of (15.22) for  $n=1$ . Laplace's procedure for  $n=1$  was long and complicated and not to be followed by the English. We omit its reproduction but only mention that his ingenuity lay in the following technique. Instead of developing  $(e^{a(du/dt)} - 1)^{-1}$ , he took  $a(e^a - 1)^{-1}$  noticing that

$$(15.27) \quad a(e^a - 1)^{-1} = a/2(e^{a/2} - 1)^{-1} - a/2(e^{a/2} + 1)^{-1}.$$

By means of (15.27), Laplace proceeded by his usual method of differentiation arriving at

$$(15.28) \quad \Sigma u = \frac{\int u dt}{a} - \frac{1}{2} u + ah_1 \frac{du}{dt} - a^2 h_2 \frac{d^2 u}{dt^2} + a^3 h_3 \frac{d^3 u}{dt^3} + \dots$$

where the value of  $h_1$  was given now by a general formula in his [1780, 320–325] (9).

Laplace acknowledged Lagrange for his "excellent memoir" [1774], claiming that his own demonstration was as simple and as direct as one would desire. He also asserted that by means of it one had the advantage of noticing a priori the reason for the existence of the "analogie singulière" between indices of exponentiation and finite differences [1780, 327].

The third and last demonstration of Lagrange's theorem by Laplace was based on his peculiar method of generating functions. This method first appeared in his memoir "Sur les suites" [1782]. In the introduction he wrote that this method was useful in the interpolation of series as well as in the integration of linear finite difference equations. He added that his respective researches led him to the application of definite integrals particularly to partial differential equations which did not admit so far of known methods of integration [1782, 207–209]. A more thorough study of generating functions was presented in his book on the theory of probability [1812] (9).

The basic theory of the generating functions runs as follows: let  $y_x$  be a function of  $x$ . Let next the infinite series

$$(15.29) \quad y_0 + y_1 t + \dots + y_x t^x + \dots + y_\infty t^\infty.$$

Then we can always conceive a function  $u(t)$  whose development equals (15.29). We define thus  $u(t)$  as the generating function of  $y_x$ . For convenience we will write

$$(15.30) \quad u = Gy_x,$$

where symbol  $G$  stands for the "generating function" (10). From the definition of  $G$ , by means of (15.29)–(15.30), it follows easily that

$$(15.31) \quad u/t^r = Gy_{x+r} \quad \text{and} \quad (15.32) \quad u(1/t - 1) = G\Delta y_x$$

[1782, 211–212; OC, 7–8; see (9) above].

Laplace let the expression  $\alpha y_x + \dots + q y_{x+n}$  be denoted by  $\nabla y_x$ . By the definition of  $G$  it follows easily that

$$(15.33) \quad u(a + b/t + c/t^2 + \dots + q/t^n) = G\nabla y_x.$$

Denoting further  $\nabla^2 y_x$  the expression  $\nabla y_x$  where  $y_x$  is replaced by  $\nabla y_x$ , he inferred inductively from (15.33) and (15.32) that:

$$(15.34) \quad u/t^r (a + b/t + c/t^2 + \dots + q/t^n) (1/t - 1)^i = G\Delta^i \nabla^r y_{x+r}$$

[1782, 212–13; OC, 8].

The most common technique that Laplace used in the course of his calculus of generating functions was to provide identities in terms of them and then switch from the generating functions to their respective coefficients. For example, casting  $u/t^1$  into  $u(1+1/t-1)^1$  we have

$$(15.35) \quad u/t^1 = u[1 + i(1/t - 1) + i(i-1)/2(1/t-1)^2 + \dots].$$

We notice the following

$$(15.36) \quad \begin{aligned} u/t^1 &= Gy_{x+1} \\ u &= Gy_x \\ u(1/t - 1) &= G\Delta y_x \\ u(1/t - 1)^2 &= G\Delta^2 y_x \\ &\dots \end{aligned}$$

according to (15.31), (15.32) and (15.34). Due to (15.36), formula (15.35) is an identity between generating functions. Laplace, switching to the coefficients  $y_{x+1}$ ,  $\Delta y_x$  given by (15.36), is led accordingly to the interpolation formula

$$(15.37) \quad y_{x+1} = y_x + i\Delta y_x + i(i-1)/2!\Delta^2 y_x + \dots$$

[1782, 216; OC, 11–12; see also (10) above]. Formula (15.37) has another interesting application. Assume the equation

$$(15.38) \quad \Delta^n y_x = 0.$$

Then (15.37) terminates; and setting  $x=0$  it provides us with the complete integral of (15.38) in  $n$  arbitrary constants [OC, 12; see (9) above].

In order to deduce Lagrange's theorem, Laplace cast  $u(1/t^i - 1)^n$  in the form

$$(15.39) \quad u\left[\left(1 + \frac{1}{t} - 1\right)^i - 1\right]^n.$$

Now, the coefficient of  $t^x$  in  $u(1/t^i - 1)^n$  is the  $n^{\text{th}}$  difference of  $y_x$  when  $x$  is increased by  $i$ . Denoting this difference by  ${}^i\Delta^n y_x$ , or

$$(15.40) \quad {}^i\Delta^n y_x = y_{x+i} - y_x,$$

we have

$$(15.41) \quad u\left(\frac{1}{t^i} - 1\right)^n = G {}^i\Delta^n y_x.$$

Accordingly

$$(15.42) \quad u\left(\frac{1}{t} - 1\right)^n = G \Delta^n y_x.$$

Taking under consideration (15.39)-(15.42), and by the same reasoning illustrated above for (15.37), we have the identity

$$(15.43) \quad {}^i\Delta^n y_x = [(1 + \Delta y_x)^i - 1]^n$$

[1782, 245; 1812, 39]. The final step demands the passage from  $\Delta y_x$  to the differential  $dy_x/dx$ . Laplace's procedure, non-rigorous for post-Cauchy standards, is typical of that period. In brief it has as follows: Let  $\Delta y_x = dy_x$ . If  $i$  is infinitely large, then  $idy_x = a$ , "a" finite quantity. Then  $\log(1 + dy_x)^i = i \log(1 + dy_x) = idy_x = idx(dy_x/dx) = a(dy_x/dx)$ . According to this result, (15.43) becomes

$$(15.44) \quad {}^i\Delta^n y_x = (e^{a dy_x/dx} - 1)^n, \quad n > 0,$$

with a respective formula following for  $n < 0$  [1782, 247; 1812, 39-41].

Laplace widely applied his method of generating functions for the solution of finite difference and differential equations. We will see another instance of this application in [1.7]. See additionally [1805, Book 9, art 5; Book 10, art 5 and Goldstine 1977, 204-5]. Laplace [1782] was of considerable influence on Brisson [see 1.6]. In England Laplace's generating functions were applied to a diversity of problems such as <sup>the</sup> solution of differential equations, problems of probability, definite integrals, as well as a means for demonstration of the basic theorems of the calculus of operations, but not any significant development of its theory was to be noticed<sup>(11)</sup>.

We now proceed to a brief review of Arbogast [1800]. The main idea in Arbogast's calculus of derivations was to find algorithms for converting algebraic expressions into series of the form  $\sum a_r x^r$  and consequently to determine the coefficient of  $x^n$  in the expansion of  $\Phi(a+bx+cx^2+\dots)$  and in other similar expansions. In this sense Arbogast's work was highly combinatorial and very laborious and it was followed by few English analysts.

What Arbogast called "derivation" was the operation  $D$  by which the coefficients of  $F(\alpha+x)$  were deduced from  $F(\alpha)$ . Let  $F(\alpha+x)$  be expanded as follows:

$$(15.45) \quad F(\alpha+x) = a + bx + c/2!x^2 + \dots$$

Then  $a=F(\alpha)$ ,  $b=DF(\alpha)$ ,  $c=DxD^2F(\alpha)=D^2F(\alpha)$  etc [ $x$  is the symbol of multiplication] and (15.45) is cast accordingly into

$$(15.46) \quad F(\alpha+x) = D^0F(\alpha) + D^1F(\alpha)x + \dots, [1800, 2-3].$$

If  $D$  is taken to be differentiation, then (15.46) is reduced to Taylor's series [1800, 305-324]. As he mentioned in the introduction of his book, Arbogast's aim was to create a calculus which would include the differential calculus as a particular instance [1800, i]. He was apparently the first to discuss at length the advantages of algebraic symbolical notation. Brevity and efficiency in operations was taken under consideration. Introducing new symbols he wrote: "Le secret de la puissance de l'Analyse se consiste dans le choix et l'emploi heureux de signes simples et caractéristiques de chose qu'ils doivent représenter" [1800, ii].

A key doctrine of Arbogast's symbolical procedures is that of the equivalent operators, achieved more clearly than by Lagrange or Laplace—through his method "de séparation des échelles d'opérations". In other words, he separated the symbols of operation from those of quantity giving emphasis to the former symbols. An early example of equivalent operations in his book is the following:

$$(15.47) \quad d = d \cdot 1 + d^1 \cdot,$$

where  $d$  the operation of total differential of  $\varphi(x,y)$ ,  $d \cdot 1$  the same operation applied to  $y$  and  $d^1 \cdot$  respectively to  $x$ . Thus, one can write

$$(15.48) \quad d(xy) = (d \cdot 1 + d^1 \cdot)xy = d \cdot 1(xy) + d^1 \cdot(xy) = xdy + ydx$$

It follows in general that

$$(15.49) \quad d^n = (d \cdot 1 + d^1 \cdot)^n \quad [1800, 319].$$



Arbogast applied his theory of equivalent operations for the development of partial differential equations in respect to one of the variables. His procedure was far too laborious and apparently unsuccessful. Equally long and complicated was his demonstration of Lagrange's theorem by means of his calculus of derivations. However, Arbogast suggested that great simplicity can be achieved by writing  $du/dx$  as  $\delta u$  and by applying his method of detachment. Thus, one avoids the necessity to change the indices of exponentiation into those of differentiation after the development of (15.1). He consequently wrote

$$(15.50) \quad (1+\Delta)u = (1 + \xi\delta + 1/2!\xi^2\delta^2 + \dots)xu$$

where  $u=u(x)$ ,  $\xi=\Delta x$ ,  $\delta u=du/dx$  and  $x$  -in the right-hand side of (15.50)- the symbol for multiplication (not to be confounded with the variable  $x$ ).

Formula (15.50) is cast next into the form  $(1+\Delta)u=e^{\xi\delta}xu$ , and Lagrange's theorem is obtained as

$$(15.51) \quad \Delta^n u = (e^{\xi\delta}-1)^n xu.$$

By separation of symbols he obtained the operational equations

$$(15.52) \quad \Delta = e^{\xi\delta}-1 \text{ or } 1+\Delta = e^{\xi\delta}.$$

By generalization he inferred from (15.52)

$$(15.53) \quad F(1+\Delta)xu = F(e^{\xi\delta})xu,$$

for any function  $F$  which applies only to the operations [1800, 350]. Another innovation was his introduction of the operator  $E$  defined by

$$(15.54) \quad E\phi(x) = \phi(x + \Delta x).$$

By separation of symbols he deduced the equivalent operators

$$(15.55) \quad E = 1+\Delta \text{ and } E^n = (1+\Delta)^n$$

which, applied to any function  $u$ , provide us with another formula useful in interpolation of series:

$$(15.56) \quad E^n u = (1 + n\Delta + n(n-1)/2!\Delta^2 + \dots)xu \quad [1800, 375-377] (12).$$

We will now proceed to a study of the development of Lagrange's new calculus in France during the period 1808-1822.

1.6 B. Brisson, J.F. and F.J. Français, F.Servois and F.Sarrus on aspects of the calculus of operations: 1808-1822.

Brisson is acknowledged as the first to interpret a differential equation as an operator on a function and to proceed to its solution by symbolical techniques. The only work that was

published by him on this subject is a memoir on partial differential equations [1808]<sup>(1)</sup>. He wrote three more papers on 1821, 1823 and 1827 respectively, which are lost and the only account of them that survives is provided by Cauchy [see 1.7].

Focusing only on his [1808], we notice a combination of algebraic manipulations of series à la Lagrange and Arbogast, together with analytical techniques à la Laplace. The main motivation for this memoir was Laplace's application of definite integrals for the solution of partial differential equations. The latter's memoir [1782], is the only reference provided by Brisson in his [1808, 191-2, 250, 259].

Brisson's principal aim in this memoir is to provide methods for the solution of partial differential equations in form of series which can be easily reduced consequently in definite integral form. Among his considerations, the case of equations with constant coefficients deserves our attention. If by changing the differential indices in exponents, the equation is decomposed in factors of the first degree, then its solution can follow easily. When factorization fails, then the solution can always be provided in the form of a definite integral [1808, 253-4].

Let  $V$  be an "analytical expression" and  $\psi$ ,  $n$  arbitrary functions in  $u$ . Then the model of Brisson's general formula for expansion in series was the following:

$$(16.1) \quad V(\psi, n) = V(\psi) \cdot n + \Gamma \left[ \frac{V(\psi)}{u} \right] \frac{dn}{du} + \frac{\Gamma^2}{1.2} \left[ \frac{V(\psi)}{u^2} \right] \frac{d^2n}{du^2} + \dots$$

in which  $\Gamma$  is defined by

$$(16.2) \quad \Gamma \left[ \frac{V(\psi(u))}{u} \right] = V(u\psi(u)) - uV(\psi(u)) \quad \text{and}$$

$$\Gamma \left[ \frac{\Gamma \left[ \frac{V(\psi(u))}{u} \right]}{u} \right] \quad \text{is denoted by} \quad \Gamma^2 \left[ \frac{V(\psi(u))}{u} \right]$$

[1808, 194-197].

The attempt to provide the coefficients of a development in a uniform way by recursive relations was peculiar to Arbogast and Laplace. Further influence by analogy from algebra and from Laplace's methods can be seen in Brisson's procedure below. Formula (16.1), noticed Brisson, is but an extension of Taylor's theorem for expansion of  $f(x+i)$  in series. Exactly as the latter

theorem affords a method for providing expansions analogous to those of  $(x+i)^n$  for any  $n$ , his own formula leads to expansions of forms such as  $d^n(\psi(u)n(u))/dx^n$  in powers of  $n$  in analogy with the development of  $a^n$  [1808, 198-199].

Thus, since the development of  $a^n$  involves the transformation  $a^n = (1+a-1)^n$ , that of  $d^n u/dx^n$  would accordingly involve the transformation  $u = e^x u e^{-x}$ . Thus, if  $Z$  stands for  $d^n u/dx^n$ , by putting  $v = u e^{-x}$ , the given expression whose development is sought becomes  $Z = d^n(e^x v)/dx^n$  and (16.1) can accordingly be applied with  $u=x$ ,  $\psi(u)=e^x$ ,  $n(u)=v(x)$  and  $V$  the  $n^{th}$  differential [1808, 199-200].

Brisson used to cast partial differential equations in the operator form  $\nabla z = 0$ ,  $z$  <sup>being</sup> the function of  $x, y, \dots$  under determination. Then he would set  $z$  in the form of series. In the case where coefficients are proved to be independent of the form of the unknown function  $n(u)$  of that development, Brisson would assume for convenience  $e^{au}$ , " $a$ " constant, instead of  $n(u)$ . Then, like Laplace, he would proceed by differentiation relative to  $a$  and by putting afterwards  $a=0$  [1808, 254-261].

Another characteristic of Brisson's work was his switching from differential indices to exponents. Proposing the solution of

$$(16.3) \quad \xi + \nabla \xi = M,$$

by iteration he arrived at

$$(16.4) \quad \xi = \frac{M}{1-\nabla} = M - \nabla M + \nabla^2 M - \nabla^3 M + \dots$$

Putting  $\nabla' M$  instead of  $\nabla M$  to denote the change of indices, he was led to the solution of (16.3) in the operational form

$$(16.5) \quad \xi = \frac{1}{1+\nabla'} M$$

[1808, 238] (2).

J.F.Français spoke with admiration of Brisson's work but did not regard it as sufficiently rigorous [1813, 270]. His elder brother, F.J.Français wrote a paper on partial differential equations in 1795 and collaborated with Arbogast in elaborating his Calcul des dérivations [1800]. He produced further work in developing functions in series but his work was not published. It was by inspiration from Arbogast's work and by the study of his brother's papers that J.F.Français produced his memoir [1813]

where he tried to justify the method of separation of symbols and apply it consequently to the solution of various kinds of equations.

Comparing series of equations, as  $aF(x,y)+bF(x,y)+\dots=0$  and  $(a+b+\dots)F(x,y)=0$ , Français was very close to the notion of distributivity, which was introduced by Servois in 1814. Despite his claim for establishing rigour in his processes, Français was based in fact solely upon analogy and induction. Further comparing the equations mentioned above with differential equations, such as  $\bigcirc^n F(x,y)+a\bigcirc^{n-1}F(x,y)+\dots=0$ , he claimed that "on peut considérer les constants comme des échelles .... et traiter les échelles comme des constants" [1813, 245-6].

Thus, according to the spirit of the times, Français viewed differential and finite difference operators as algebraic entities. His procedures were primarily based upon the application of <sup>the</sup> operator  $E$  defined as  $1+\Delta$ . Denoting  $du/dx$  by  $\delta u$ , he obtained, on lines similar to Arbogast, the relations

$$\begin{aligned} E &= 1+\Delta = e^{\delta}, & E^{\kappa} &= e^{\kappa\delta} = (1+\Delta)^{\kappa} \\ (16.6) \quad \Delta^{\kappa} &= E^{\kappa}-1 = (1+\Delta)^{\kappa}-1 = e^{\kappa\delta}-1 \\ \delta &= \log E = \log(1+\Delta) = \log(1+\Delta)^{1/\kappa} \end{aligned}$$

[1813, 250].

He consequently provided examples of developments in which  $E$  could be conveniently applied. The following example was attributed to his brother. Starting with Euler's formula

$$(16.7) \quad \frac{n}{2} = \sin a - \frac{1}{3^2} \sin 3a + \frac{1}{5^2} \sin 5a - \dots,$$

he obtained a much more general result. First (16.7) is cast in exponential form

$$(16.8) \quad \frac{n}{2} a\sqrt{-1} = (e^{a\sqrt{-1}} - e^{-a\sqrt{-1}}) - \frac{1}{3^2} (e^{3a\sqrt{-1}} - e^{-3a\sqrt{-1}}) + \dots$$

Next,  $a\sqrt{-1}$  is replaced by  $\delta$ , and according to (16.6),  $e^{a\sqrt{-1}} = e^{\delta} = E$ . Thus (16.8.) is written as

$$(16.9) \quad (n/2)\delta = (E-E^{-1}) - (1/3^2)(E^3-E^{-3}) + \dots$$

When applied to an arbitrary function  $\phi(x)$ , formula (16.9) yields

$$(16.10) \quad (n/2)\delta\phi(x) = [\phi(x+1)-\phi(x-1)] - (1/3^2)[\phi(x+3)-\phi(x-3)] + \dots$$

[1813, 252-3].

Formula (16.9) is a remarkable result in the theory of equivalent operations different from those given in (16.6). Due to the bold step of substituting a constant with an operation, he was led from a specific formula of trigonometric expansion to a general development in an arbitrary function  $\varphi(x)$ . Most of his procedures are very similar in nature to those used by English analysts in the late 1830's.

Français went on to apply his method of equivalent operations for the solution of differential equations. Before doing so he made the following observations in order to draw attention to certain restrictions that had to be considered. Let  $F(x,y)=0$  be put in the form  $y=\varphi(x)$ . Applying  $\delta-a$  on both members we have

$$(16.11) \quad (\delta-a)y = (\delta-a)\varphi(x).$$

If the second member of (16.11) is  $\neq 0$ , then

$$(16.12) \quad (\delta-a-b)y = (\delta-a-b)\varphi(x)$$

can be deduced from (16.11), but not otherwise [1813, 255].

If  $(\delta-a)\varphi(x)=0$ , then it holds that  $(\delta-a)y=0$ . This latter equation expresses a relation between "les échelles". Hence  $\delta-a=0$  follows, but not  $y=0$ . The former relation is used for the determination of  $\varphi(x)$  as follows:

$$\delta-a=0 \longrightarrow \delta=a \longrightarrow e^a=e^a$$

$$(16.13) \quad E=e^a \longrightarrow E^\kappa = e^{a\kappa} \longrightarrow 1=e^{a\kappa}E^{-\kappa}$$

$$\varphi(x)=e^{a\kappa}\varphi(x-\kappa) \longrightarrow \varphi(\kappa)=e^{a\kappa}\varphi(0)=ce^{a\kappa}$$

and finally  $\varphi(x)=ce^{ax}$  [1813, 255-6].

In a similar way he solved the mixed equation

$$(16.14) \quad \varphi(x) + \Delta\varphi(x) - a\frac{d\varphi(x)}{dx} - b\varphi(x) = 0.$$

By separation of symbols and (16.6) we get an equation in  $E$ :

$$(16.15) \quad E-a\log E-b=0.$$

Let (16.15) be solved algebraically for  $E$  and let its value be  $\lambda$ . The steps followed in (16.13) will lead us to

$\varphi(x)=\lambda^\kappa E^{-\kappa}\varphi(x)=\lambda^\kappa\varphi(x-\kappa)$ , and finally to  $\varphi(x)=c\lambda^x$ ,  $c=\varphi(0)$ . Français added that up to that time equation (16.14) could be integrated only by the series method [1813, 257].

By means of factorization, he was able to deal with linear equations of order  $>1$  with constant coefficients. His procedure in the case of multiple roots is worth of attention. Suppose we have to determine  $\varphi(x)$  given by

$$(16.16) \quad (\delta-a)^2 \varphi(x)=0.$$

It follows that  $(\delta-a)^3=0$ . Let  $\delta-a=\mu$ , ( $\mu^3=0$ ). Then  $e^\mu=1+\mu+(1/2)\mu^2$  and thus

$E=e^\delta=e^{\mu+a}=e^\mu e^a=(1+\mu+(1/2)\mu^2)e^a$ , so  $\varphi(x)=\varphi(0) \cdot (1+\mu+(1/2)\mu^2)^x e^{ax}$ , according to the last steps in (16.13). Since  $\mu^3=0$ , the development of  $(1+\mu+(1/2)\mu^2)^x$  can be put in the form

$$1 + (\mu + \frac{1}{2}\mu^2) \frac{x}{1} + \mu^2 \frac{x(x-1)}{1 \cdot 2} = 1 + \mu_1 x + \mu_2 x^2.$$

It follows finally that  $\varphi(x) = (c_1 + c_2 x + c_3 x^2) e^{ax}$  [1813, 260].

J.F.Français produced a summary of Arbogast's book [1800] in his [1815]. In this account he tried to make Arbogast's calculus more accessible by clarifying his notations, principles and proofs [1815, 62]. In the conclusion of this memoir he made the following observation: the passage from Taylor's theorem to those provided by Arbogast for the development of functions of polynomials or functions of other functions is nothing more but the passage from the differentiation of a function, regarding the differential of the principle variable as a constant, to the differentiation of that function without regarding any differential as a constant [1815, 110-111] (3).

With Servois we have the next step, after Français, towards rigour in the calculus of operations. In his "Essai sur un nouveau mode d' exposition des principes du calcul différentiel" [1814], he provided a foundational study of the calculus of functions and of differential operators. First he gave the definition of the inverse function  $f^{-1}$  of  $f$  given by the double equation (16.17)  $f \circ f^{-1} z = f^{-1} \circ f z = z$

[1814, 94]. As we shall see below, Sarrus proved with counter-examples that (16.17) is in fact erroneous as a definition. Next he introduced the terms "distributive" and "commutative function" as follows. Distributive is a function  $\varphi$  if the equation

$$(16.18) \quad \varphi(x+y+\dots) = \varphi(x) + \varphi(y) + \dots$$

holds true. Two functions  $\varphi$  and  $f$  are commutative if

$$(16.19) \quad \varphi f z = f \varphi z$$

[1814, 98].

Servois provided with various examples such as  $aEz = Ea z$ ,  $a \sin z \neq \sin az$ , where  $a$  is a constant and  $\sin(z+u) \neq \sin z + \sin u$  or  $E(x+y) = Ex + Ey$ . <sup>There</sup> followed a list of theorems such as

- (16.20) The composition of distributive functions is a distributive function,
- (16.21) If a number of functions are commutative in twos, then they are commutative taken in any number,
- (16.22) If  $\varphi$ ,  $f$  are commutative, then  $f, \varphi^{-1}$  as well as  $f^{-1}, \varphi^{-1}$ , are commutative too
- [1814, 99-102].

I would like to add few examples that illustrate Servois's applications of definitions (16.18)~ (16.19) and the accompanying theorems. Let  $F(x) = \varphi^x(z)$ . Then  $\Delta F(x) = \varphi^{x+\alpha}(z) - \varphi^x(z)$ . If  $\varphi$  is distributive then  $\Delta F(x) = \varphi^x[\varphi^\alpha(z) - z]$ . Let further  $\varphi^\alpha(z) - z = f(z)$  and  $\varphi$  commutative. Then  $\varphi^\alpha(f(z)) - f(z) = f(\varphi^\alpha(z)) - f(z) = f(\varphi^\alpha(z) - z) = f^2(z)$ . By iteration it follows inductively that  $\Delta^n F(x) = \varphi^x(f^n(z))$  [1814, 111-112]. With the following example,

$\varphi(z) = f(z) + f_1(z) \longrightarrow f^{-1}\varphi(z) = z + f^{-1}f_1(z)$ , he implies the distributivity of the inverse of a distributive function. However, this proof is missing from his account, together with the theorem  $(f\varphi)^{-1} = \varphi^{-1}f^{-1}$ , for arbitrary functions [1814, 115] (4).

Servois was most probably influenced by Français's work on equivalent operations. The latter had been very close to the notions of distributivity and commutativity which were implicitly taken for granted into his procedures. Both Français and Servois had a considerable impact on the development of the calculus of operations in England in the late 1830's and early 1840's. They also influenced certain less known figures such as Sarrus and Schmidten. The former's work bears a strong resemblance to Murphy [1837]. Pressuming that it is quite probable that Murphy drew from Sarrus [1822] we will devote the last part of this section to the work of Sarrus and Schmidten (5).

Like Français and Servois, Sarrus's and Schmidten's papers were published in Gergonne's Annales. The Cambridge Mathematical Journal, founded by Gregory in 1839, was roughly equivalent in level with the Annales des Mathématiques Pures et Appliquées. Gergonne's journal, launched in 1810, included articles on unusual applications and algebraic speculations by provincial fringe figures, whereas the Journal de l'Ecole polytechnique took mainly contributions from Parisian authorities (6).

Sarrus published during the period 1819-1826 around 17 memoirs in the Annales. The topics of his research included number theory, differential equations, expansions in series,

geometrical constructions, theory of fluids and the calculus of variations. For his contributions on the latter in 1848 he is acknowledged by [Jellet 1850, xix; Kline 1972, 584].

We will focus here on his "Essai sur le développement des fonctions en series" [1822]. The aim of Sarrus's memoir was to provide a simple, direct and uniform method for the development of functions in series other than the method of separation of symbols and of generating functions. This method would rest upon principles which would render it beyond any objections, leading to an infinite <sup>number</sup> of formulae unattainable by the two former methods. His point of departure was Servois [1814] <sup>which</sup> would now be enriched with theorems which had escaped his notice [1822, 289].

The most interesting aspect of Sarrus's work is his study of inverse functions. His procedure, <sup>while</sup> resting on not altogether faultless principles, is far more rigorous than that followed by Servois. Sarrus was apparently the first to determine the inverse of a distributive operation  $A$  by the addition of its application to 0,  $A^{-1}(0)$ . He also demonstrated, in a rather unclear way, that the inverse of every distributive operation is also distributive. Many of his results in the foundations of inverse operations are attributed nowadays to Murphy [1837] [see Pincherle 1912, 5-9].

French analysts of that time referred to the letters  $F$  or  $D$ , which denote a function or an operator, as "characteristiques". The same term was used by Sarrus for the symbol  $\nabla$  which stood for an arbitrary function or operation. If  $u$  is a given function, then  $\nabla u$  is called the "result" and  $u$  the "subject". The inverse of  $\nabla$  is denoted by  $\nabla^{-1}$  [1822, 290-1].

Let  $\nabla$  denote the function that changes  $x$  into  $\log x$ . Then  $\nabla^{-1}$  denotes the function that changes  $x$  into  $e^x$ , since  $\log(e^x) = x$ . Therefore, by definition,

$$(16.23) \quad \nabla \nabla^{-1} u = u,$$

and by iteration,

$$(16.24) \quad \nabla^n \nabla^{-n} u = u$$

[1822, 292]. With a <sup>u</sup> counterexample Sarrus refuted the formula (16.17) given by Servois. Let  $\nabla$  be an operation which applied to any function  $u$  has as result the change of  $x$  into  $x^{n+1}$  and division of this result by  $u$ . If  $u=ax$ , " $a$ " a constant, then

$$\nabla^{-1} \nabla u = \nabla^{-1} \frac{ax^{n+1}}{ax} = \nabla^{-1} x^n = kx$$



for any  $k$ , since  $\nabla kx = \nabla x = x^n$ . Thus, in general

$$(16.25) \quad \nabla^{-1} \nabla u \neq u$$

Another counterexample was with  $u = 2\arctan x$  and  $\nabla u = \cos u$ . Once more (16.25) holds true [1822, 293].

Sarrus introduced the known concept of commutativity and proved among other theorems that

$$(16.26) \quad \nabla^{-1} \Gamma u = \Gamma \nabla^{-1} u \quad \text{and} \quad (16.27) \quad \nabla^{-1} \Gamma^{-1} u = \Gamma^{-1} \nabla^{-1} u,$$

only under the restriction that  $\Gamma$  and  $\nabla$  are commutative [1822, 294-295].

Then, he gave the definition of distributivity and proved by induction that if  $\nabla$  is distributive,  $\nabla^n$  is also distributive [1822, 296]. Cautious with the restrictions implied in the notion of the inverse operation  $\nabla^{-1}$ , Sarrus, before providing an answer to whether  $\nabla^{-1}$  is distributive, asks the following question "Quelles sont les divers valeurs de la dérivée d'ordre négatif  $\nabla^{-1}p$  ?" [1822, 297].

Let  $u$  a particular value of  $\nabla^{-1}p$  and  $u+t$  another value of it. Then,

$$(16.28) \quad \nabla(u+t) = \nabla u = p.$$

Also, by the distributivity of  $\nabla$ , we have

$$(16.29) \quad \nabla(u+t) = \nabla u + \nabla t = p + \nabla t.$$

Therefore, from (16.28) and (16.29) it follows that  $\nabla t = 0$  and that

$$(16.30) \quad \nabla^{-1}p = u+t, \quad \nabla t = 0.$$

Thus,  $\nabla^{-1}p$  is defined. At this point Sarrus introduces the concept of "fonctions complémentaires" which, as  $t$ , have to be added to a particular value of a "dérivée d'ordre négatif, pour en deduire les autres valeurs de la même dérivée" [1822, 297] (7).

Next Sarrus proves the distributivity of  $\nabla^{-1}$  in a rather unorthodox manner. The difficulty he is confronted with, is due to the fact that, instead of starting with the direct formula provided by (16.29), he starts with the formula  $\nabla^{-1}p + \nabla^{-1}q = u$ . Then, applying  $\nabla$ , he gets  $p+q = \nabla u$  or  $\nabla^{-1}(p+q) = \nabla^{-1} \nabla u = u+t$ . Or,

$$(16.31) \quad \nabla^{-1}(p+q) = \nabla^{-1}p + \nabla^{-1}q + t,$$

$t$  the complementary function. In order to regard that (16.31) can be devoid of  $t$  he wrote that it is always possible to choose for either of  $\nabla^{-1}p$ ,  $\nabla^{-1}q$  the same complementary function as that for  $\nabla^{-1}(p+q)$ . Thus  $t$  can be regarded as equal to 0 [1822, 298].

The proof would have been much simpler, without the interaction<sub>of</sub> of  $t$ , if Sarrus had started with

$$\nabla(p+q) = \nabla p + \nabla q = p_1 + q_1 \longrightarrow \nabla^{-1}(p_1 + q_1) = p + q = \nabla^{-1}p_1 + \nabla^{-1}q_1.$$

The reason why Sarrus was not able to foresee this way of demonstration was because his definition of  $\nabla^{-1}$  lacked the necessary precision. For this proof, given by Murphy [1837, 188], is based throughout on the following definition (in Sarrus's notation):

$$(16.32) \quad \nabla u = v \longrightarrow \nabla^{-1} v = u,$$

from which  $\nabla \nabla^{-1} v = v$  follows as a result. Sarrus was very close to (16.32) but it was a failure not to express it clearly.

Then he goes on to prove a very general theorem for the expansion of a function  $u$  in arbitrary operations whose only property is their distributivity. Let  $\nabla u = p + \Gamma u_1$ ,  $\nabla$ ,  $p$ ,  $\Gamma$  given. Then assume  $\nabla_1 u_1 = p_2 + \Gamma_2 u_2$  etc. From the first formula we have

$$(16.33) \quad u = \nabla^{-1} p + \nabla^{-1} \Gamma u_1, \text{ and by iteration,}$$

$$(16.34) \quad u_1 = \nabla_1^{-1} p_1 + \nabla_1^{-1} \Gamma_1 u_{1+1}.$$

By substitution of  $u_1$ ,  $u_2$  etc from (16.34) in (16.33) we have the formula

$$(16.35) \quad u = \nabla^{-1} p + \nabla^{-1} \Gamma \nabla_1^{-1} p_1 + \nabla^{-1} \Gamma \nabla_1^{-1} \Gamma_1 \nabla_2^{-1} p_2 + \dots$$

which, regarding  $i \longrightarrow \infty$ , is a series dependent on  $p_1, p_2, \dots$ . "Il est de plus évident", observed Sarrus, "qu'en choisissant ces fonctions, ainsi que les caractéristiques  $\nabla, \nabla_1, \dots, \Gamma, \Gamma_1, \dots$  d'une manière convenable, cette série pourra toujours être rendue aussi convergente qu'on voudra" [1822, 299-300].

If we let  $\nabla_i = \nabla$  and  $\Gamma_i = \Gamma$ , for all values of  $i$ , then (16.35) becomes

$$(16.36) \quad u = \nabla^{-1} p + (\nabla^{-1} \Gamma) \nabla^{-1} p + \dots + (\nabla^{-1} \Gamma)^{i-1} \nabla^{-1} p + (\nabla^{-1} \Gamma)^i u.$$

This formula is identical with that given by Murphy, where  $\nabla$  stands for  $\Theta, \Gamma$  for  $\Theta_1$  and  $u$  for  $(\Theta - \Theta_1)^{-1} p + u_1$  [Murphy 1837, 194; Sarrus 1822, 301] (8).

As an interesting application of (16.36), he chose the following. Let

$$(16.37) \quad \Psi(u) = p,$$

an equation of the first degree in finite differences or differentials. The problem is to determine  $u$  on the basis that  $\Psi$  is distributive. Let  $\nabla$  be another distributive operation. Then  $\nabla u = p + \nabla u - \Psi(u)$ . Replace  $\nabla u - \Psi(u)$  by  $\Gamma(u)$ . Then

$$(16.38) \quad \nabla u = p + \Gamma u, \Gamma \text{ distributive.}$$

The function  $u$  can accordingly be determined by formula (16.36) [1822, 301-302]. The operation  $\nabla$  is to be chosen so that  $\nabla^{-1} p$  is easily determined and also so that (16.36) converges. This proce-

dure reminds us strongly of Brisson's respective one in (16.3)-(16.5), only this by Sarrus is even more general and abstract and more complicated.

Sarrus drew on Servois's definitions

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$\frac{u}{x} = \psi(x+1, y)$ , where  $u = \psi(x, y)$ , and applied his theorem (16.36) in order

to obtain various interpolation formulae. These formulae provide further evidence on the analogy already remarked between ordinary interpolation series and Taylor's theorem [1822, 302-306; Servois 1814, 95].

Finally, I refer to H.G. Schmidten, whose "Mémoire sur l'intégration des équations linéaires" [1821] is a typical outcome of the influence of Brisson, Servois and Français on the symbolical solution of differential equations. Schmidten denotes a differential or finite difference equation in the form

$$(16.31) \quad fz = M,$$

where  $z$  the function sought,  $M$  known and  $f$  a "linear" (in the sense of distributive) function with constant coefficients. By the principles of Servois, he claimed, (16.37) is integrated as

$$(16.38) \quad z = N + \frac{1}{f} M, \text{ where } N \text{ a function such as } fN = 0.$$

Thus (16.38), "qui a la forme d'un polynome, pourra être développée par toutes les méthodes connues pour le développement des fonctions, purement algébriques, et l'on parviendra ainsi directement, d'après ces principes, à tous les résultats de M. Français" [1821, 304-5] (9).

## 1.7 Solutions of partial differential equations in definite integral and symbolic forms: 1800-1830.

As we have seen, the study of partial differential equations contributed to the development of other branches of analysis, such as that of functional equations, and the calculus of operations [1.4; 1.6]. Another of its outcomes was the development of definite integral methods introduced by Euler in the 1740's end

the 1750's. Langrange contributed also in this field between 1750 and 1770, followed by Laplace in the late 1770's [Deakin 1981, 346-350].

According to Laplace, Euler was not happy with his use of integral forms for the solution of differential equations, calling them as "fonctions inexplicables". Laplace undertook to clarify these functions presenting his first systematic study of definite integral methods in his [1872]. In this memoir he founded these methods within the wider framework of his "calcul des fonctions génératrices" which provided, according to him, an excellent mode of demonstration [Laplace 1811, 357-8].

Laplace's memoir [1782] had a marked impact on Brisson [1.6]. By the late 1800's we notice an extensive development of definite integral methods, including evaluation of definite integrals, by Laplace, Brisson, Poisson, Fourier and Cauchy. Apart from Laplace, all these analysts made some contributions around the same time also to the application of operator methods of solution of partial differential equations. Important novelties were introduced such as the Fourier transform, extended by Cauchy in the 1820's. Thus, the starting point of this section will be Laplace's work during the period 1785-1811 on definite integral methods. We focus next on Fourier's innovations, commenting also on Poisson's work. We end with Cauchy's reformulation of the calculus of operations in the 1820's.

One instance of Laplace's work in this area is his method of solving ordinary differential equations of the form

$$(17.1) \quad V + sT = 0,$$

where  $V, T$  are linear functions of  $y(s)$  and its differentials -and/or its differences- and where the variable  $s$  enters only in the first degree. Laplace assumed  $y(s)$  to be of the form

$$(17.2) \quad y(s) = \int \phi(x) e^{-sx} dx,$$

and under this transform, later called after him, equation (17.1) assumes the form

$$(17.3) \quad \int \phi(x) dx \left( M + N \frac{de^{-sx}}{dx} \right),$$

where  $M, N$  functions of  $x$  [1785, 249-250].

From (17.3) he deduced the equations

$$(17.4) \quad M\varphi(x) - \frac{d(N\varphi(x))}{dx} = 0, \quad \text{and}$$

$$(17.5) \quad C + e^{-\sigma x} N\varphi(x) = 0.$$

By integration, (17.4) leads to the determination of function  $\varphi(x)$  as

$$(17.6) \quad \varphi(x) = \frac{H}{N} e^{\int \frac{M}{N} dx}$$

in an arbitrary constant. Equation (17.5) relates only to the limits of the integral form (17.2) [1875, 250-251] <sup>(1)</sup>.

In the case of partial differential equations, the method which featured prominently in the 1800's led to series solutions converted to definite integrals. This method was applied in <sup>the</sup> late 1800's almost simultaneously by Poisson, Laplace and Fourier for the solution of the heat-diffusion equation. Laplace's procedure in 1809 motivated Fourier to establish his own integral methods in 1811 which form an important advance in analysis. Omitting Poisson's procedure, we will first give a sketch of Laplace's solution of this equation in his "Mémoire sur divers points d'analyse" [1809].

Laplace first solved the equation

$$(17.7) \quad \Delta^2 y(x, x_1) = \Delta_1 y(x, x_1),$$

where  $\Delta$  and  $\Delta_1$  are relative to  $x$  and  $x_1$  respectively. For he believed that "Dans les questions délicats de l'Analyse infinitésimale, il est très utile de considérer les choses relativement aux différences finies, et de voir les modification qu'elles subissent dans le passage du finis à l'infiniment petit" [1809, 187].

Hence, Laplace regarded  $u(t, t_1)$  as the generating function of  $y(x, x_1)$ . Accordingly, by his theory as in [1782] -see section 1.5- the generating function of  $\Delta^2 y(x, x_1) - \Delta_1 y(x, x_1)$  will be  $u[(1/t - 1)^2 - (1/t - 1)]$ . By development and passage from the functions to their coefficients, he obtained the solution of (17.7) in the series form

$$(17.8) \quad y(x, x_1) = y(x, 0) + x_1 \Delta^2 y(x, 0) + \frac{x_1(x_1 - 1)}{1.2} \Delta^4 y(x, 0) + \dots$$

[1809, 188].

Now, switching from finite differences to differentials and putting  $y(x,0)=\varphi(x)$ , equation (17.7) becomes

$$(17.9) \quad \frac{\partial^2 y}{\partial x^2} = \frac{\partial y}{\partial x_1},$$

and its solution (17.8) respectively,

$$(17.10) \quad y = \varphi(x) + x_1 \frac{d^2 \varphi(x)}{dx^2} + \frac{x_1^2}{1.2} \frac{d^4 \varphi(x)}{dx^4} + \dots$$

[1809,189]. Based upon known results, such as

$$(17.11) \quad \int_{-\infty}^{+\infty} e^{-z^2} dz = \sqrt{\pi},$$

he obtained consequently (17.10) in the functional form

$$(17.12) \quad y = \frac{1}{\sqrt{\pi}} \int_{-\infty}^{+\infty} e^{-z^2} \varphi(x+2z\sqrt{x_1}) dz.$$

Poisson had given the solution of (17.9) in power-series with two arbitrary functions [see (17.26)] observing that the complete integral depends only upon one arbitrary function. So Laplace went on to show that Poisson's claim was right and that indeed his own definite integral solution (17.12) was the complete integral of the heat-diffusion equation (17.9) [1809,187,190-193] (2).

Two years later Laplace applied his transform (17.2) for the solution of linear partial differential equations related to problems of probability. Dealing with the equation

$$(17.13) \quad \frac{U}{r} = 2U + 2\mu \frac{\partial U}{\partial \mu} + \frac{\partial^2 U}{\partial \mu^2},$$

he assumed the transform

$$(17.14) \quad U(\mu) = \int \varphi(t,r) e^{-\mu t} dt,$$

and arrived at the complex definite integral form

$$(17.15) \quad U(\mu) = e^{\mu^2} \int_{-\infty}^{+\infty} e^{-s^2} \Gamma[(s-\mu i)e^{-2r}] ds,$$

where  $\Gamma$  an arbitrary function [1811,377-379] (3).

In the introduction of this paper Laplace claimed that Arbogast's method of separation of symbols forms part of his own "calcul des fonctions g  n  ratrices". This calculus "donne   la fois la d  monstration et la vraie m  taphysique" of Arbogast's

method as well as illustrates "l'analogie singulière des puissances et des différences" observed by Leibniz [1811, 358-360] <sup>(4)</sup>.

New methods in this direction were proposed by Fourier around 1811. Dealing with the diffusion of heat, Fourier contributed substantially in both the physical and mathematical aspects of this theory establishing a new general integral method which involved trigonometric functions. Acquainted with Laplace's work on the diffusion equation, he was motivated to find an integral solution for this equation. His major achievements were presented in a paper published in 1811 <sup>(5)</sup>.

His results were consequently incorporated in his Théorie de la Chaleur [1822] where he presented the application of operator calculus for the solution of partial differential equations. Operator methods were first introduced by Fourier in a paper written in 1818 [Grattan-Guinness 1990, art 10.4.2]. Dealing with

$$(17.16) \quad \frac{du}{dt} = \frac{d^2u}{dx^2} \quad [d \text{ read as } \partial]$$

in a boundary domain, he assumed its solution to be in the series form

$$(17.17) \quad u = \sum_n a_n e^{-q_n^2 t} \cos q_n x .$$

Under some further assumptions, (17.17) was changed into an integral

$$(17.18) \quad u = \int_0^\infty Q(q) \cos qx e^{-q^2 t} dq ,$$

with the initial condition

$$(17.19) \quad v = f(x), \text{ when } t=0, 0 \leq x \leq +\infty .$$

Under (17.19) we have

$$(17.20) \quad f(x) = \int_0^\infty Q(q) \cos qx dq ,$$

an integral equation where  $Q(q)$  is to be determined. Converting (17.20) back to summation, Fourier, integrating the series term by term over the interval  $[0, n/dx]$ , evaluated its coefficients  $Q_n$  arriving finally at the formula

$$(17.21) \quad Q(q) = \frac{2}{\pi} \int_0^{\infty} f(x) \cos qx \, dx.$$

Substitution of (17.21) back into (17.20) led to the "Fourier integral theorem" for the representation of an arbitrary function  $f(x)$  in the form

$$(17.22) \quad f(x) = \frac{2}{\pi} \int_0^{\infty} \int_0^{\infty} f(u) \cos qu \cos qx \, du dq$$

[1822, art. 342-359].

Therefore, the general solution of (17.16) of the form (17.18) becomes

$$(17.23) \quad u = \frac{2}{\pi} \int_0^{\infty} \int_0^{\infty} f(u) \cos qu \cos qx \, e^{-q^2 t} \, du dq.$$

Fourier used his theorem (17.22) for the direct evaluation of definite integrals, such as (17.11) [1822, art 358-370]. Finally he compared the various forms of the solution of (17.16) <sup>that</sup> he had obtained so far.

These solutions included (17.23) in the form

$$(17.24) \quad u = \frac{1}{2\pi} \int_0^{\infty} \int_0^{\infty} f(u) \cos(qx-qu) \, e^{-q^2 t} \, du dq,$$

Laplace's functional form (17.12), as well as a third form

$$(17.25) \quad u = \int_{-\infty}^{\infty} \frac{du \, f(u)}{2 \cdot \sqrt{\pi} \cdot \sqrt{t}} \, e^{-\frac{(u-x)^2}{4t}}.$$

The form (17.25) was deduced from (17.24) by integration relative to  $q$  and from his earlier evaluation of the definite integral which thus emerges. In fact, Laplace's solution (17.12) was deduced from (17.25) by letting  $(u-x)/(2\sqrt{t})=z$ ,  $t=x_1$  and  $f(u)=\varphi(u)$  [1822, art 397-398].

In the next article of [1822] he deduced Laplace's series form (17.10) and Poisson's respective solution

$$(17.26) \quad u = \varphi(t) + \frac{x^2}{2!} \varphi'(t) + \frac{x^4}{4!} \varphi''(t) + \dots + x \varphi(t) + \frac{x^3}{3!} \varphi'(t) + \dots$$

acknowledging the latter for his remark that the complete solution depends upon one arbitrary function.

In article 401 Fourier observed that the series (17.10) can in fact be written in the form

$$(17.27) \quad u = e^{-tD^2} \varphi(x), \quad D = d/dx.$$



Indeed, by expansion, (17.27) assumes the form

$$(17.28) \quad u = \varphi(x) + t \frac{d^2}{dx^2} \varphi(x) + \frac{t^2}{2!} \frac{d^4}{dx^4} \varphi(x) + \dots$$

which is identical to (17.10) if we regard  $t=x_1$ . Further, since we know that

$$\cos x = 1 - \frac{x^2}{2!} + \frac{x^4}{4!} - \frac{x^6}{6!} + \dots \quad \text{the first part of the formula}$$

(17.26) can be written symbolically in the form

$$(17.29) \quad \cos[x\sqrt{(-D)}]\varphi(t).$$

The second part of (17.26) can be deduced from (17.29) by integration relative to  $x$  and substitution of  $\psi(t)$  for  $\varphi(t)$ . Hence, the complete solution of the heat-diffusion equation can also be written as

$$(17.30) \quad u = \cos[x\sqrt{(-D)}]\varphi(t) + \psi(t) \int_0^x \cos[x\sqrt{(-D)}] dx^{(?)}$$

He went on to verify these operator forms by differentiation. A similar treatment was undertaken for the solution of other equations, such as:

$$(17.31) \quad \frac{d^2 u}{dt^2} = \frac{d^2 u}{dx^2} + \frac{d^2 u}{dy^2}$$

For (17.31) he put  $D = \frac{d^2}{dx^2} + \frac{d^2}{dy^2}$  and solved the equation  $\frac{d^2 u}{dt^2} = Du$

[1822, art 401-403; see also, Grattan-Guinness 1990, chapter 10, particularly 10.4.2].

Fourier's solution of partial differential equations as in his [1822] had a considerable impact on English analysts around 1840, particularly R.L.Ellis and D.F.Gregory. However, the Fourier transform was not widely used. It was acknowledged, however, by Boole in 1859 that Fourier's theorem affords the only general method known for the solution of partial differential equations with more than two independent variables<sup>(e)</sup>. Of considerable influence were also Poisson's methods for the solution of partial differential equations, including his original work on the evaluation of definite integrals. Particularly, his solution for the 3-dimensional wave equation, (17.32), was acknowledged by Boole as "entirely special" [see (8)].

Around 1818 Poisson used operator methods for the solution of the wave equation:

$$(17.32) \quad \frac{d^2 u}{dt^2} = h^2 \left[ \frac{d^2 u}{dx^2} + \frac{d^2 u}{dy^2} + \frac{d^2 u}{dz^2} \right]$$

h a constant. Putting  $\delta^2$  for  $\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}$  he wrote (17.32) in the form

$$(17.33) \quad \frac{d^2 u}{dt^2} = h^2 \delta^2 u.$$

He then provided a power-series solution in which the factor

$$(17.34) \quad e^{h\alpha t}$$

appeared where  $\alpha = \rho \cos \theta$  and  $\rho^2 = \delta^2$ . Thus (17.34) was a differential operator and by application of Lagrange's theorem (15.1) and by properties of definite integrals, he arrived at the solution of (17.32) in functional trigonometric form<sup>(9)</sup>.

In his [1823] Poisson undertook a general study of the equation

$$(17.35) \quad \frac{d^2 z}{dt^2} = a^2 \left[ \frac{d^2 z}{dx^2} - \frac{mz}{x^2} \right].$$

Putting  $x_1 = x + at$ , he regarded  $z$  to be a series in ascending powers of  $x$  in the form

$$(17.36) \quad z = x^k \phi x + A_1 x^{k+1} \frac{d\phi x_1}{dx_1} + A_2 x^{k+2} \frac{d^2 \phi x_1}{dx_1^2} + \dots$$

where  $k$  is determined by

$$(17.37) \quad k(k-1) = m.$$

He then provided recursive relations for the coefficients  $A_n$  and finally found the complete series solution of (17.37) as a series in two arbitrary functions [1823, 216-8].

Next he noticed that for  $m=0$  (17.35) becomes the vibrating-string equation (14.13), and under the transformation  $z = e^{h\alpha t} y(x)$  we arrive from (17.35) at the generalized form of the earth figure equation (14.8) with  $m$  assumed now to be of the form  $i(i+1)$ . Poisson mentioned that Legendre had integrated the latter

equation in finite form when  $i$  is an integer [1823, 219-222]. Then, he went on to put the complete series solution of (17.35) in finite form by means of definite integrals. Putting

$$A_n = \frac{B_n}{n!} \text{ and } B_n = \int_0^{\pi} P p^n dp, \text{ } P \text{ a function of } p, \text{ he determined } A_n \text{ as}$$

as definite integrals and finally, by means of Taylor's theorem, he arrived at the form

$$(17.38) \quad z = x^k \int_0^{\pi} \phi(x \cos w + at) \sin^{2k-1} w dw + x^{k_1} \int_0^{\pi} \phi(x \cos w + at) \sin^{2k_1-1} w dw$$

where  $k, k_1$  the two values of  $k$  determined by (17.37) [1823, 222-225]. He then gave specific values to  $k, k_1$  and provided by (17.38) the solutions of known equations. Poisson's procedure was resemblant to that of Brisson's in his [1808] whom in fact he mentioned at a certain instance [1823, 228].

English analysts, such as Ellis, Gregory and Boole, though aware of Poisson's work in general, seem to have ignored his treatment of the earth-figure equation<sup>(10)</sup>. Aware also of definite integral solutions, they did not use them widely. What was for them a routine method in <sup>the</sup>late 1830's <sup>and</sup> early 1840's was the use of series method on lines similar to those of Poisson but most probably independent from him [see chapter 4].

We end with Cauchy's work on operator calculus published in 1827. In the introduction of his paper "Sur l'analogie des puissances et des différences" [1827] Cauchy mentioned Brisson's unpublished memoirs written in 1821 and 1823. Cauchy was impressed by Brisson's symbolical methods which he developed in his [1827] in a modified, more general and more rigorous way<sup>(11)</sup>. We will note here some of the most characteristic procedures and results given by him in that memoir.

Putting  $D$  for  $d/dx$ , Cauchy wrote symbolically the solution  $u$  of the equation

$$(17.39) \quad F(D)u = f(D)f(x)$$

in the form

$$(17.40) \quad u = \frac{f(D)}{F(D)} f(x).$$

One can easily establish, he wrote, the formulae

$$(17.41) \quad F(D, \Delta) [f(D, \Delta) f(x)] = [F(D, \Delta) f(D, \Delta)] f(x) = f(D, \Delta) [F(D, \Delta)] f(x),$$

$$D^{n_{ex}} = r^{n_{ex}}$$

$$(17.42) \quad F(D)e^{rx} = e^{rx}F(r)$$

$$F(D)[e^{rx}f(x)] = e^{rx}F(e+D)f(x).$$

and others of similar nature [1827,200-201]. Formulae (17.41)-(17.42) were to be reinvented by Gregory and Boole around 1839-1844 [chapter 4].

Cauchy's innovation was to draw on his own complex-variable version of Fourier's integral theorem

$$(17.43) \quad f(x) = \frac{1}{2\pi i} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} e^{a(x-\lambda)} f(\lambda) da d\lambda,$$

where  $i = \sqrt{-1}$ , in order to find expressions for  $F(D)f(x)$ ,  $F(\Delta)f(x)$  etc. According to the transformation (17.43), each of the expressions mentioned above acquires a double integral form which, he claimed, has very important properties [1827, 202-208] <sup>(12)</sup>.

He then went on to introduce two methods for the solution of a linear equation with constant coefficients which is written symbolically in the form

$$(17.44) \quad F(Dx, Dy, \dots, \Delta x, \dots)u = f(x, y, \dots).$$

The principles of the first method sketched below were attributed to Brisson [see also 1.6]. Let  $F$  be decomposed in  $n$  integral functions  $F_1, \dots, F_n$ , then equation (17.44) is reduced to the system of equations

$$F_1(Dx, \dots) u_{n-1} = f(x, y, \dots)$$

(17.45) .....

$$F_n(Dx, \dots)u = u,$$

If function  $F$  is of degree  $n$ , then the integration of (17.44) amounts, to the integration of  $n$  equations of the first order.

According to this method, independently introduced by Gregory in 1839, the values of  $u$ ,  $u_1$ ,  $u_2$ , ... are given in the form of multiple integrals which are reduced to integrals of a lower order by integration by parts. Cauchy obtained for the case of a differential equation of order  $n$  with constant coefficients  $a_0, a_1, \dots, a_n$  the characteristic equation

$$(17.46) \quad F(r) = a_0 r^n + \dots + a_{n-1} r + a_0 = 0,$$

and examined the cases where the roots of (17.48) are distinct or multiple, real or imaginary [1827,211].

In the course of his paper he regarded the integrals of simple first-order equations, such as,

$$(17.47) \quad \frac{dy}{dx} - ry = f(x) \quad \text{or} \quad (D-r)y = f(x)$$

as known. It was only in the addition to this paper that he applied the symbolic formulae (17.42) for the solution of (17.47). He proposed two methods. According to the first he assumed  $f(x)=0$ , found  $y$  by integration by parts and then, according to the last formula of (17.42) he deduced  $(D-r)[e^{rx}z] = f(x)$  or  $Dz = e^{-rx}f(x)$ , hence

$$z = \frac{e^{-rx}f(x)}{D} = \int e^{-rx}f(x)dx, \text{ and since } y = e^{rx}z \text{ we get}$$

$$(17.48) \quad y = e^{rx} \int e^{-rx}f(x)dx,$$

as it is known.

According to the second method he substituted  $e^{-rx}f(x)$  for  $f(x)$  in the last formula of (17.42) and arrived at

$$(17.49) \quad F(D)[f(x)] = e^{rx}F(D+r)[e^{-rx}f(x)].$$

Finally, applying (17.49) when  $F(D)=D-r$  we have from the second formula in (17.47) that

$$y = \frac{f(x)}{D-r} = e^{rx} \frac{1}{D} [e^{-rx}f(x)] = e^{rx} \int e^{-rx}f(x)dx.$$

This last observation Cauchy attributed again to Brisson [1827, 236-238].

As it is evident from the formulae (17.41) Cauchy was aware of the commutative law. Other instances show also his acquaintance with the distributive law [1843b, 31]. However, despite a general reference to "les géomètres" who had applied symbolical methods for the solution of equations such as (17.47), Cauchy did not refer to either Servois or Français [1827, 236].

The second method suggested for the solution of linear differential equations of order  $n$  with constant coefficients was the expansion theorem (17.51). In the case where the roots of (17.46) are unequal, the solution of equation

$$(17.50) \quad F(D)y = f(x)$$

is given by the theorem

$$(17.51) \quad y = \sum_{k=1}^n \frac{f(x)}{F'(r_k)(D-r_k)},$$

$$\text{where } \frac{f(x)}{D-r} = e^{-rx} \int e^{-rx} f(x) dx \quad [1827, 228].$$

He also studied the case of equal roots and provided examples of the application of the expansion theorem for the solution of finite difference and mixed equations [1827, 229-235].

Cauchy was the first to introduce the expansion theorem (17.51), nowadays attributed to Heaviside [Petrova 1987, 4, 8-11]. This theorem was reinvented independently by Lobatto in 1837 and Boole in 1841 [Deakin 1981, 376]. It is somehow surprising that though Gregory was acquainted by 1839 with Cauchy's methods, he proceeded independently, following a method which was apparently less "confusing" than Cauchy's and closer to that by Brisson [chapter 4].

Most of the results published in Cauchy [1827] were already presented by him in earlier work in 1821-1825. However, I have quoted from [1827] as it is better known. He also produced some work on functional equations of the form

$$\begin{aligned} f(x+y) &= f(x)f(y) \\ (17.52) \quad f(x+y) &= f(x)+f(y) \\ f(xy) &= f(x)+f(y) \end{aligned}$$

[1821, 98-113]. Despite his rigorous study, Cauchy did not develop any general methods for the solution of functional equations, and though his respective work was known around 1836, he was not followed by the English. Equations of the form (17.52) had been studied earlier by Legendre and Lacroix [Dhombres 1986, 157-164].

Cauchy did not carry his symbolical procedures any further. Commenting upon the symbolic form of Taylor's series, he wrote: "Toutefois, ces formules, ainsi deduites d'une équation symbolique, ne pourront encore être considérées comme rigoureusement établies, la méthode qui les aura fait découvrir n'étant en réalité qu'une méthode d'induction" [1843a, 27]. Thus, despite his attempt to ground his method in Fourier integrals, he did not trust the calculus of operations as a rigorous method in general and did not try to justify its principles any further. What he did though, was to focus on problems of convergence and

to apply his "Residual calculus" to the integration of differential and finite difference equations [1843b]. However, this method was not followed by his contemporaries<sup>(13)</sup>.

Closely linked with the operator calculus was another branch of analysis which flourished in the 1820's and 1830's, that of fractional integration. Its possibility was considered by Euler, Arbogast and Laplace. Fourier's theorem (17.22) provided

the value of  $\frac{d^i f(x)}{dx^i}$  for any number  $i$ , but the actual theory of

fractional indices appeared in 1832 in Liouville's work. This branch of analysis was studied by English analysts, particularly by Greatheed in 1839, but it will not be discussed any further in our thesis<sup>(14)</sup>.

As it is argued, Cauchy's work was considerably influenced by Lagrange's algebraic calculus and one immediate evidence for this is Cauchy's description of the content of his Cours d'analyse [1821] as "analyse algebrique" [Grabiner 1981b, 15-18]. However, by the 1820's we have gradually a decline of Lagrangian calculus in France and a new foundation of the calculus upon the notion of limits. The English, though, developed the Lagrangian tradition during the 19<sup>th</sup> century despite their knowledge of Cauchy's work.

The methods presented in this section had not as great an impact on English analysts as those sketched in earlier sections, particularly 1.4-1.6. However, since results by Laplace, Poisson, Fourier and Cauchy were often referred to in their writings, we thought it appropriate to include them so as to have a complete background on French mathematics. And now we proceed to the final section of this chapter where we will introduce the main issues that form the skeleton of our study of English symbolical methods in analysis and logic.

## 1.8 Philosophical and educational issues of French mathematics: 1780-1820; semiotics.

Up to this point we have briefly covered those aspects of French mathematics which were to play a prominent role in the development of symbolical methods in England from the 1800's up to the 1860's. In the course of our study we noticed two different but complementary approaches towards late-18<sup>th</sup>-century mathematization of mechanics. With Laplace we have the "analytization" of mechanics, whereas with Lagrange <sup>the</sup> <sub>^</sub> "algebraization" of analysis. The former elaborated analysis via his generating functions and definite integral methods, the latter put forward his variational and algebraic operator calculus [1.2, 1.3, 1.5, 1.7]. Lagrange rejected emphatically geometrical intuitive elements in mechanics and Laplace followed on his lines. In different ways, both analysts established formalization in mathematics<sup>(1)</sup> and one of the most obvious cases where their complementary approaches become evident is in the demonstration and applications of Lagrange's theorem [1.5]. Laplace's work had a considerable impact on 19<sup>th</sup>-century English analysts; but it was above all Lagrange's algebraic calculus, as further developed by Arbogast and his followers [1.6], which influenced the development of symbolic methods in England.

In this section our aim is to discuss various aspects of the Lagrangian calculus in the wider philosophical and educational climate in France at the turn of the century. For this purpose we will touch upon the main epistemological issues of the work of Carnot, Condillac, Dégérando, Condorcet, Lacroix and Gergonne<sup>(2)</sup> for their prominent role in the development of the mathematical and physical sciences of that time. Despite the substantial differences in the work of these men, we perceive a common profound interest in algebra—as the language of philosophy and science par excellence—as well as in the so-called method of "analysis", the most characteristic method of inquiry of <sup>the</sup> <sub>^</sub> Enlightenment thought.

The principal figure under discussion is Condillac. The link between his semiotic philosophy and the mathematical theories of Lagrange and Laplace is a very subtle one. In fact, there is hardly any direct influence perceived between his philosophy and Lagrangian algebraic analysis of the late 18<sup>th</sup> century. However,



instances of the epistemology of Condillac and his followers had a considerable impact on the shaping of Arbogast's work and on the text-book writer Lacroix, as well as in the development of other branches of science. Moreover, these theories were not totally unfamiliar to English mathematicians, such as for example Babbage and De Morgan.

Our discussion will focus mainly on those epistemological issues that concern

1. The importance of algebra as a language,
2. The tendency towards universal methods and unity,
3. The form-matter distinction, and finally
4. The properties of signs and their influence in mathematical reasoning. These properties are associated with analogy, symmetry succession between ideas and power for invention.

In various forms, these issues are prominent in aspects of Lagrange's and Arbogast's calculus, as well as in the theories of the philosophers and mathematicians mentioned above. Borrowed from the French, these issues were gradually reformulated and emphasized by English analysts of the 1810's and 1830's, to be further applied successfully to the development of symbolical methods within mathematics and logic during the period 1840-1860. In what follows we will point out instances of these issues within French mathematics, philosophy and instruction of mathematics, hinting at their probable influence in the work of the main English mathematicians and logicians under study. In fact, apart from certain direct influences from the Lagrangian school, it is worth of attention to perceive interesting independent applications of these issues by the English well after the impact of both Lagrangian mathematics and Condillac's epistemology had faded away in France.

Our point of departure will be Carnot and his formulation of Lagrange's issue on the various degrees of indeterminateness which was to play an important role in De Morgan's work. One of the main concerns of late-18<sup>th</sup>-century mathematicians was the foundations of the calculus. The "hol y horror" of the infinitesimals motivated lengthy philosophical disputes and interesting inquiries<sup>(3)</sup>. As we saw in 1.5, Lagrange defined in 1772 the derivative of a function algebraically as the coefficient of the second term in the Taylor's series expansion [(15.5)-(15.6)]. His more extended treatment in his Théorie des fonctions

analytiques [1797] was on purpose devoid of limits or infinitesimals. Keenly interested in the metaphysics of the calculus, he proposed the problem of the infinite in mathematics to the Academy of Berlin for the prize contest in 1786. The winner was L'Huilier, but it is Carnot's paper which will concern us here<sup>(4)</sup>.

L. Carnot's paper was published as a book<sup>en</sup> titled Réflexions sur la métaphysique du calcul infinitesimal [1797]. This book saw an enlarged modified edition as [1813]<sup>(5)</sup> translated into English as [1832]. Carnot did not distinguish sharply between "analysis" and "synthesis". By convention, the former method assumed the solution known in unknown terms and was often linked with algebra, whereas the latter moved from the known to the unknown and was regarded as peculiar to geometry. According to Carnot, these two procedures were essentially the same; for, in principle, all analytic results could be obtained synthetically [Carnot 1813, 199; see also Daston 1986, 274; Gillispie 1971, 143].

Carnot had a wide knowledge of all the known methods of the calculus and showed a particular admiration for "l'analyse leibnitzienne" [1813, 205]. He compared ancient and modern methods of the calculus, holding that the latter, as founded upon algebra or upon infinitesimal algorithms, had immense advantages over the former. However, he argued that the principles of infinitesimal analysis were better established than those of algebra [1813, 201-202]. The main difference between those methods was the fact that by means of infinitesimal analysis certain auxiliary, non-sensical, "semi-arbitraires" quantities were eliminated during the course of its procedures, while this was not so with the method of algebra.

Carnot, contrary to Condillac, did not accept negative quantities. While the justification of negative or imaginary numbers will not be pursued in this thesis, let us see an interesting passage where arbitrary quantities are discussed. Referring to Lagrange, whom he admired a lot, Carnot wrote in [1832, 124-5]:

It has been remarked many times by this profound thinker, that the real secret of analysis consists in the art of making one's self acquainted with the different degrees of indetermination, of which the quantity is capable; an idea which always struck us as forcible, and which occasioned our regarding the method of

indeterminate quantities of Descartes as the most important corollary to the method of exhaustion.

In all branches of analysis taken generally, we observe that these operations are always founded on the different degrees of indetermination in the quantities compared by it. An abstract number is less determinate than a concrete, since the latter determines not only the quantity but the quality of the object submitted to the calculus. Algebraic quantities are more indeterminate than abstract numbers, because they do not specify the quantity. Amongst these last, variables are more indeterminate than constants, since the latter are considered fixed for a longer period in the calculation. Infinitesimal quantities are more indeterminate than simple variables, since they still continue susceptible of change, even when it has already been agreed on to consider the others as fixed. Lastly, the variations are more indeterminate than the simple differentials, since the latter are restricted to varying according to a given law, instead of which the law according to which the others change is arbitrary. This gradation in the different degrees of indetermination is endless, and it is in this union of quantities more or less defined, more or less arbitrary, that is founded the fertile principle of the general method of indeterminate quantities, of which the infinitesimal Calculus is in truth but a fortunate application.

This passage, as in the French edition [1813, 207-9], was quoted with approval by Babbage [1827, 343 fn ]. Under the influence of Babbage's and Carnot's work on the etymology of mathematics, De Morgan delved into the issue of the degrees of indetermination of algebraic symbols in his [1836] and consequently applied it to his logic under the name of the form-matter distinction [see 2.9, 3.5, 6.4-6.7]..

The form-matter issue is also encompassed in Arbogast's method of separation of symbols of operation from those of quantity [1.5]. Applied by Brisson and Français to the solution of differential equations, Arbogast's method -partly justified by Servois and Sarrus [1.6]- was further developed by Herschel, Murphy, Gregory and Boole and was of considerable impact on the latter's philosophical and logical enquiries [chapters 2-8].

In fact, Arbogast was the first mathematician to discuss at length the influence of signs in mathematical thinking. He held

that the secret of analysis lies in the happy choice of signs which have to be simple and "caractéristiques de chose qu'ils doivent représenter" [1800, ii; 1.5]. On similar lines Laplace claimed that signs can serve as a source of invention: "La langue de l'Analyse, la plus parfaite de toutes, étant par elle-même un puissant instrument de decouvertes, se notations, lorsqu'elles sont necessaires et heureusement imaginées, sont les germes de nouveaux calculs" [1811, 360].

Following Lagrange's observation, both Laplace and Arbogast paid particular attention to the remarkable analogy between exponentiation and differentiation indices. Analogy, illustrated either by means of the generating functions or the separation of symbols, was a powerful tool for generalization [1.5-1.6]. Happy notation, clearness of signs, brevity of computation and inductive generalization via analogy were in the core of French "semiotics" which flourished in Condillac's work in the late 18<sup>th</sup> century. Lagrange's algebraic calculus found a congenial climate in Condillac's theories. Together with Laplace, both analysts paid much respect to Condillac in the Séances of the Ecole Normale in 1795. At that time all students of this school were given a copy of Condillac's Logique [1780], published shortly after his death. While Lagrange's work was shaped well before his acquaintance with Condillac's work, that of Arbogast's shows obvious traces of the semiotic import of Logique<sup>(6)</sup>. In the rest of this section we will study the main issues of Condillac's work, its impact as well as its critical reception.

Following Locke's empirical philosophy, Condillac put forward his own position according to which our senses are the main source for knowledge<sup>(7)</sup>. The highlights of his so-called later "genetic epistemology" are evident above all in his Logique and in his unfinished La langue des calculs which was posthumously published in 1798. The former book is totally devoid of any examples from Aristotelian logic, as he was a fervent enemy of traditional syllogistic<sup>(8)</sup>. The book is divided in two parts. In the first part Condillac illustrated with simple examples how nature teaches us the method of analysis. According to this method, we acquire knowledge by tracing our ideas back to their origin, observing their generation and finally by comparing them under all possible relations. All ideas are of individual things at first, for example a particular tree. Gradually, a child can dis-

tinguish between kinds of trees and hence, via analysis, we approach the concept of classification [1980, 87-112].

In the second part of his Logique, drawing on Port-Royal logic<sup>(9)</sup>, Condillac proceeded to show how analysis is linked with the issue of elucidation of language. In brief, he held that every language is a tool of analysis, and the more simple and perfect the language, the more precise our reasoning. He argued that there is a unique method of analysis, equally applicable to logic, metaphysics and mathematics, which leads "from the known to the unknown by means of reasoning, that is, by a series of judgments which are contained one within the other" [1980, 286-7]. With an example of simple calculation, first translated in ordinary language and finally reduced to that of algebra [1980, 287-297], he was led to the conclusion that algebra is a well-made language par excellence<sup>(10)</sup>. In [1980, 305] he wrote:

Algebra is, in fact, an analytic method: but it is no less a language for that, if all languages are themselves analytic methods. Now the fact is, I repeat, that they are analytic methods. But algebra is a very striking proof that the progress of the sciences depends solely upon the progress of their languages; and that well-made languages alone could give to analysis the degree of simplicity and precision of which it is capable in each area of our studies.

Well-made languages could do this, I say: for in the art of reasoning as in the art of calculating, everything is reduced to compositions and decompositions; and it must not be thought that these are two different arts.

Condillac observed that among mathematicians, who as a rule make use of the method of synthesis starting from abstract principles and general definitions, there are some rare exceptions, such as Euler and Lagrange who make use of analytic methods. These analysts, he wrote, "write superb algebra-the language in which good writers are the rarest because it is the best-made language of all" [1980, 279]. At this point I would like to add that Condillac's natural way of discovery had influenced Euler's presentation of his material, particularly in his Institutionum Calculi Differentialis, published in 1755, which, according to Dhombres, is built according to a "Condillac pattern"<sup>(11)</sup>. Euler's own way of exposing his material from simple, particular

cases to more general ones was immensely admired by Babbage [2.9] but criticized by Herschel [2.9, (3) ].

But, despite his apparent knowledge of the work of Euler and Lagrange, Condillac's mathematical knowledge and ability was inadequate. His method of natural generalization, successful in presenting elementary arithmetic and algebra in his La langue de calculs, failed to explain the difficulties involved in more complicated problems, such as: the logarithm of a complex number the use of discontinuous functions or the solution of equations of degree higher than 4. According to Dhombres, La langue was not finished for good reasons, for Condillac must have felt the inadequacy of his theory<sup>(12)</sup>. His poor mathematical knowledge was to be particularly criticized by Condorcet, as we shall see below.

However, this book had a considerable educational and semiotic import. Condillac presented a philosophy of elementary algebra focusing on the clarity and certainty of formal algebraic theories. Above all he tried to show that his analogical derivation from finger-language to words and from words to numerals and letters should be the model for the formation of all languages which would thus become precise instruments of reasoning [1948, 419-421, 426-31, 464-69]. The issues of analogy and of clarity of signs he put forward are evident in the following citations: "Un mot devient naturellement le signe d'une idée, lorsque cette idée est analogue à la première qu'il a signifiée, et alors on dit qu'il est employé par extension" [1948, 428]. Moreover, at page 468 we read:

Or si les opérations, quand on calcule, se font sur les idées, ce sera dans l'analogie des idées mêmes qu'il faudra chercher les méthodes: au contraire, si faudra chercher les méthodes dans l'analogie des signes, si c'est sur les signes que se font les opérations. [.] Certainement calculer c'est raisonner, et raisonner c'est calculer: si ce sont-là deux noms, ce ne sont pas deux opérations. Avec des signes algébriques, le calcul et le raisonnement ne demandent presque point de mémoire: les signes sont sous les yeux, l'esprit conduit la plume, et la solution se trouve mécaniquement.

These are instances where we see some of the issues that characterised the semiotic philosophy of Condillac<sup>(13)</sup>, which was further developed by the *Idéologues*. But before we proceed to his

followers, a few words on Condillac's pedagogical issues. He ardently believed that a child or student should deal first with particular cases and acquire the mechanical habit of calculating and reasoning in ordinary algebra well before he is confronted with general definitions and complicated principles<sup>(14)</sup> [As we shall see in 3.4 his theory had a strong appeal to De Morgan who would recommend La langue to first-year students in 1831]. The same issue, evident (as mentioned above) also in Euler, was to be followed by Lacroix. Most probably independently of either Condillac or Lacroix, a similar fashion for the presentation of scientific material characterized Boole, particularly evident in his [1859] [see 8.8].

Condillac's theories were widely applied in chemistry, crystallography, anatomy, the natural sciences, as well as to the political and social sciences<sup>(15)</sup>. Successful notation and the tendency for classification via mathematization were among his most important issues closely linked with the method of analysis and the construction of a well-made language. Destutt de Tracy founded in 1796 the "Idéologie" after Condillac's semiotic philosophy. Following Condillac, he rejected Aristotelian syllogistic and for this reason his work on "logique" was regarded by early English logicians, such as Kirwan, Whately and G. Bentham as of no practical import [6.2-6.3]<sup>(16)</sup>.

Among Tracy's followers, called often as "Idéologues" we notice Laromiguière, Garat, Cabanis, Dégérando and others<sup>(17)</sup>. The latter discussed amply the question of the nature and epistemological function of signs in his four-volume book Des signes et de l'art de penser [1800], written after Tracy's prize contest on the issues of Idéology. Dégérando stressed among the advantages of algebraic notation its brevity and rapidity in reasoning for, this has as a result the close succession between two ideas in the mind as represented by the algebraic signs. He wrote :

La rapidité d'une opération intellectuelle est toujours en raison inverse des efforts qu'on demande à l'attention et à la mémoire. Cette opération qui consiste à fixer les rapports de ses idées pour leur appliquer les mêmes jugements s'exécutera donc autant plus promptement qu'il nous sera facile de nous rappeler et de remarquer ces rapports<sup>(18)</sup>.

This passage, following Dégérando's comparison of decimal arith-

metic with the numerical Roman system, was to be quoted with admiration by Babbage in his [1827, 332; see also 2.9].

But nowhere is Condillac's classification into method revealed with more success than in the work of Condorcet. Condorcet, while considerably influenced by Condillac's theories, was one of his most bitter critics. A much better mathematician than Condillac -though inferior to Lagrange and held in contempt by him- Condorcet followed a different direction. He contributed in differential equations, probability theory and, mathematicizing the political and social sciences, he introduced a new branch of science, that of "social mathematics".

Condorcet's own epistemology is above all evident in his Tableau historique, published in 1794, the year of his suicide. Firmly believing that the historical task of the 18<sup>th</sup> century was to codify the scientific method, he introduced a decimal system for the numerical classification of science. Diverging from Condillac's method of analysis -which he severally criticized- he chose the combinatorial analysis of mathematics as the essential model for scientific reasoning. He maintained that there is no real difference between the analytic method of algebra and the synthetic method of geometry [Baker 1975, 115-124]. However, his work was strongly algebraic and in 1789 he dismissed classical geometry as an intellectual antique which had been "abandoned by almost all mathematicians in favor of modern analytic methods" [Daston 1986, 270].

Beside his method of classification, another of Condorcet's novelties was his project of a universal language of the sciences. In both these projects he is compared with Leibniz who had introduced a century ago his "arte combinatoria" and "*Characteristica universalis*"<sup>(19)</sup>. Fragments of Condorcet's latter project are included in his Tableau. His unpublished manuscript essay is edited by Granger together with a commentary [1954]. In this essay Condorcet set out a kind of symbolic logic appropriate to a universal language; algebra was to feature prominently in his plan. "Je commencerai", he wrote in the introduction, "par la science pour laquelle il est le plus aisé de former une langue universelle, par l'algebre" [Granger 1954, 204]. His project had not any impact. However, the pursuit of general methods in mathematics and logic, as based upon the formal language of differential -operator algebras, was to be the project par excellence



for Boole [chaps 4, 7, 8].

There is another interesting link to be noticed: between Condorcet's probability theory and that of Laplace. While discussing the poorly studied historically connections between logic(s) and probability theory, Grattan-Guinness wrote: "With Condorcet and Laplace there was a connection of a semiotic kind; that just as logique emphasized the importance of signs in developing idéologie, so probability values indicated the strength or weakness of the signs of evidence concerning the structure of the (physical or social) world (or, in the subjective interpretation of probability, of our degrees of belief about that structure). It is worth noting that the students at the Ecole Normale in 1795, armed with the obligatory copy of Condillac's Logique, heard Laplace discuss questions of probability in the last of his lectures [...], a talk which in the 1810's was to blossom into his influential Essai philosophique sur les probabilités, [1988b, 75-6] (20).

Condorcet played an important role in the educational programs of Parisian institutions and universities at the turn of the century [Hodgkin 1981, 59]. The work of S.F.Lacroix was largely inspired from the epistemological theories of both Condorcet and Condillac. Lacroix published his 3-volume Traité du calcul in 1797-1800. He had started gathering material in 1787, and during these years he was substantially encouraged by Laplace. A disciple of Condorcet, Lacroix presented in his work all the prevailing 18<sup>th</sup>-century Continental methods of the calculus. Including derived functions, differentials and limits, he summarized all the major work by leading mathematicians such as Lagrange, Laplace, Legendre, Euler and D'Alembert. For the educational program suggested by Condorcet and followed by Lacroix paid emphasis on the construction of elementary textbooks which would enable students to proceed to the study of advanced treatises and research papers (21).

Lacroix's Traité du calcul saw an enlarged edition in 1810-1819. Now, the third volume would include instances of the work of Arbogast, Français and Servois on differential operator calculus. At the same time D'Alembert's limits would feature more prominently than in the first edition. Though specific references are missing, a rich bibliography is to be found in his Traité. Despite the plurality of theories offered, one could say that

Lagrange's and Laplace's methods were those to which he paid the most attention. In 1802 a Traité élémentaire was published on the calculus. It was its second edition in 1806 which was translated by Babbage, Herschel and Peacock in 1816. The English were to criticize Lacroix for including D'Alembert's limit-based method and as a result the translation was enriched—mainly by Herschel—with instances of Lagrange's algebraic calculus. Nevertheless, Lacroix's big Traité—particularly volume 3—formed the main background in French mathematics for Babbage and Herschel in 1813<sup>(22)</sup>. According to Grabiner, Lacroix's Traité played a considerable role in the shaping of Cauchy's work [1981b, 79–80].

Lacroix's educational and epistemological concerns are best illustrated in his influential Essais sur l'enseignement, first published in 1805, with a third enlarged edition in 1828<sup>(23)</sup>. Condorcet's tableau for classification is acknowledged [1828, 20] and Condillac's method of analysis and its applications in the development of sciences stressed [1828, 22–24, 149–157]. The encyclopedistic tradition of the *Ideologues* is apparent when he writes that "le rapprochement des divers sciences est le moyen le plus propre a decouvrir la methode generale qui doit diriger l'esprit humain dans la recherche de la verite" [1828, 24]. Independently from Lacroix, the Leibnizian issue of the "comparative science"—as encapsulated in the above quotation—will be further discussed in connection with Boole and Gräy [see 8.9].

Lacroix observed that the method of analysis can be traced back to the origins of geometry. It is wrong, he held, to regard that "l'Algebre constitue exclusivement l'analyse". Algebra can be also used in synthetic type of demonstrations since it is "qu'une écriture abrégée et régulière, par le moyen de laquelle on represente toutes les relations que les grandeurs peuvent avoir entre elles" [1828, 204–5]. From this and other passages it is evident that like Carnot and Condorcet, Lacroix did not distinguish sharply between analysis and synthesis. He said, in fact, that the main import of what came to be called "methode analytique" is distinguished in the exposition of scientific discoveries [1828, 213].

Slightly critical towards Condillac he wrote that the latter's aim to show in his Logique that algebra is but a language was not far from Clairaut's lucid and precise notations in

1748 is his Elémens d'Algebre [1828, 205]<sup>(24)</sup>. Examining Logique, Lacroix remarked that he had traced in it a synthetic approach. And further, drawing on Maclaurin's work on the attraction of spheroids, he concluded that synthesis leads at times to a result in a more simple manner than analysis does [1828, 217]: The progress of the metaphysics of Locke and Condillac, claimed Lacroix, was not due to the analytic method but "parce qu'ils ont puise leurs premières notions dans la nature, et non pas dans leur imagination: c'est parce qu'ils sont remontés à la véritable origine des connaissances au lieu d'en créer une à leur facon" [1828, 217-8].

Association of ideas, abstraction, generalization, classification, simplicity of principles were all mathematical issues which were discussed amply in his Essais [1828, 223-7, 305]. After Laplace, he suggested that we always choose "les méthodes les plus générales" [1828, 178]. The pedagogical import of Lacroix's work was mostly influential in the early shaping of De Morgan's views on the instruction of mathematics; together with La langue, the Essais was recommended to both teachers and students of mathematics [3.4].

But the most severe critic of "la secte Condillacienne" was Gergonne. In 1813 he wrote an essay on the "méthodes de la synthese et de l'analyse mathématique" for the prize contest of the Academy of Bordeaux. The essay -extracts of which are included in [Dahan 1986]-was rejected because its author himself had rejected the distinction between these two methods [Dahan 1986, 97-103]. Gergonne's views on mathematics -as in this polemic essay- are particularly interesting as an epilogue to this section -and in fact to the whole chapter- as they coincide with those of the English analysts under study.

In the educational domain it was above all in the instruction of algebra, noticed Gergonne, that Condillac's influence was mostly irritating. He rejected Lacroix's style of introducing in his Eléments d'Algebre in 1799 theoretical explanations only at the moment they are needed interrupting "sans cesse le raisonnement par des recherches incidentes" and rendering the material difficult for the reader [Dahan 1986, 121-2]. A very similar criticism is noticed in Herschel's manuscripts for Euler's work [see 2.9, (3)].

On lines similar to those in Lacroix's Essais, Gergonne

argued against Condillac's sharp distinction between analysis and synthesis and claimed that neither <sup>is</sup> human language imperfect, nor definitions useless. He added that "De même que les symboles algébriques ne constituent pas seuls l'algèbre, je ne pense pas non plus que l'on puisse dire avec Condillac que l'art<sup>de</sup> de raisonner se réduit à une langue bien faite" [Dahan 1986, 109]. Mathematics was for Gergonne the "seul modèle paradigmatic" of science. An admirer of Lagrange, he regarded him as responsible for the perfection of mathematical notation and of rendering algebra "une langue toute nouvelle" [Dahan 1986, 105, 113].

Gergonne stressed particularly the analogy between indices of exponentiation and differentiation prominent in Lagrange's theorem (15.1). He wrote

Le jeu des accents et des indices a en effet de lui-même fait découvrir par une sorte d'intuition, une multitude d'importantes vérités qui peut-être seraient demeurées éternellement cachées, pour la plupart, sans leur utile secours, et rien ne me semble plus propre à montrer toute l'influence des signes sur les idées.

This passage is quoted from [Dahan 1986, 113-14]. Its last phrase, noted Dahan, is the key for understanding Gergonne's reasoning. By "signe" he also means "form" as opposed to "matter".

It was in fact in Gergonne's journal Annales that the formalistic algebraic theories of Lagrange and Arbogast flourished in the works of Français, Servois, Sarrus and others in the years 1810's and 1820's [1.6]. Gergonne himself contributed several articles in his journal, some of them in the philosophy of mathematics<sup>(25)</sup>. Worthy of attention is also the fact that he translated part of Babbage's work on functional equations in his [1822; see 2.5.(1)]. Nowadays he is best remembered for his enquiries in the logic of the Euler diagrams and his implicit definitions in the late 1810's<sup>(26)</sup>.

Summing up, with Gergonne we have in the 1810's and early 1820's that it was the algebraic language of differential and finite difference operators that constituted the language par excellence in advanced French mathematics and not the vaguely illustrated language of algebra by Condillac and his followers in late 18<sup>th</sup> century. The etymology of mathematics and the foundation of the calculus was to move in another direction in the

1820's with Cauchy<sup>(27)</sup>. However, as we stressed in 1.1 and 1.7, the English were reluctant to follow Cauchy and focused solely on aspects of Lagrangian algebras. Far from adopting the encyclopedistic tendency of the Enlightenment period, and only slightly under the educational theories of Condillac, the English would advocate the formalistic issues of Lagrange, Arbogast and (implicitly) Gergonne applying them to the domain of analysis and logic.

## Chapter 2

### Herschel and Babbage on the calculi of operations and functions : 1812-1822.

#### 2.1 Introduction.

After a long period of isolation, various reforms took place in British universities and institutions in an attempt to deploy Continental calculus and mechanics. The main stimuli for the reformers came from Laplace's Mécanique and Lagrange's operator calculus [1.3, 1.5]. In section 2.2 we will discuss briefly the contributions of Woodhouse, Playfair, Wallace, Spence, Ivory and Brinkley in the 1800's and the 1810's. Particular emphasis will be paid on the latter's demonstration of Lagrange's theorem in 1807.

Woodhouse, together with the members of the Analytical Society, were the main precursors of the reform at Cambridge University around 1816. The Analytical Society, formed mainly by the undergraduates Herschel, Babbage and Peacock in 1812, lasted for hardly two years. The main outcome of this Society was a single volume of its Memoirs published in 1813. Herschel and Babbage were in fact the only contributors to this journal<sup>(1)</sup>. Together with Peacock they also translated the abridged textbook of Lacroix on the calculus in 1816 [1.8] which they enriched with a collection of Examples in 1820. By means of these publications Continental notation and Lagrangian algebraic calculus were gradually adopted in the University in the late 1810's<sup>(2)</sup>. These two textbooks were particularly useful for certain Cambridge analysts up to the 1840's [chapters 3,4].

During the period 1813-1822 Herschel and Babbage contributed mathematical papers to the Philosophical Transactions of the Royal Society. In 2.3 we will focus on Herschel's work on the calculus of operations around 1813-1814, particularly on his theorem for the development of exponential functions of the form  $f(e^t)$ . In 2.4 we will discuss Babbage's and Herschel's early work on functional equations. Aware by 1813 of the work of Lagrange, Monge and Laplace on this branch [1.4], the two analysts collaborated fervently and applied mainly Laplace's method which they tried to generalize with little success. Their frequent

correspondence during the period 1812-1817 will shed further light on their work.

In 1814 Babbage set out his own approach towards functional equations. Despite the originality of his general procedures, he was substantially inspired and helped by his friends, Herschel, Bromhead and W.H. Maule; a subtle influence from Monge is also apparent in his work. Babbage's work consists mainly of two lengthy papers [1815] and [1816] devoted to equations in one variable and two variables respectively. Drawing mainly on these two papers, we will study representative cases of first-order functional equations in one variable in 2.5 and of equations of order higher than one in 2.6. Section 2.7 will cover his paper on analogy between the calculus of functions and other branches of analysis published in 1817.

Peacock did not participate in the interest of Babbage and Herschel in Lagrangian algebras. His work on the principles of algebra, published in 1830, will be briefly discussed in chapter 3 in connection with De Morgan's mathematical work. In the late 1810's both Babbage and Herschel were discouraged to continue their mathematical research which sounded too abstract and impractical to most of their contemporaries [Enros 1979, 182, 209]. However, we notice Herschel's work on circulating functions [1818] and functional equations [1822], which will be discussed in 2.8, as well as Babbage's papers on functional and algebraic notation, [1822] and [1827], with which we will conclude our study in 2.9.

Through our discussion in the following sections we will notice the genesis of Herschel's and Babbage's theories on the operational and functional calculi as well as their keen interest in the establishment of successful notation. Drawing on Lagrange, Arbogast and Laplace, these two analysts worked in different ways. For Herschel the basis was finite-difference equations and expansions in series, whereas for Babbage functional methods based on analogy from algebra. However, a tendency for abstraction and generalization in their procedures and for brevity and symmetry in notation was to characterize the work of both of them. A semiotic influence is only slightly evident in Babbage [1827]; it is above all the Lagrangian influence which is prominent in their work together with a considerable Laplacian influence, particularly evident in Herschel's work.

## 2.2 Reforms in early-19<sup>th</sup> century Britain; Brinkley on Lagrange's theorem.

In England some minor reforms took place at the Royal Military College at Great Marlow and the Royal Military Academy in Woolwich<sup>(1)</sup>. Of more significance, however, was the reform at Cambridge University whose first precursor was R. Woodhouse. Via his Principles of analytical calculations [1803], Lagrange's and Arbogast's calculi were introduced in a critical manner. This book, read by the members of the Analytical Society, called attention to problems of convergence for the first time in England<sup>(2)</sup>.

However, more influential was Woodhouse's Trigonometry [1809] which circulated much more widely than his [1803]<sup>(3)</sup>. Later this book, [1809], was acknowledged as having contributed more than any other work "in revolutionizing the mathematical studies in England" [Peacock 1833, 295]. But Peacock's remark was certainly an exaggeration for other contributions had a similar effect to those by Woodhouse [Panteki 1987, 120-6]. In fact Woodhouse's own work extended further than his [1803] and his [1809]. He wrote a short history on the calculus of variations [1810], characterized as rather "confused" and "unfit" for students [Airy 1826, vii], but, nonetheless significant for the evolution of the subject [Jellet 1850, xix-xx].

Woodhouse's contributions in the diffusion and instruction of physical astronomy are also worth of notice. His treatment of the problem of the three bodies in his [1818] was particularly praised by Airy [1826, iv]. In this treatise he tackled thoroughly the solution of the differential equation

$$(22.1) \quad \frac{d^2u}{dx^2} + k^2u + \Pi = 0,$$

where  $\Pi$  is a constant or a suitable function of  $x$ , important in many problems of astronomy including that of the three bodies and that of the tides [see (14.9)]. Equation (22.1) was solved by the method of variations of constants and was reduced to the simple form

$$(22.2) \quad \frac{d^2s}{dv^2} + s = 0$$



mentioned before as (14.10) [1818, 23, 92-105]. Woodhouse followed Laplace's procedures in very few cases, trying to render his textbook comprehensible by students. There is no trace, for example, of the earth-figure or the Laplace equations in his book. However, he would refer very often in a footnote to Laplace's procedures in every case that an alternative, easier method was used<sup>(4)</sup>.

Among the Scottish mathematicians who contributed in the diffusion of French mathematics and physical astronomy in their country, we notice Playfair, Wallace, Spence and Ivory. In fact, the Scottish had not been isolated from the Continent in the same degree as the English. As early as 1790, J. Playfair called attention at Laplace's and Lagrange's work in his paper "On the astronomy of the Brahmin". Of most significance was, however, his review of Laplace's Mécanique in 1809, mentioned with admiration in the "Preface" to the Memoirs [1813, ii]. This review was to have important impact on Ivory's original researches in the 1810's which we shall mention below [Enros 1981, 136, 140; Guicciardini 1989, 102-3].

Of great importance were also the translations of J. Toplis, a schoolmaster from Nottinghamshire. In 1804 he translated Lacroix's Essais [1.8; Grattan-Guinness 1990, art. 3.2.5] and in 1814 the first two books of Laplace's Mécanique [Enros 1981, 136, 140; Guicciardini 1989, 116-118]. Toplis rightly stressed in the preface of his latter translation that "...obstacles in both Mécanique Céleste and Mécanique Analytique principally arise from the difficulty of integrating the equations of which their authors made use" [Laplace 1814, iii-vii].

The astronomer W. Wallace, faithful to the Scottish tradition, was mainly interested in geometrical problems. He also possessed a remarkable analytical skill evident in his [1805] on the rectification of the ellipse. This work drew on Ivory's respective paper [1798]. However, it was admired for its originality by Brinkley [1803, 148], Woodhouse [1804, 229-30, 231, 244-5] and Herschel [1820, 59-61]. Wallace contributed substantially in the diffusion of Continental mathematics by translating memoirs of Lagrange and Legendre for the Mathematical Repository and by composing numerous articles for Encyclopedia Britannica and the Edinburgh Encyclopedia. Of most importance was his "Fluxions" in the latter Encyclopedia in 1815 partly devoted

to the differential calculus. Moreover, together with Ivory they introduced the differential notation in the late 1800's in the Repository. Realizing that his contributions had been passed over in Peacock's Report in 1833, Wallace felt deeply disappointed<sup>(5)</sup>.

Another interesting figure is W. Spence for his work on finite difference, algebraic and functional equations. But above all, of most impact was his work on logarithmic transcendents, which were defined by

$$(22.3) \quad \overset{n}{L}(1 \pm x) = \pm \frac{x}{1^n} - \frac{x^2}{2^n} \pm \frac{x^3}{3^n} - \dots$$

and which satisfied the property

$$(22.4) \quad \overset{n}{L}(1 \pm x) = \int_0^x \frac{\overset{n-1}{L}(1 \pm x)}{x} dx \quad \text{paper}$$

[Spence 1809, 1-4]. Spence developed in his <sub>A</sub>[1809] a theory of these transcendents and their properties and then applied his results to the integration of differential expressions and to the summation of series. In the same paper he solved simple functional equations, such as

$$(22.5) \quad \varphi(1+x) + \varphi(1+y) = \varphi(1+x+y+xy),$$

by Lagrange's method [1.4, (9)]. That is, he developed each function in series of ascending powers of  $x$ ,  $y$  and then determined  $\varphi$  by comparing the terms on the left and right-hand side of (22.5) [1809, iv-ix].

Spence's essay [1809] is described by Guicciardini as "a bold attempt for an unknown and provincial mathematician to break with the fluxional tradition and to employ the newish symbolical techniques of the Lagrangian school" [1989, 106]. This statement is true, but in later writings Spence showed an ambivalence between fluxional and differential notation. We quote from a paper on algebraical and differential equations of 1814:

....betwixt the two notations of Newton and Leibniz [....] we are left to choose, and having no prepossession in favour of either of them, it may be remarked that the system adopted by Leibniz is more distinct capable of diversified applications, and more easily written than that of Newton. This, however, is merely stated as opinion<sup>(6)</sup>.

Spence left many unfinished essays which Herschel dis-

covered in 1814. Above all Herschel was attracted by Spence's work on logarithmic transcendents which he extended in his [1814] [see 2.3]. Babbage, on the other hand, admired Spence's method for the solution of finite difference equations and his way to resolve a complicated expression in separate parts [1827, 333-4]. Herschel edited Spence's Mathematical essays in 1820 (2.8).

The last Scottish mathematician we will mention is J. Ivory. A very close friend of Wallace, Ivory worked mainly in England. He is mostly known though for his original research in physical astronomy, acknowledged by Laplace and Poisson [Anon. 1842, 409]. His first paper on the attraction of ellipsoids, [1809], containing a theorem later named after him, is regarded as the most important of the series [Todhunter 1873b, 222]. Ivory includes in his early papers a comprehensive historical account of the known researches in potential theory. In his [1812a, 23] he defined the conditions so that Laplace's theorem (13.8) holds true [Todhunter 1873b, 257]. Another interesting result was the deduction of the equation

$$(22.6) \quad (n-m)(n+m+1)(1-y^2) \frac{d^m U_n}{dy^m} + \frac{d \left[ (1-y^2)^{m+1} \frac{d^{m+1} U_n}{dy^{m+1}} \right]}{dy} = 0,$$

where  $U_n$  are as in (13.5)-(13.6) [1812b, 55-6]. Equation (22.6), closely related to that of the "Laplace coefficients", was named after Ivory by Whewell [1830, 146].

Ivory was keenly interested in the expansion of  $(e^x-1)^n$ , for  $n>0$ , in powers of  $x$ . In the course of the demonstration of

$$(22.7) \quad n^m - n(n-1)^m + \frac{n(n-1)}{2} (n-2)^m - \dots + (-1)^{n-1} n = 0$$

for  $m < n$ , he determined, by means of the binomial theorem, the coefficient  $A_m$  of  $x^m$  in the expansion of  $(e^x-1)^n$  to be

$$(22.8) \quad A_m = \frac{1}{m!} [n^m - n(n-1)^m + \dots + (-1)^{n-1} n], \quad m > n$$

[1806, 127-9] (?). Expansions of exponential functions were needed in the treatment of problems of interpolation, summation of series and definite integrals in physical astronomy. Laplace dealt satisfactorily only with particular cases [1.5]. But the first systematic approach was taken by Brinkley in 1807, followed by Herschel in 1814.

A Cambridge graduate, J. Brinkley became professor of astronomy at Dublin University in 1790 . He and B. Lloyd brought in a reform in the early 1810's<sup>(8)</sup>. Under the influence of Laplace, Brinkley produced various papers and text-books on physical astronomy, such as his [1803 a, b], [1813] and [1820]. He also produced considerable work on analysis. In his [1800] he introduced his method of "fluxions per saltum" by means of which one can readily determine the  $n^{\text{th}}$  fluxion of a given function. In 1803 he claimed that this method was discovered independently from Arbogast's calculus of derivations with which it resembled. Regarding in fact his own method as superior to that of derivations, he announced his intention to incorporate it in a book together with its applications [1803a, 84, fn].

But Brinkley delayed the publication of the book -written in his own peculiar notation- unwilling "to offer a fluxional notation different from either that of Newton or Leibniz, each of which is very inconvenient as far as regards the application of the theorems for finding fluxions "per saltum" " [1807, 120]. It was in fact only in 1818 that he decided to switch to the differential notation; however, the book was apparently never completed [see his 1820, 40]<sup>(9)</sup>.

Lagrange's and Laplace's influence blend together in his paper on " An investigation of the general term of an important series in the inverse method of finite differences" [1807]. This "important series " is no other than the expansion of  $(e^t-1)^n$ , where  $t=hd/dx$ , when  $n<0$ . Brinkley introduced first Lagrange's theorem in fluxional notation, then denoted the  $n^{\text{th}}$  differences of zero by  $\Delta^n 0^m$  and next proceeded to prove Lagrange's theorem.

We will present Brinkley's procedure as slightly modified by Herschel in his Notes in Lacroix [1816], appending the original proof to an endnote [see (10) below]. The proof of Lagrange's theorem depends upon Taylor's theorem, written by Herschel in the symbolical form

$$(22.9) \quad u(x+n) = e^{n \frac{d}{dx}} u(x),$$

and the formula

$$(22.10) \quad \Delta^n u(x) = u(x+n) - \frac{n}{1} u(x+n-1) + \dots + n(-1)^{n-1} u(x+1) + (-1)^n u(x)$$

easily derived from the definition

$$(22.11) \quad \Delta u(x) = u(x+h) - u(x).$$

Herschel for simplicity assumed  $h=1$  in (22.11) and it is for this reason that  $h$  is missing in the right-hand side of (22.9). Putting  $n=n-1, n-2, \dots, 0$  in (22.9) we obtain the values of  $u(x+n)$  which we substitute in (22.10). Then, by separating the symbols of operation from those of quantity it follows from (22.10) after the substitutions that

$$(22.12) \quad \Delta^n u(x) = \left(e^{\frac{d}{dx}} - 1\right)^n u(x)$$

[Lacroix 1816, 487-8] (10).

For the proof of the expansion formula of the right-hand side of (22.12), Brinkley took for granted the properties of the  $\Delta^n 0^m$  numbers defined as the  $n^{\text{th}}$  differences of the series  $0^m, 1^m, 2^m \dots$  [1807, 116]. From (22.11) it follows that  $\Delta^n x^m = 0$ , for  $m < n$ , and  $\Delta^n x^n = n!$ . Hence

$$(22.13) \quad \Delta^n 0^n = n! \text{ and } \Delta^{n+m} 0^n = 0, \quad m > 0.$$

The value of  $\Delta^n 0^m$  for  $m > n$  is deduced from (22.10) when  $u(x) = x^m$  and  $x$  is replaced by 0 after the expansion. Thus,

$$(22.14) \quad \Delta^n 0^m = n^m - \frac{n}{1}(n-1)^m + \frac{n(n-1)}{1.2}(n-2)^m - \dots + (-1)^{n-1}n.$$

Based upon the properties of  $\Delta^n 0^m$  and Lagrange's theorem, where  $\Delta u(x)$  is given by (22.11) with  $h=1$ , Brinkley proved the formula

$$(22.15) \quad \Delta^n u(x) = h^n \frac{d^n u(x)}{dx^n} + h^{n+1} \frac{\Delta^n 0^{n+1}}{(n+1)!} \frac{d^{n+1} u(x)}{dx^{n+1}} + \dots$$

This formula is the same as Lagrange's (15.12) and Laplace's (15.20). But Brinkley has now directly determined the latter's coefficients  $q, q_1, \dots$  by means of his differences of zero. In fact, the  $m^{\text{th}}$  coefficient in the expansion of the right-hand of (22.15) is  $\Delta^n 0^m / m!$ , or, due to (22.14), this coefficient is identical with  $A_m$  defined in (22.8) [1807, 118].

Brinkley's step from (22.12) to (22.15) was essential in the development of the calculus of operations. His procedure, via the  $\Delta^n 0^m$  numbers, renders redundant the definition of the coefficients of various expansions by means of complicated recursive relations. Let us regard  $d/dx=t$  in (22.12). Then, by means of (22.15) we have

$$(22.16) \quad (e^t - 1)^n = \Delta^n 0 + \frac{\Delta^n 0^1}{1} t + \frac{\Delta^n 0^2}{2!} t^2 + \dots$$

for  $n > 0$  [Herschel 1820, 80]. Brinkley set off next to provide the expansion of the right-hand side of (22.12) for  $n < 0$ . His procedure, based upon his "fluxions per saltum", is quite lengthy and complicated. We will present, though, his main result. If we expand  $t^n(e^t - 1)^{-n}$ , where  $t$  stands for  $h\delta/dx$ , or, in Brinkley's notation, for  $h\delta/\dot{x}$ , and where  $n > 0$ , then the coefficient  $A_x$  of  $t^x$  is found to be

$$(22.17) \quad A_x = -n \frac{(n+2) \dots (n+x)}{(x-1)!} \frac{1}{1} \frac{\Delta^{10} 0^{x+1}}{(x+1)!} + n(n+1) \frac{(n+3) \dots (n+x)}{(x-2)!} \frac{1}{1 \cdot 2} \frac{\Delta^{20} 0^{x+2}}{(x+2)!} + \dots$$

[1807, 125-127].

Brinkley acknowledged Laplace's "ingenious" technique for the development of  $t^n(e^t - 1)^{-n}$ , for  $n=1$  [1807, 129]. Only now, by means of (22.17) Laplace's formula (15.28) is readily obtained without need to depend upon a recursive definition of its coefficients. The paper ended with further illustrations of the facilities offered by the differences of zero in the computation of expansions of finite differences [1807, 129-132].

Brinkley's elegant proof of Lagrange's theorem impressed Herschel who, commenting upon it, wrote to Babbage in 1813; "I do not think for clearness and shortness that this can easily be surpassed"<sup>(11)</sup>. Herschel improved considerably upon Brinkley's method and results. For example, he showed that (22.16) holds true also for  $n < 0$ ; thus

$$(22.18) \quad (e^t - 1)^n = \frac{\Delta^n 0^n}{n!} t^n + \frac{\Delta^n 0^{n+1}}{(n+1)!} t^{n+1} + \dots$$

[1820, 82]<sup>(12)</sup>. The  $\Delta^n 0^m$  numbers were to prove very useful, particularly in the determination of the Bernoulli numbers of different orders<sup>(13)</sup>. In the following section we will study Herschel's development of the calculus of operations, as mainly motivated by Laplace [1782], Arbogast [1800] and Brinkley [1807]. We will focus mainly on his own theorem introduced for the development of any function of the form  $f(e^t)$  from which (22.16) and (22.18) follow as special cases.

### 2.3 Herschel on the calculus of operations: 1813-1816; origins and applications of Herschel's theorem.

During the period 1813-1816 Herschel contributed in various aspects of the calculus of finite differences, his mathematical research topic par excellence. He studied trigonometric series, generating functions, Spence's logarithmic transcendents, Brinkley's formulae for expansion, the determination of the Bernoulli numbers, finite difference equations and functional equations. In this section we will focus mainly on his theorem for the development of exponential functions which was actually formulated and proved in his paper "On the development of exponential functions; together with several new theorems relating to finite differences" [1816].

In fact, the germs of Herschel's theorem for the expansion of  $f(e^x)$  appeared in his very first paper "On trigonometric series" [1813a] in which he enquired into the development of  $e^{e^t}$ . He considered further developments of exponential functions, other than  $e^{e^t}$ , in his paper on "Consideration of various points of analysis" [1814]. In this paper he introduced for the first time operator methods, on lines similar to those by Arbogast, as well as Laplace's generating functions. Certain instances of his work on the calculus of operations and on generating functions were appended at the end of Lacroix [1816]. Moreover, a revised version of the proof of his theorem on  $f(e^x)$ , together with a variety of its applications, were included four years later in his Examples [1820]. The consultation of these two books will be helpful in our study of his early papers.

Before we proceed to study Herschel's early work as published in the Memoirs [1813], let us comment upon the now rare journal published in a single volume by the Analytical Society. To the great disappointment of its authors, the Memoirs were not reviewed and were very unsuccessful in their circulation<sup>(1)</sup>. But this proved later to be rather an advantage than a misfortune, for, as we shall see, serious errors were spotted in it [2.4.(6)]. As a result, the papers of Babbage and Herschel which were published in this journal were not to be referred to <sup>in</sup> their later work<sup>(2)</sup>, and in fact the Memoirs remained basically unread up to our days<sup>(3)</sup>.

However, there was one English mathematician who was to go

through the obscure material of the Memoirs and to refer to it in his work : De Morgan. On the first page of his own copy of the Memoirs we read his prophetic inscription:

The time will come when this work will be sought after by the curious, as the earliest indication of the change which was taking place in English mathematics. I think it is all written by Herschel and Babbage: the preface by Herschel. No more was published under this name. Sept. 11, 1858<sup>(4)</sup>.

in fact

The "Preface" to the Memoirs, written<sub>A</sub> by Babbage in collaboration with Herschel, consists of a brief account of the state of 18<sup>th</sup>-century Continental analysis<sup>(5)</sup>. A preference for Lagrange's and Arbogast's algebraic approach is noticed, as well as an emphasis on notation, which, by its brevity and clarity, would enable the process of discovery. Moreover, the analogy between indices of repeated functional operations and those of exponentiation was stressed. The direction engraved in this journal was decisive in the development of functional and operator methods in mid-19<sup>th</sup>-century England<sup>(6)</sup>.

Another useful source of information on the evolution of Babbage's and Herschel's mathematical work is their voluminous correspondence<sup>(7)</sup>. During the period 1812-1816 at least one hundred letters were exchanged between these two analysts including information on the composition of the Memoirs<sup>(8)</sup>, the translation of Lacroix, the desire for a revival of the Society<sup>(9)</sup> and, above all, the difficulties they were confronted with in the course of their research [2.4-2.7]. These letters formed a most challenging ground for inspiration and creativity through mutual influence and assistance. It was Herschel who had insisted on the anonymity of the Memoirs, but he had objections to the anonymity of the Analytical Society as such and its aims<sup>(10)</sup>.

For both analysts the summation of trigonometric and logarithmic series was the first area for systematic investigation [Enros 1983, 34; 2.4]. Herschel opened his first paper with the property

$$(23.1) \quad f^m f^n(x) = f^{m+n}(x).$$

Considering  $f^0(x)=x$  as a necessary assumption, he introduced the reverse operation  $f^{-m}$  by putting in (23.1)  $n=-m$ . Thus

$$(23.2) \quad f^m f^{-m}(x) = x.$$

It was implicitly assumed that the value of  $f^{-1}$  is unique. Other



novel notations were introduced, such as "log<sup>-n</sup>x" which stood for

$$(23.3) \quad e^{e^{\dots^e}} \quad \cdot \quad e^{(e^e)}$$

For  $n=3$ , (23.3) denotes  $e^{(e^e)}$  [1813a, 33, 47; 1814, 441-2].

Among his concerns was the summation of the series  $u_n$ , given by

$$(23.4) \quad u_n = 1^n + \frac{2^n}{1!} + \frac{3^n}{2!} + \frac{4^n}{3!} + \dots$$

Three methods were suggested. The first two, based on the finite differences of  $u_n$ , led to formulae from which it followed that

$$(23.5) \quad u_1 = 2e, u_2 = 5e, u_3 = 15e, \text{ etc}$$

[1813a, 61-2]<sup>(11)</sup>. We are, however, interested in the third method by means of which the value of  $u_n$  was determined via the coefficients of the expansion of  $e^{e^t}$ . This idea had occurred to Herschel in August 1812. We shall briefly compare the method sketched in the letter, where it first appeared, with its later presentation in the Memoirs, for a curious error is to be noticed in the latter.

Herschel had initially used the series  $\frac{1^n}{1} + \frac{2^n}{1.2} + \frac{3^n}{1.2.3} + \dots$  or  $u_{n-1}$ . He wrote to Babbage: "I will now give you my summation of  $\frac{1^n}{1} + \frac{2^n}{1.2} + \frac{3^n}{1.2.3} + \dots$ , and if I find that this has been already done, I shall certainly hang myself. If you know of it, keep me in happy ignorance" [H.S., 20:2, 12 Aug. 1812]. The procedure in this letter runs as follows: Let  $e^{e^x} = A_0 + A_1x + \dots + A_nx^n$ .

Then, since  $e^{e^x} = 1 + \frac{e^x}{1} + \frac{e^{2x}}{1.2} + \dots$ ,

if we replace each  $e^{nx}$  by its development in powers of  $x$ , it is easily derived that

$$(23.6) \quad A_n = \frac{1}{n!} u_{n-1} = \frac{1}{n!} D_0 n e^{e^x}$$

where  $D = d/dx$ <sup>(12)</sup>. The final step was to determine  $D^n e^{e^x}$  by a "tolerably laborious process" which amounted to the equation

$$(23.7) \quad u_{n-1} = \left[ 1 + \frac{n(n-1)}{2} + \frac{n \dots (n-3)}{2.4} + \frac{n \dots (n-2)}{1.2.3} + \dots \right] e$$

from which (23.5) follow as partial results.

In the Memoirs Herschel followed exactly the same procedure up to (23.6), but he made an error in the computation of the coefficient of  $t^2$  in the right-hand side of  $e^{e^t}$ . Thus, instead of the right result (23.6) he arrived at the erroneous one

$$(23.8) \quad u_n = \frac{d^n e^{e^t}}{dt^n} \Big|_{t=0}$$

where  $t$  equals 0 after the differentiation [1813a, 62]<sup>(13)</sup>.

In reply to Herschel's letter Babbage wrote "Your expression for  $\frac{1^n}{1} + \frac{2^n}{1.2} + \dots$  is strange I certainly never met with it be-

fore nor do I feel quite confident of the law followed by it" [H.S, 2: 5, 22(?) Dec. 1812]. The fact is that the procedure followed in Herschel's letter was right. By means of his theorem for the expansion of  $f(e^t)$  -see (23.18)- the development of  $e^{e^t}$  was included as an example in [1820, 82-3]. It was shown that

$$e^{e^t} = e + e \frac{t}{1} + 2e \frac{t^2}{1.2} + 5e \frac{t^3}{1.2.3} + \dots$$

where  $e=u_0$ ,  $2e=u_1$  etc. (as in (23.5)). In other words, (23.6) holds true, while (23.8) is obviously wrong [see (23.27) below].

In May 1813 Herschel set <sup>to</sup> work on Laplace's Mécanique, suggesting its study to Babbage<sup>(14)</sup>. In reply the latter informed him of Laplace's paper "Sur divers points d'analyse" [1809] and of various papers by Lagrange<sup>(15)</sup>. In 1814 Herschel became acquainted with Spence's work. The result of his new studies was his paper <sup>on</sup> titled "Consideration of various points of analysis" [1814], quite probably after Laplace [1809]. Babbage was outraged with the title, but that was as far as he had read his friend's paper<sup>(16)</sup>. For, when in 1817 he proposed a certain principle of notation, Herschel, disappointed, wrote at the end of Babbage's letter: "If Babbage had ever read my paper [[1814] ...] he would not have thought it necessary now to remark this. I have all along gone on that principle" [H.S, 2: 76, 29 March 1817].

What Babbage had suggested was the distinction between a characteristic which operates on quantity, like an  $f$ , and another which operates on other symbols of operation, like  $D = d/dx$ . This distinction is evident in the principles of notation given by Herschel in his [1814]. His rather complicated notation has as follows. Multiplication of two functions was denoted by  $\phi.\psi(x)$ ,

whereas their combination by  $\phi\psi(x)$ . The successive of  $f(x)$  was denoted by  $f^2(x)$ , while the product  $f(x).f(x)$  by  $\{f\}^2:x$ . Further, if  $F(x)$  stood for

$$(23.9) \quad ax^\alpha + bx^\beta + \dots,$$

then  $a\phi^\alpha(x) + \dots$  was denoted by  $(F:\phi):x$ , whereas, if  $x$  was replaced by  $\phi(x)$  in (23.9) the result would be  $(F\{\phi\}):x$  [1814, 442-3].

Next Herschel introduced the sign of derivation  $D$  according to the rule

$$(23.10) \quad D\phi f(x) = (D\phi):f(x),$$

in other words  $D$  affects only the characteristic which follows it immediately. If  $D$  was intended to affect a combination, it had to be written as  $D(\phi f):x$ . This description and rule held true also for  $d$ ,  $\Delta$ ,  $\delta$ ,  $\int$  and  $\Sigma$ . The last rules on notation were: "Every functional characteristic is affected by all the characteristics preceding it, in the same manner as if it were a symbol of quantity" and "every characteristic of operation performed on quantity affects all which follow it, as if it were one symbol" [1814, 443]. Thus, according to the last rule, if  $f(x)$  stands for the quantity in (23.9), then  $fD\phi(x) = a\{D\phi(x)\}^\alpha + \dots$  but this would not hold true for  $D$  instead of  $f$  because of (23.10) [1814, 443-4] (17).

By that time Herschel was acquainted with Arbogast [1800] [see (17) above] and his introduction of symbol "D" and its principles is an obvious influence from the latter. Moreover, he borrowed Arbogast's method of separation of symbols which he claimed to have extended in his paper further than had been customary [1814, 441]. In fact, he provided an original application of this method to the solution of differential equations that follows below.

Casting a linear differential equation of the first order with constant coefficients  ${}^1A, {}^2A, \dots$  in the form

$$(23.11) \quad u + {}^1ADu + {}^2AD^2u + \dots + {}^nAD^nu = 0,$$

he solved it by factorization:

$$(23.12) \quad (D-p)(D-q)\dots\dots: u = 0.$$

He examined first the case of distinct roots and ended with that of multiple ones [1814, 467-8; see also Koppelman 1971, 185]. Herschel was the first British mathematician to solve a differential equation by symbolical methods. His method reminds us of Français's [1.6], but he was totally unaware of either Français's or Brisson's work on the calculus of operations [Grattan-Guinness

1978, 85).

Apart from a small section on functional equations [see 2.4], the rest of [1814] was devoted to the theory of Laplace's generating functions and its application to Spence's theory of logarithmic transcendents in an attempt to sum trigonometric series in finite form. The procedures exhibited in this paper are rather wild and were not used in his more mature work. Of interest, though, is to notice once more the motivation for a general theorem which would afford the expansion of any function  $f(e^t)$ .

Laplace's generating functions were regarded as a "fertile source of new discovery" and as a "most comprehensive and uniform" medium for exhibiting known results. This calculus, he added, "commands a wider and more magnificent prospect than any which has yet been opened to the view of the speculative philosopher" [1814, 440]. If  $A_x$  is the coefficient of  $t^x$  in the expansion of  $\phi(t)$  then this was denoted by:

$$(23.13) \quad \phi(t) = G_t\{A_x\},$$

where  $t$  was omitted in  $G_t$  if obvious from the content of the discussion<sup>(18)</sup>.

Among Herschel's results were the following. Let  $aA_{x+\alpha} + bA_{x+\beta} + \dots$  be denoted by  $\nabla A_x$ . Then, according to the definition of  $G$  it was proved that

$$(23.14) \quad G(\nabla A_x) = \phi(t) \cdot f(1/t),$$

where  $f(t) = at^\alpha + bt^\beta + \dots$ . Another theorem was

$$(23.15) \quad G(x^i A_x) = \left(t \frac{d}{dt}\right)^i \phi(t)$$

[1814, 444-447]. Further substituting  $1/(\log t)$  for  $t/dt$  in (23.15), and letting  $\phi$  and  $f$  be as in (23.13) and (23.14), he obtained a rather awkward theorem [see (33.4)] which afforded the summation of trigonometric series in finite form [1814, 447-9]<sup>(19)</sup>.

Thus Herschel went on to prove the utility of the calculus of generating functions by making free use of his latter theorem in order to obtain relations between Spence's transcendents and the Bernoulli numbers. Among his concerns was once more to determine the coefficients of particular functions of  $e^t$ , e.g. of  $-1/(\sqrt{-1+e^t})$  [1814, 450-2]. His procedure -devoid of any considerations on convergence -was far more general and systematic than in his [1813a] offering a wider range of applications.

However, it was soon to be superseded by the theory presented in his [1816].

This paper opened with a brief review of the demonstrations provided by Laplace, Arbogast and Brinkley for Lagrange's theorem, which he wrote in the form

$$(23.16) \quad \Delta^n u_x = (e^{\Delta x \cdot D} - 1)^n u_x,$$

where  $D = d/dx$ . Laplace's method lacked elegance, according to Herschel, as it involved the passage from finite to infinite. But, like Brinkley, he praised Laplace's treatment for the important case of  $n = -1$ . Arbogast was mentioned in connection with his generalization of (23.16) for any function  $f$ ,

$$(23.17) \quad f(1+\Delta)u_x = f(e^{\Delta x \cdot D})u_x$$

[1816, 25-26].

Herschel took these two theorems for granted satisfied with their demonstrations by Brinkley and Arbogast [see 2.2]. Claiming next that Brinkley's method for the expansion of the right-hand side of (23.16) led to complications, he went on to provide the expansion of the more general expression  $f(e^{\Delta x \cdot D})$  in (23.17), replacing  $\Delta x \cdot D$  for convenience by  $t$ . His theorem amounts to the following symbolical formula:

$$(23.18) \quad f(e^t) = f(1) + \frac{t}{1} f(1+\Delta)0 + \frac{t^2}{1.2} f(1+\Delta)0^2 + \dots$$

where, given  $f$ , the development at the right-hand side is readily determined by means of the  $\Delta^n 0$  numbers [1816, 27-28; 30].

The proof given in that paper was based on a finite difference equation which was rather obscurely derived and far from lucidly integrated [1816, 28-29]. But, few months after the publication of his paper, he discovered a much simpler proof, which with delight he sent to Babbage in a letter [H.S. 20:36, 10 Oct. 1816]. The same demonstration was included in his Examples [1820] enriched with numerous applications. Quoting from his [1820] the proof of (23.18) has as follows.

According to Taylor's theorem we have

$$(23.19) \quad f(e^t) = f(1+e^t-1) = f(1) + \frac{f'(1)}{1} (e^t-1) + \frac{f''(1)}{1.2} (e^t-1)^2 + \dots$$

In order to determine the coefficient of  $t^x$  in the right-hand side of (23.19) he reasoned as follows. Let  $x \neq 0$ , then the coefficient of  $t^x$  in the first term  $f(1)$  is 0 and, if  $x=0$ , then it is  $f(1)$ . Thus, in both cases, this coefficient can be written as

$f(1) \cdot 0^x$ . Accordingly, the coefficient of  $t^x$  in the second term was found to be  $f'(1) \cdot (1^x - 0^x)/x!$  written as  $f'(1) \cdot \Delta 0^x/x!$  and of the third term  $(f''(1)/1.2) \cdot (\Delta^2 0^x/x!)$  and so on. Adding these results, the coefficient  $A_x$  of  $t^x$  in the expansion of  $f(e^t)$  was determined readily as

$$(23.20) \quad A_x = \frac{1}{x!} \left[ f(1) 0^x + \frac{f'(1)}{1} \Delta 0^x + \dots \right].$$

Separating next the symbols of operation from those of quantity, (23.20) was reduced to

$$A_x = \frac{1}{x!} \left[ f(1) + \frac{f'(1)}{1} \Delta + \frac{f''(1)}{1.2} \Delta^2 + \dots \right] 0^x = \frac{f(1+\Delta)}{x!} 0^x,$$

hence, theorem (23.18), was obtained [1820, 66-8].

Herschel obtained two more symbolical expressions equivalent to (23.18), namely

$$(23.21) \quad f(e^t) = f(1+\Delta) e^{0 \cdot t} \text{ and } f(e^t) = e^{\Delta \cdot 0 + 0 \cdot t} f(1)$$

[1816, 30]. His next task was to determine the development of  $(e^t - 1)^{-n}$ ,  $n > 0$ . Like Laplace and Brinkley, he wrote the latter function in the more convenient form  $t^{-n} \cdot t^n / (e^t - 1)^n$  which equals  $t^{-n} f(e^t)$ , where  $f$  is defined by

$$(23.22) \quad f(e^t) = \left( \frac{\log e^t}{e^t - 1} \right)^n.$$

He then applied his theorem (23.18) to the function (23.22) obtaining

$$(23.23) \quad \left( \frac{t}{e^t - 1} \right)^n = 1 + \frac{t}{1} \left[ \frac{\log(1+\Delta)}{\Delta} \right]^n 0 + \frac{t^2}{1.2} \left[ \frac{\log(1+\Delta)}{\Delta} \right]^n 0^2 + \dots$$

Thus the coefficient  $C_x$  of  $t^x$  in (23.23) is

$$(23.24) \quad C_x = \frac{1}{x!} \left[ \frac{\log(1+\Delta)}{\Delta} \right]^n$$

[1816, 31; 1820, 82].

As we saw in 2.2, Brinkley had also provided a direct formula for the determination of the coefficient  $A_x$  of  $t^x$  for the same development by (22.17). Though both expressions ( $A_x$  and  $C_x$ ) led to the same results, they differ in their form. It was only in 1860 that Herschel was able to prove the equivalence between  $C_x$ , as in (23.24) and  $A_x$ , as in (22.17) [see his 1860]. As we mentioned in 2.2, Herschel proved in his [1820, 82] that the expression given by Brinkley for  $n > 0$  holds also for negative powers

[See (22.18) and 2.2, (12)].

Herschel's next concern was the direct computation of the Bernoulli numbers. Let  $B_x$  and  ${}^nB_x$  stand for the Bernoulli numbers of the first and the  $n^{\text{th}}$  order, defined as the coefficients of  $t^x/x!$  in the development of  $f(e^t)$  given by (23.22). It follows, thus, that

$$(23.25) \quad {}^nB_x = x!C_x,$$

$C_x$  as in (23.24). He then went on to provide the development of (23.24) in powers of  $\Delta$  determining thus, via (23.25), the values of  ${}^nB_x$  [1816, 31-35]. This was the first direct, simple method for the determination of  ${}^nB_x$ . Proud of his theorem he wrote to Babbage: "... it is astonishing how fertile this theorem is of curious results and particularly of the variety of forms they afford for the numbers of Bernoulli" [H.S, 20: 36, 10 Oct. 1816].

Next, based on Lagrange's theorem (15.10), he proceeded to develop any function  $f(e^t, e^{t'}, \dots)$  with more than one independent variables  $t, t', \dots$  [1816, 40-43]. The paper ended with a rather complicated formula for the development of  $f\psi^n(t)$  in powers of  $t$ . Making  $\psi(t) = \log^{-1}(t)$ , Herschel was now able to develop  $f(\log^{-n}(t))$  where  $\log^{-n}(t)$  stands for (23.3) [1816, 43-5]. This result was sent in a letter to Babbage [H.S, 20: 26, 24 Sept. 1815]. This theorem was however omitted in the Examples due to its outmost abstractness and generality. Only instances of it, deduced directly from the theorem (23.18) were presented, such as the direct computation of  $e^{e^t}$  given below.

By means of (23.18) it follows that  $e^{e^t} = e^{1+\Delta 0^x} = e \cdot e^{\Delta 0^x}$ . Or, by means of (23.20) we have that the coefficient  $A_x$  of  $t^x$  in the development of  $e^{e^t}$  is determined as

$$(23.26) \quad A_x = \frac{e}{x!} e^{\Delta 0^x} = \frac{e}{x!} \left[ 0^x + \frac{\Delta 0^x}{1} + \frac{\Delta^2 0^x}{2!} + \dots + \frac{\Delta^x 0^x}{x!} \right],$$

thus  $A_0 = e$ ,  $A_1 = 2e/2!$ ,  $A_2 = 5e/3!$ , or

$$(23.27) \quad e^{e^t} = e + e \frac{t}{1} + 2e \frac{t^2}{2!} + 5e \frac{t^3}{3!} + \dots$$

[1820, 82-3]. In a separate example he provided the sum of the series  $S$  given by:

$$(23.28) \quad S = 1^n + \frac{2^n h}{1} + \frac{3^n h^2}{1.2} + \frac{4^n h^3}{1.2.3} + \dots,$$

which, for  $h=1$  is the series  $u_n$  studied as (23.4) in his [1813a].

S was written in the form:

$$S = 0^{n+1} + \frac{1^{n+1}}{1} h + \frac{2^{n+1}}{1.2} h^2 + \dots = e^{h(1+\Delta)} 0^{n+1} =$$

$$= e^h [0^{n+1} + \frac{\Delta 0^{n+1}}{1} h + \dots + \frac{\Delta^{n+1} 0^{n+1}}{(n+1)!} h^{n+1}].$$

We omit details on the computations which were based upon results of the application of (23.18). The importance lies in that the latter series is a terminating one due to the properties of the  $\Delta^n 0^m$  numbers [See (22.13)]. Supposing finally  $h=1$  Herschel could determine the values of  $u_n$  for  $n = 1, 2, 3, \dots$  [1820, 90-1; see also (23.5)].

In fact all these last results had been deduced in the letter he wrote to Babbage in 1812 quoted in the beginning of our section. But, the new method established in 1816 was far more convenient, direct and general. The first to notice Herschel's theorem was Lacroix in 1819<sup>(20)</sup>. But from 1820 up to the mid 1830's no research was carried in England on Herschel's lines. It was W.R. Hamilton who discovered the important theorem (23.18) naming it after Herschel [1837, 235]. In this paper Hamilton proved a generalization of (23.18) which has as follows:

$$(23.29) \quad \varphi(1+\Delta)f\psi(0) = f(1+\Delta')\varphi(1+\Delta)(\psi(0))^{0'},$$

where  $f, \varphi, \psi$ , are arbitrary functions and  $\Delta'$  refers to the variable  $0'$ . However, as "Hamilton's theorem" was later named his concise symbolic form of Maclaurin's theorem

$$(23.30) \quad f(x) = f(1+\Delta)x^0.$$

which, in fact, is (23.21) if we replace  $x$  by  $e^x$  [Hamilton 1837, 235-6; Bronwin 1847d, 137-8; 5.2].

Up to 1840, Lagrange's algebraic calculus -as developed further by Brinkley and Herschel - was mainly diffused in England via Lacroix [1816] and Herschel's Examples [1820]. These two works were gradually replaced by revised presentations of their material enriched with further applications, such as Gregory's Examples [1841] and De Morgan's textbook [1842c]. From that time onwards, Herschel's theorem -included in many textbooks on finite differences<sup>(21)</sup>- formed a cornerstone in the research on operator methods. This theorem was to be acknowledged in France late in the 19<sup>th</sup> century by [Laurent 1890, 86].



## 2.4 Babbage and Herschel on functional equations: 1813-1814; applications of Laplace's method.

Babbage became involved with functional equations in 1809 but started producing work around 1811 [Dubbey 1978, 52]. He initiated Herschel in this subject in 1812. In his first letter to him he asked whether there are functions  $\phi$  which satisfy the equation " $\psi x. \phi x = \phi f x$ " [H.S., 2:1, 20 June 1812]. This letter motivated Herschel to delve into a study of functional equations. Enthusiastically he answered "Alas, ... I was stringing one functional equation after another with uncramming delight" [H.S., 20:1, 1 July 1812]. And, whereas Babbage dealt with functional equations in Laplace's way only in the paper "On continued products" (1813) he contributed in the Memoirs, Herschel applied Laplace's method on functional equations in three papers [1813a,b; 1814] providing his friend with further inspiration for the original work he was soon to produce in 1815-1816 (2.5-2.6).

At this early stage, Laplace's method -as studied in 1.4- was the only general method available for the treatment of functional equations. The main drawback of this method, however, was its dependence upon the integration of finite difference equations which were "above the powers of analysis in the present state" [Babbage 1813, 11]. Under the influence of Laplace's work on finite differences, as in his Mécanique [see 2.3, (14)], Herschel set off to provide a general theory of finite difference equations of the first degree in one variable [1813b, 65-83]. In the same paper he studied functional equations reducing them à la Laplace to finite difference ones [1813b, 96-102]. In our present study we will focus on the early treatment of functional equations by Babbage and Herschel paying attention to the intricacies involved in the procedures and to the evaluation of their results. Finite difference equations will not be dealt with in detail, though.

In his first paper, [1813], Babbage was interested in determining various trigonometric products, based upon the formula

$$(24.1) \quad \psi(f(x)) \dots \psi(f^n(x)) = \frac{\psi(f^{n+1}(x))}{\psi(f(x))}$$

derived from the functional equation

$$(24.2) \quad \psi(x) \cdot \varphi(x) = \varphi(f(x))$$

by successive substitution of  $f(x), f^2(x), \dots, f^n(x)$  for  $x$ . Given the functions  $f$  and  $\psi$ , Babbage would solve (24.2) for  $\varphi$  and then substitute the values of  $f, \psi, \varphi$  in (24.1). Finally, replacing quantities in  $x$  by trigonometric functions in (24.1) he would obtain the products sought. The left-hand side of (24.1) was called a continued product and was denoted by  $\prod (\psi f^n x)$  [1813, 1-3] (1).

Let us illustrate his procedure with the following example. Let  $\psi$  and  $f$  be given as

$$(24.3) \quad \psi(x) = 1+x^2+\dots+x^a, \quad f(x) = x^{a+1}.$$

Formula (24.2) is thus reduced to

$$(24.4) \quad \frac{x^{a+1}-1}{x-1} \varphi(x) = \varphi(x^{a+1}).$$

According to Laplace's method, equation (24.4) is solved by replacing first  $x$  and  $x^{a+1}$  by  $y(z)$  and  $y(z+1)$  respectively, and next by the substitution of  $u(z), u(z+1)$  for  $\varphi(x)$  and  $\varphi(x^{a+1})$ . Thus, the first step amounts to the solution of the finite difference equation

$$(24.5) \quad y^{a+1}(z) = y(z+1)$$

which gives readily  $y$

$$(24.6) \quad y(z) = c^{(a+1)^z} \quad (=x \text{ by definition}).$$

Due to the second transformation and the result (24.6), equation (24.4) is reduced to the finite difference equation

$$(24.7) \quad (c^{(a+1)^{z+1}} - 1)u(z) = (c^{(a+1)^z} - 1)u(z+1).$$

Solving (24.7) for  $u(z)$  the value of  $\varphi(x)$  is readily obtained.

Babbage observed that the equation (24.7) is of the form

$$(24.8) \quad Q(z+1) \cdot u(z) = Q(z) \cdot u(z+1),$$

whose solution is known to be

$$(24.9) \quad u(z) = b \cdot Q(z),$$

$b$  constant. From (24.7) and (24.9) it follows consequently that

$$(24.10) \quad \varphi(x) = u(z) = b(c^{(a+1)^z} - 1) = b(x-1).$$

Replacing the constant  $b$  by 1, Babbage obtained from (24.10) a particular solution  $\varphi(x)=x-1$  of (24.4). Substituting  $f, \psi$  from (24.3) and  $\varphi(x)=x-1$  in (24.1), he was able to obtain different continued products for different values of " $a$ " [1813, 3-5]. Replacing next the quantities  $x+x^{-1}, x-x^{-1}$ , which thus appeared, by  $2\cos\theta$  and  $2\sqrt{(-1)}\sin\theta$  respectively, he would obtain the

trigonometric products he had sought [1813, 6-12].

In May 1813, Herschel announced to him that he had found a technique for determining the  $z^{\text{th}}$  successive function of  $f(x)$  [H.S., 20:8, 4 May 1813]. His result was published in [1813a, 47-51]. The procedure followed is the following: let  $u(z) = f^z(x)$ . Then  $f^{z+1}(x) = u(z+1) = f(u(z))$ , thus

$$(24.11) \quad f(u(z)) - u(z+1) = 0.$$

The solution of (24.11) is of the form

$$(24.12) \quad u(z) = F(z, c),$$

$c$  constant. Since  $u(0) = f^0(x) = x$ , the arbitrary constant  $c$  in (24.12) will be determined if  $z$  is replaced by 0 in (24.12). Consequently  $f^z(x)$  will be given by (24.12).

For example, let  $f(x) = 2x^2 - 1$  and  $f^z(x) = u(z)$ . Equation (24.11) gives by solution

$$(24.13) \quad u(z) = \frac{1}{2} (c^{2^z} + c^{-2^z}).$$

Let  $z = 0$ . Then (24.13) is reduced to  $x = 1/2(c + 1/c)$  and one of the values of  $c$  is found to be  $x + \sqrt{(x^2 - 1)}$ . Herschel, substituting this value in (24.13), obtained the  $z^{\text{th}}$  successive value of  $f$  as

$$(24.14) \quad f^z(x) = \frac{1}{2} [(x + \sqrt{(x^2 - 1)})^{2^z} + (x - \sqrt{(x^2 - 1)})^{2^z}].$$

The same example was to be presented in his [1814, 458-9].

In his second paper, Herschel undertook to study the first-order functional equation

$$(24.15) \quad F(x, \phi(F_1(x)), \dots, \phi(F_{n+1}(x))) = 0,$$

$F, F_1, \dots, F_{n+1}$  given and  $n \geq 1$ . He communicated his general theory for the solution of (24.15) in a lengthy letter to Babbage [H.S., 20:10, 25 July 1813]. In this letter Herschel sounded very confident of his procedures and proud of extending Laplace's method which affords the solution of (24.15) only for  $n \leq 1$ . Nonetheless Herschel asked Babbage to provide him with any relevant work by Lagrange on the integration of partial difference equations which were involved in the case of  $n > 1$ . Babbage, in reply, informed him about Laplace's, Monge's and Lagrange's respective work available and also suggested an alternative method for the solution of functional equations, that of expanding the functions in series [see 2.3, (15)]. This method was due to Lagrange [1766] and was followed also by Spence [1809] [see 1.4, (9); (22.5); (26.1)].

Herschel thanked Babbage for his helpful information and wrote: "In the total want of any general method your way of infinite series values" [H.S. 20:11, 16 August 1813]. However, it was only within their correspondence that Lagrange's method was discussed [see 2.6, (2)]. By that time both analysts were acquainted with the main work of Lagrange, Monge and Laplace on finite difference and functional equations. The influence of the latter two is apparent in Herschel [1813b] <sup>(2)</sup>. As his procedure for  $n > 1$  is very complicated, and as Herschel later discovered that it involved contradictions, we will present it in brief.

Herschel built his general theory starting with Monge's simplest case (14.20), or

$$(24.16) \quad \varphi(F(x)) - A(x) = 0,$$

where  $\varphi$  is to be determined. On lines similar to those followed by Monge [1.4], he was based on the "absolute perfection of analysis", that is, he assumed the existence of  $F^{-1}$ , when  $F$  is given, and replacing  $x$  by  $F^{-1}(x)$  he derived from (24.16)  $\varphi(x) = A(F^{-1}(x))$ . He argued then on the superiority of his notation " $F^{-1}$ " for the inverse function and claimed that by generalization of the process mentioned above one can deduce the following result:

$$(24.17) \quad \text{If } \varphi A_1 A_2 \dots A_n(x) = B(x) \text{ then } \varphi(x) = B A_n^{-1} \dots A_1^{-1}(x).$$

Calling (24.17) a "theorem" he claimed that it "sets in clear light the analogy between functional and exponential indices" [1813b, 98] <sup>(3)</sup>.

Herschel was to stress often in his letters to Babbage the analogy between functional and exponential indices as well as the method of separation of symbols of operation from those of quantity. Certain instances of the outcome of their correspondence on analogy will be discussed in 2.7. What in fact Herschel implicitly proved by his (24.17) was the property

$$(24.18) \quad (f_1 \dots f_n)^{-1} = f_n^{-1} \dots f_1^{-1}$$

which, overlooked by Servois and Sarrus, was to be introduced by Murphy [1837] [see 3.3]. So, Herschel is to be acknowledged for his coming close to (24.18). However, he did not delve any further in the properties of inverse functions. In fact, up to the 1815's, both analysts implicitly assumed the uniqueness of the inverse function [Enros 1979, 176; see also 2.6, (6)].

Herschel's next step was the solution of

$$(24.19) \quad F(x, \varphi(F_1(x)), \varphi(F_2(x))) = 0.$$



how practically impossible and will I'm afraid, ever remain. This however must not deter you from proceeding onwards; the theory of Functions will I am confident at some future period meet with that attention which its difficulty and importance justly merit" [see 2.3, (15)]. Indeed, Babbage's comments proved to be right. Herschel was to discover three years later that his procedure was erroneous. "I found that I have committed one of the most egregious blunders that ever grinned in a man's face from a printed book [....]. These equations [(24.20)] with shame to myself I confess it, never perceived to be contradictory ...", he wrote to Babbage in his [H.S, 20:36, 10 Oct. 1816]<sup>(5)</sup>.

Herschel's extension of Laplace's method was <sup>to be</sup> noticed by De Morgan, who amended it stressing that it "can only apply, with perfect generality, to cases in which the unknown function has not more than two subjects"<sup>(7)</sup>. In fact, from 1836 onwards Laplace's method was to be applied only for the case (24.19) and thus, with the exception of De Morgan's article [1836], Herschel's own work remained without any impact whatsoever.

Herschel had in fact tackled in his letter the linear equation

$$(i) \quad {}^1fF\varphi_{(1)}(x) + \dots + {}^nA_x {}^{n+1}fF\varphi_{(n+1)}(x) + B_x$$

and not equation (24.15) as he had done in his [1813b]. In the end of his [H.S, 20:10, 25 July 1813], where he had communicated his general method to Babbage, he wrote that one cannot conceive a more general equation than (i) without introducing "things of the form

$$f\{A_x F(\varphi_{(1)}(x)) + B_x \cdot F^2\varphi_{(2)}(x) + \dots\} \text{ or } F(\varphi(x) + a_x F(\psi(x) + \dots))$$

But such things have not yet been imagined, and it is even something to have arrived at a point from whence we can look onward to such difficulties. Perhaps equations involving  $F^2$ ,  $F^3$ , might with more propriety be called functional equations of the second, third....order. Do you think a few words more might not be inserted in the preface regarding this? If you have not entered deeply into the subject or are idle, I will undertake to say something pretty profound on it. But then I must have before me an abstract of what has been previously inserted"<sup>(8)</sup>.

Thus, motivated by Babbage's early enquiries, Herschel got seriously engaged with the subject of functional equations and introduced in his [1813b, 111-113] equations of order higher than

the first, as well as the recursive definition for "partial" functions

$$(24.21) \quad \varphi^{m+1}(x,y) = \varphi(\varphi^{m-1+1}(x,y),y),$$

which reappeared in his [1814, 459] and were followed by Babbage in his later work [see 2.6]<sup>(9)</sup>. But of outmost importance in the subsequent development of functional equations was his treatment of equations

$$(24.22) \quad \varphi^n(x) = f(x)$$

and

$$(24.23) \quad \varphi^2(x) = x.$$

We will conclude our section with Herschel's work on these two equations as in his [1814].

Dealing with the form (24.22) he applied his favourite method of separation of symbols, reasoning as follows:

$\varphi^n(x)=f(x) \longrightarrow \varphi^n=f \longrightarrow \varphi=f^{1/n}$ . Thus, if  $f(x)=2x^2-1$ , then by (24.14) -putting  $n$  instead of  $z$ - we obtain a satisfactory particular solution of (24.22) as

$$(24.24) \quad \varphi(x) = f^{1/n}(x) = \frac{1}{2} [x + \sqrt{(x^2-1)}]^{\frac{1}{2}} + (x - \sqrt{(x^2-1)})^{\frac{1}{2}}].$$

"We may here observe", he wrote, "that any one of the  $n$  values of  $\sqrt[n]{2}$  will equally afford a satisfactory value of  $\varphi(x)$ " [1814, 463]. In fact, Herschel had conceived the possibility to determine the iterated values  $f^z(x)$  for  $z$  fractional or imaginary in his [1813a]. This was an immediate consequence of the formula (24.14) for the particular example under consideration<sup>(10)</sup>.

Two methods were given for the solution of (24.23) in his [1814]. Both methods were primarily based upon the substitution

$$(24.25) \quad x = u_z, \quad \varphi(x) = u_{z+1}.$$

According to the first method, it follows from (24.23) and (24.25) that

$$(24.26) \quad \varphi(u_z) = u_{z+1} \text{ and } \varphi(u_{z+1}) = u_z = x.$$

From (24.26), by subtraction, we have that  $\Delta(\varphi(u_z)+u_z)=0$ . This integrated gives

$$(24.27) \quad \varphi(u_z) + u_z + c = 0,$$

where  $c$  an arbitrary function. Cross-multiplication of the formulae (24.26) gives additionally

$$(24.28) \quad u_{z+1}\varphi(u_{z+1}) = u_z\varphi(u_z).$$

Formula (24.28) conveys that the function  $u_z.\varphi(u_z)$  does not vary when  $z$  varies. As a result,  $c$  in (24.27) may be any function of  $u_z.\varphi(u_z)$ , thus, the solution  $\varphi(x)$  of (24.23) is given implicitly

by means of (24.27) as

$$(24.29) \quad x + \varphi(x) + f(x\varphi(x)) = 0,$$

for any assigned form of  $f$  [1814, 461].

According to the second method we obtain from (24.23) and (24.25) the relation

$$(24.30) \quad \varphi(u_{z+1}) = u_z = \varphi(u_{z-1}).$$

Taking the inverse function  $\varphi^{-1}$  we deduce from (24.30) that

$$(24.31) \quad u_{z+1} = u_{z-1}.$$

By solution of the finite difference equation (24.31) we get the value of  $u_z$  in two arbitrary functions  $C, C'$  :

$$(24.32) \quad u_z = C(\cos 2\pi z) + (-1)^z C'(\cos 2\pi z).$$

Regarding next  $z$  as a function of  $x$ , he deduced from (24.32) the value of  $\varphi(x)$ . "This method", he wrote, "applies also to the more general equation  $\varphi^2(x) = f(x)$ , by the substitutions  $f(x) = u_z$ ,  $\varphi(x) = u_{z+1}$ , but, owing to the transcendental equations it introduces, must be regarded as totally ineffectual and useless" [1814, 462-3].

The first of these methods, as based upon the observation that follows from (24.28), was in fact motivated by Babbage's reasoning in a paper on mixed difference equations presented to the Analytical Society in 1812<sup>(11)</sup>. Herschel acknowledged Babbage for the procedure based on (24.28) in his [H.S. 20:28, 6 Nov. 1815]. But apart from this implicit influence, Babbage's main assistance to Herschel during the period 1813-1814 was in the form of bibliographical information and encouraging comments blended with criticism.

On the contrary, Herschel was critical enough to perceive serious errors in Babbage's drafts and quite often helped by clarifying certain paradoxes his friend was confronted with. His encouraging comments though would often turn to a harsh criticism [2.5-2.6]. But, when at a certain point he had had enough of his friend's bitter remarks, <sup>Babbage</sup> he apologised adding:

You would think the calculus of functions much more wonderful if I had never mentioned a word about them and I were now to send all my papers on the subject.

[H.S. 2:45, 9 Nov. 1815; on this letter see also 2.6, (12)]. He thus claimed, in a way, priority in this branch of analysis.

But could Babbage have completed and published his work without first consulting Herschel? To what extent did he benefit



from his friend's encouraging and critical remarks ? As will be evident in the following sections [2.5-2.7] Babbage was considerably influenced and helped by Herschel in many ways. But let us see first Herschel's answer to Babbage<sup>a</sup> few years later when the latter decided to write a treatise on the history of the calculus of functions and asked temporarily<sup>for</sup> his letters back<sup>(12)</sup>. Herschel found the right opportunity to remind him of his first papers -studied above- claiming, in his turn, his own priority over Babbage's work. In his [H.S, 20:40, 31 March 1817] he wrote:

As to any share I may have had in the theory of funct. equations, my papers in the Anal. Soc. and Phil. Trans. can speak for me. I am not conscious of having advanced anything on that subject in those papers good, bad or indifferent that I cannot strictly lay claim to, not excepting the original solution of  $\phi^2x=x$  considered as a functional equation of that form. It was I think after the date of those papers that you began to apply with such great success to the subject. The Idea of partial functional equations, the notation I used to express them -the theory (with all its imperfections on its head) of the elimination of arbitrary constants from functional equations-the theory of  $n^{\text{th}}$  functions in general- these I certainly am indebted for to no one<sup>(13)</sup>.

So, Herschel provides us with a quite satisfactory answer as to the extent of his own priority over the development of the calculus of functions. Moreover, the tendency for generalization -mostly evident in his early work [see (8) above]- was a characteristic peculiar to Herschel and had a direct impact consequently on Babbage. Thus, it is rather a fact to say that Herschel, via his early papers and letters, prepared the ground for Babbage's original contributions in 1815 which we proceed to study in the next 3 sections.

2.5 Babbage on functional equations of the first order in one variable: 1815-1816.

The core of Babbage's work on functional equations is presented in two lengthy papers [1815], [1816], whereas further illustrations of his theory are exposed in his [1817] and in his Examples [1820]. From the early 1820's up to our century, his name was linked mainly with the following techniques. The one

concerns the transformation

$$(25.1) \quad \psi(x) = \varphi^{-1}f\varphi(x),$$

where  $\varphi$ ,  $f$  initially are regarded as arbitrary functions, as used for the solution of functional equations of order higher than one in  $\psi$  [2.6]. The other, concerning equations of the form

$$(25.2) \quad F(x, \varphi(x), \varphi(\alpha(x)), \dots, \varphi(\alpha^n(x))) = 0$$

where  $F$ ,  $\alpha$  are known and  $\alpha$  such that  $\alpha^{n+1}(x) = x$ , amounts to successive substitution of  $\alpha(x), \dots, \alpha^n(x)$  for  $x$  and to consequent elimination of the quantities  $\varphi(\alpha(x)), \dots, \varphi(\alpha^n(x))$  <sup>(1)</sup>.

With the exception of (25.2), in all the cases he dealt with, the general solution was based on the existence of a particular one given either from the beginning or easily found by speculation over the functional equation under solution.

In this section we will focus only on first-order functional equations in one variable as presented in his [1815] including the case (25.2) which, though published in his [1816], was tackled earlier than 1816 [see his 1815, 423]. But before we proceed to discuss Babbage's methodology, let us have a brief look at the origins and early stimulations of his research. Our main source of information will be the correspondence between him and Herschel during the period 1813-1815. Interested at this stage principally in Babbage's published material, we will postpone few interesting matters discussed in this correspondence (which had no direct impact on the shaping of Babbage's work as presented here) for 2.6-2.7.

The first time when Babbage <sup>became</sup> involved with the solution of functional equations was during his attempt to solve a problem of a geometrical nature around 1811. This problem, mentioned by Pappus, related to the inscription of circles in a semicircle. Motivated by this problem, he set off to investigate another of a similar nature involving the inscription of circles between an hyperbola and its asymptotes. As he recalled later, perceiving the difficulty of the later problem, he put aside the subject for about two years [1815, 391-2].

In 1812 Babbage consulted Herschel on the equation (24.2) which was tackled in his [1813] by means of Laplace's method [2.4]. Gradually he became acquainted with Lagrange's and Monge's functional methods which he readily communicated to Herschel in a letter [see 2.3, (15)]. Herschel was additionally consulted on the problem of the hyperbola which he tried to solve by means of

his favourite method of finite differences -a branch of analysis that was also among Babbage's early mathematical concerns [see 2.4, (11)]. The outcome of Herschel's enquiries was rather pessimistic and in the letter regarding the problem of the hyperbola he wrote:

I would advise that you have some regard for the shortness of human life in your problems for the future. There are really dreadful calculations. The hyperbola is one of the most complicated I have met with a long time<sup>(2)</sup>.

During the period from October 1813 up to May 1814 hardly any letters of mathematical context were exchanged between them. Put off by the difficulties involved in his early researches -including those related to the solution of finite difference equations- Babbage was mainly engaged with chemistry. His interest in functional equations was revived in May 1814 by Maule's letter in which the first general solution of the equation

$$(25.3) \quad \psi^n(y) = y$$

was introduced, namely

$$(25.4) \quad \psi(y) = \varphi^{-1}((-1)^{2/n}\varphi(y))$$

where  $\varphi$  is arbitrary<sup>(3)</sup>.

Inspired by Maule's solution (25.4), Babbage set off to work on equations of order higher than one and to apply gradually the transform (25.1). Anxious, he sent to Herschel his first results. In his [H.S. 2:24, 4 July 1814] he announced to him that he had solved the equations

$$" \psi^2 y = y, \psi^n y = y, \psi^n y = Ay, \psi a \psi B(y) = \psi(y) " \dots\dots\dots$$

and finally

$$" A(\psi^n y, \psi^{n-1} y, \dots, \psi y, y) = 0 ".$$

Babbage did not present any sketch of their solution. The scope of his brief letter was only to ask Herschel to explain a "paradox" he was confronted with while trying to solve the equation " $\psi^n y = Ay$ " [see (26.3)] (where  $A, a, \beta, \dots$  were all known functions). Another, rather pathetic, letter followed including personal complaints and a brief statement on the last of the equations mentioned above [H.S. 2:25, 1 Aug. 1814].

Herschel sounded quite pessimistic in his answer [H.S. 20:17, 4 Aug. 1814]. "And in your case", he wrote, "what is the result? a heap of fusty old functions, fished up in night and solitude, out of Lethe (metaphor) or from the Slough of Despond

[....] but where the Devil am I got to? -From  $\psi^2(y) = y$  to the Dead Sea, via Hell! [....]. I have however looked at your functional - $\psi^2(y) = y$ , you remember gives  $0 = \psi(y) + y + f(y\psi(y))$  [(24.29)]  $f$  being an arbitrary  $f_n$  of  $\psi^n(y) = f(y)$  a particular sol. can always be obtained, and frequently  $n$  such. I should like to see how you get an arb. function into your sol $_n$  of  $\psi^n(y) = A(y)$  -and I think you can hardly, as you say, have really found a solution of  $A(\psi^n(y), \dots, y) = 0$ ". Herschel went on to clarify Babbage's "paradox" [2.6] and asked him for a way to determine a solution of (25.24)[See (10) below].

Babbage's somehow delayed reply in September 1814 was really generous. In this lengthy letter he presented his mode of solution of the previous equations, making a systematic application of the transform (25.1)[see 2.6]. Moreover, he included the first traces of his methodology for the solution of first-order equations, such as (25.5), together with few examples. Encouraged by his friend's interest he wrote: "Your letters generally bring on a paroxysm of my Analytical or Chemical mania; but I really believe oxygen and functions are as necessary for the mind as bread and meat are for the body" (4).

Herschel was delighted to go through his friend's research and responded with enthusiasm: "You have indeed in my opinion laid the foundations of a calculus totally new and immensely powerful" [H.S., 20:20, 25 Oct. 1814]. In this letter he pointed out to Babbage the analogy between functional, as in  $f^{1/n}(x)$ , and exponential indices, as in  $\sqrt[n]{x}$ . From that point onwards plenty of discussions were to follow concerning principally the transform (25.1), the intricacies involved in its application and matters of analogy. In fact Herschel became so interested in this transform that he attempted to apply it beyond functional equations [2.8, (6)]. From October 1814 up to December 1816 the two men held a most interesting correspondence on mathematics, helping and challenging one another with enthusiastic or critical comments [2.7].

Babbage opened his first essay on the calculus of functions [1815] with a distinction between direct and inverse calculations, stressing that the latter are more difficult but also often more useful than the former [1815, 389-390]. He introduced next the basic principles of notation of his own inverse method of functions -such as notation (24.21) for  $m = 2$  - adding that

"Equations of the second and higher orders have never been even mentioned", totally omitting any reference to Herschel's respective work (see 2.4, (13)). Giving a few words on the origins of his researches which concerned problems of geometric nature, he went on to acknowledge the first authorities in the branch of functional equations, that is D'Alembert, Euler and Lagrange; "but it is to Monge", he added, "that we are indebted for the most general view of the subject" [1815, 393] (5). He mentioned Monge's memoir [1776] adding that in the same volume with Monge's memoir there was a paper of Laplace on that subject. Without providing any reference, he mentioned Herschel's extension of Laplace's method which "leaves nothing to be regretted, but the narrow limits of our knowledge respecting the integration of equations of finite differences" [1815, 394]. "From this and other causes", he went on, "I am still inclined to think that the solution of functional equations must be sought by methods peculiarly their own" [1815, 394-5].

Thus Lagrange had furnished Babbage with a first method for the solution of functional equations by means of series [2.4, 2.6], followed Laplace with the reduction to finite differences, Herschel's extension of Laplace's method [2.4], and finally Maule's hints on the (25.1) transform on one hand and Monge's general algebraic methods on the other. Most probably Babbage's technique of elimination in connection with (25.2) was inspired by Monge [1776] where the latter introduced for the first time this technique in (14.22). According to Enros [1979, 176] Babbage himself claimed that his notation was the product of his discussions with Herschel and Bromhead. But despite any influences from the above mentioned analysts, Babbage's work was largely original as Herschel himself admitted later [see 2.6].

Babbage defined a function to be the result of every operation that can be performed on a quantity omitting at this stage to mention anything whatsoever on the inverse of a function [1815, 389]. Any solution containing arbitrary constants, would be called a "particular solution" of the functional equation under study, said Babbage, whereas, "For the sake of convenience", if it contains one or more arbitrary functions it would be called a "general solution". In his first two essays there is hardly any distinction between "a general" and "the general" solution of an equation. This intricate matter was first studied by Herschel

followed by Babbage. This matter will not be pursued in our study<sup>(6)</sup>. Following Babbage we will indifferently use "the solution" or "the general solution" in what follows.

Babbage built his theory of first-order equations starting iteratively from the case

$$(25.5) \quad \psi(x) = \psi(\alpha(x))$$

up to the case

$$(25.6) \quad F(x, \psi(x), \psi(\alpha(x)), \dots, \psi(v(x))) = 0.$$

Let the equation (25.5) and assume  $f$  to be a particular solution of it. Then, "it is evident" that

$$(25.7) \quad "\psi = \varphi f"$$

where  $\varphi$  is arbitrary, will satisfy (25.5) [1815, 395; on notation see also (11) below].

Next the notion of a symmetrical function  $\varphi(x, y)$  was introduced as possessing the property

$$(25.8) \quad \varphi(x, y) = \bar{\varphi}(y, x)$$

where a bar was omitted at the left-hand side of (25.8). With the notation

$$(25.9) \quad \varphi(x, y, \bar{z}, \bar{v})$$

it was implied that  $\varphi$  is symmetrical relative to  $z$  and  $v$  [1815, 396]. Now, the ground was ready to solve (25.5) under the further assumption that we are given a particular solution of

$$(25.10) \quad f(x) = f(\alpha^2(x)).$$

Babbage assumed  $\psi$  to be symmetrical relative to two functions  $f, f_1$ , or that

$$(25.11) \quad \psi(x) = \bar{\varphi}(f(x), f_1(x)),$$

where  $\varphi$  arbitrary and  $f, f_1$  to be determined. Substituting  $\psi$  in (25.5) he deduced

$$(25.12) \quad \bar{\varphi}(f(x), f_1(x)) = \bar{\varphi}(f(\alpha(x)), f_1(\alpha(x)))$$

which gives

$$(25.13) \quad f(x) = f_1(\alpha(x)) \text{ and } f_1(x) = f(\sigma(x)).$$

From (25.13) it follows readily that

$$f(\alpha^2(x)) = f_1(\sigma(x)) = f(x).$$

Therefore, if  $f$  is a particular solution of (25.10), equations

(25.13) are satisfied if  $f_1(x) = f(\sigma(x))$ . Hence, according to (25.11) the general solution of (25.5) will be in this case

$$(25.14) \quad \psi(x) = \bar{\varphi}(f(x), f(\alpha(x)))$$

[1815, 396-7].

It was remarked consequently that if  $\sigma^2(x) = x$ , then  $f(x) = x$

in (25.14). Apparently this was an outcome of the fact that, under the latter condition, (25.10) is satisfied by any function, thus a particular solution of it could be  $f(x) = x$ . Consider for example, the equation  $\psi(x) = \psi(a^2/x)$  which is of the type (25.5) where  $\alpha(x) = a^2/x$ , "a" a constant. It follows readily that  $\alpha^2(x) = x$ , hence, according to the above remarks the solution of the given equation follows from (25.14) as  $\psi(x) = \bar{\varphi}(x, a^2/x)$ ,  $\varphi$  arbitrary and symmetrical [1815, 397-8] ('7).

The second problem tackled was the solution of the equation  
 (25.15)  $\psi(x) = A(x) \cdot \psi(\alpha(x))$   
 given a particular solution  $f$  of (25.15), as well as another,  $f_1$ , of the equation

$$(25.16) \quad \psi(x) = \psi(\alpha^2(x)).$$

Exactly as in the previous case the solution of (25.15) was determined to be

$$(25.17) \quad \psi(x) = f(x) \cdot \bar{\varphi}(f_1(x), f_1(\alpha(x))).$$

A second method was also proposed which we omit [1815, 398-9].

Apart from very rare cases, most of the examples provided by Babbage were constructed on purpose for the illustration of the equations under study. In fact, the examples were few in number and always followed after the general theory ('8). For the case of (25.15) he formulated the equation

$$(25.18) \quad \psi(x) = \frac{1-x^2}{1-2x} \cdot \psi\left(\frac{x}{x-1}\right).$$

Now  $\alpha(x) = x/(x-1)$  and (25.16) becomes

$$(25.19) \quad \psi(x) = \psi\left(\frac{x}{2x-1}\right).$$

By observation  $f(x) = 1+x$  and  $f_1(x) = x^2/(x-1)$  are particular solutions of (25.18) and (25.19) respectively. Since

$f_1(\alpha(x)) = \dots = x^2/(x-1) = f_1(x)$  we have that  
 $\varphi(f_1(x), f_1(\alpha(x))) = \varphi(x^2/(x-1))$ , or the solution of (25.18) is

$$(25.20) \quad \psi(x) = (1+x) \varphi\left(\frac{x^2}{x-1}\right).$$

[1815, 399].

The next problem studied was to determine  $\psi$  from

$$(25.21) \quad \psi(x) = A(x) \cdot \psi(\alpha(x)) + B(x)$$

given a particular solution  $f$ . By analogy apparently from its form ('8), Babbage assumed the solution of (25.21) to be of the

form

$$(25.22) \quad \psi(x) = f(x) + \phi(x).$$

Determining  $\psi(\alpha(x))$  from (25.22) and substituting the values of  $\psi(x)$  and  $\psi(\alpha(x))$  in (25.21) he obtained an equation from which subtracting (25.21) for  $\psi=f$ , he was led to the equation

$$(25.23) \quad \phi(x) = A(x) \cdot \phi(\alpha(x)).$$

Now, (25.23) is of the form (25.15) and apparently can be solved and thus the general solution of (25.21) can be finally obtained from (25.22). But, as it was typical of Babbage, the fact that (25.15) could be solved -in his essay- only under the additional condition (25.16), was a trivial matter which was totally overlooked [1815, 400].

This time the illustration which followed concerned an equation deduced by Herschel while working on Spence's transcendents for his [1814]. This equation is

$$(25.24) \quad \psi(e^y) + \psi(e^{-y}) = A(y),$$

where  $A$  a rational and integral function of  $y$ . It was also

given that  $L^{2n}(1+y)$ ,  $L^{2n+1}(1+y)$  [defined by (22.3)] are particular solutions of (25.24). According to the problem (25.21) it was easily found that the general solution of (25.24) was  $\psi(y) = f(y) + \phi((\log y)^{2n+1})$ , with  $f$  a particular solution of (25.24) and  $\phi$  arbitrary [1815, 400-401] (10).

Letting once more  $\psi(x)$  to be as in (25.22), Babbage reduced the equation

$$(25.25) \quad \psi(x) + A(x) \cdot \psi(\alpha(x)) + \dots + N(x) \cdot \psi(v(x)) + X = 0$$

to

$$(25.26) \quad \phi(x) + A(x) \cdot \phi(\alpha(x)) + \dots + N(x) \phi(v(x)) = 0.$$

Equation (25.25) is but a generalization of (25.21). The reduced form (25.26) was solved under certain further considerations -omitted as we saw in the study of (25.21)- on lines similar to those of problem (25.15). In the course of the procedure which follows below we notice the germs of his technique applied in his [1816] to the equation (25.2) or (25.38).

Babbage assumed that the known functions  $\alpha(x)$ , ...,  $v(x)$  in (25.26) are of the form  $\alpha$ ,  $\alpha^2$ , ...,  $\alpha^{n-1}$  such that

$$(25.27) \quad \alpha^n(x) = x.$$

Assuming further that, under this condition, we are given a particular solution  $f$  of the equation (25.26), it followed as in (25.17) that the solution of (25.26) is



$$(25.28) \quad \varphi(x) = f(x) \cdot \bar{\chi}(x, \alpha(x), \dots, \alpha^{n-1}(x))$$

[1815, 402-3; see also the observation following the case (25.5) under the assumption (25.10) and (25.27) -for  $n = 2$ ].

Of a different nature was the problem which required the determination of an  $\varphi$  such that

$$(25.29) \quad \varphi(x) = \varphi(\alpha(x)) = \dots = \varphi(v(x))$$

holds true. The procedure which followed amounted to the determination of a particular value  $f$  such that  $f(x) = f(\alpha(x))$ , another  $f_1$ , such as  $f_1(f(x)) = \dots = f_1(f(\beta(x)))$  and so on, apparently by speculation as in the initial problem (25.5). Thus, according to the principles applied to that problem he was led to

$$(25.30) \quad \varphi(f_n(\dots(f(x) \dots))),$$

$\varphi$  arbitrary, as the general solution of the system (25.29). Two trivial examples followed as an illustration [1815, 403-5].

As we have already mentioned above, Babbage had not delved into a study of the inverse of a function yet, and Herschel, despite his shrewd remarks in his letters and his perception of omissions, misprints or other errors in Babbage's work, was not able to observe any omission in the above case. But De Morgan was critical enough to see that Babbage's procedure for the case (25.29) is correct only under the condition that  $f_n$  does not invert  $f_{n-1}$  in (25.26), in other words that  $f_1$  is not an inverse function of  $f_{1+1}$  [1836, art. 162-3].

The last step before the consideration of the general case (25.6) was the determination of  $\psi$  such as

$$\psi(\alpha(x)) = \psi(\alpha_1(x))$$

$$(25.31) \quad \dots\dots\dots$$

$$\psi(v(x)) = \psi(v_1(x)) .$$

On lines similar to those followed in the previous case, he observed that

$$(25.32) \quad \psi(x) = \varphi f_n f_{n-1} \dots f \alpha \beta \dots v x^{(11)},$$

where  $f, f_1, \dots, f_n$  are particular solutions of (25.31) in the given order, will satisfy the above conditions. He concluded by claiming: "We now possess the means of solving a much more general problem than we have yet attempted, it is the general solution of any functional equation of the first order" [1815, 405-6].

Thus Babbage managed to discover a purely functional method in order to tackle the general case (25.6) -which, in fact, is identical with Herschel's (24.15)- devoid of finite differences.

He would have been perhaps much more proud had he known that Herschel's general method was to prove a failure, a fact discovered in 1816 [see 2.4, (6)]. His own method has indeed no common point with that of Herschel's, however one can not disregard the fact that it was Herschel who conceived first even the form of a general equation such as (25.6), let alone that he struggled to find its solution inspiring Babbage on the possibility of viewing this matter from a different angle. Moreover, despite certain omissions that rendered Herschel's solution of (25.6) incomplete [2.4, (7)], his method was far more direct than that of Babbage's sketched below which is dependent on the solution of the problems that preceded it.

Now, for the general case (25.6), or

$$(25.33) \quad F(x, \psi(x), \psi(\alpha(x)), \dots, \psi(v(x))) = 0,$$

Babbage assumed the existence of an arbitrary function  $\varphi$  such that (25.29) holds true. He then let the particular solution of (25.33) be denoted as

$$(25.34) \quad \psi(x) = f(x, a, b, c, \dots),$$

where  $a, b, \dots$  are arbitrary constants which do not appear in (25.33). It was further assumed that when (25.34) is substituted in (25.33), the constants  $a, b, \dots$  cancel each other. He next replaced the constants in (25.34) by arbitrary functions  $\varphi(x), \varphi_1(x), \dots$  which satisfy the condition (25.29) and which, according to the above assumption, will have to destroy each other in (25.33). Thus,

$$(25.35) \quad \varphi(x) = f(x, \varphi(x), \varphi_1(x), \dots)$$

is "a general solution containing as many arbitrary functions as the particular solution did constant quantities" [1815, 406]<sup>(12)</sup>.

The procedure sketched so far was to be totally disregarded in the development of the branch of functional equations by English analysts in the 1840's. It is only in De Morgan's article [1836] that we see a partial reproduction of Babbage's methodology as illustrated in (25.5)-(25.33), such as for example of the case (25.29) discussed above. Even during his correspondence with Herschel, interest was focused mainly on equations of order higher than the first in one variable, and the above procedures passed virtually unnoticed. Based upon these observations we will not delve any further into a more detailed discussion of Babbage's work so far. We will conclude this section with instances of the application of the procedure studied above to

equation (25.2) as well as to differential functional equations.

Equation (25.2), or,

$$(25.36) \quad F(x, \psi(x), \psi(\alpha(x)), \dots, \psi(\alpha^n(x))) = 0,$$

with the condition

$$(25.37) \quad \alpha^{n+1}(x) = x,$$

is but a generalization of (25.5) under the additional condition (25.37). In fact, according to the problems (25.25) and (25.31) -which were not used for the solution of (25.33)- every functional equation of the form (25.33) can in theory be reduced to the case (25.36). Thus, taking for granted this fact, the method that follows is much more direct than the previous one, conducting us to a particular solution devoid of arbitrary constants; and "if by any means we could introduce into these solutions an arbitrary constant, it would afford us general ones" [1816, 229].

Let us illustrate Babbage's method with the case

$$(25.38) \quad F(x, \psi(x), \psi(\alpha(x))) = 0, \quad \alpha^2(x) = x$$

instead with the general one (25.36). Substituting  $\alpha(x)$  for  $x$  in (25.38) we obtain

$$(25.39) \quad F(\alpha(x), \psi(\alpha(x)), \psi(x)) = 0.$$

Further eliminating  $\psi(\alpha(x))$  from (25.38) and (25.39) we will arrive at an equation of the form

$$(25.40) \quad F(x, \alpha(x), \psi(x)) = 0$$

which in theory can be easily solved [1816, 230-232].

For example, given

$$(25.41) \quad [\psi(x)]^2 \cdot \psi \frac{1-x}{1+x} = c^2 x, \quad c \text{ constant},$$

we notice that it is of the form (25.38) where  $\alpha(x) = (1-x)/(1+x)$  and  $\alpha^2(x) = x$ . Change  $x$  into  $\alpha(x)$  in (25.41), hence

$$(25.42) \quad \left[ \psi \frac{1-x}{1+x} \right]^2 \cdot \psi(x) = c^2 \frac{1-x}{1+x}.$$

Eliminating  $\psi((1-x)/(1+x))$  from (25.41) and (25.42) we have readily

$$(25.43) \quad \psi(x) = \left[ \frac{1+x}{1-x} c^2 x^2 \right]^{1/3}$$

as a particular solution of (25.41) [1820, 7].

Babbage's procedure, as applied to the equation (25.38) was reproduced by Gergonne, further extended by De Morgan and remained a standard method for the solution of functional equa-

tions up to 1860<sup>(13)</sup>. However, elimination, observed Babbage, is not always possible. Take for example equation

$$(25.44) \quad \psi(x) = (a-x)\psi(\alpha(x)), \quad \text{with} \quad \alpha^2(x) = x$$

which is homogeneous relative to  $\psi$ . Elimination of  $\psi(\alpha(x))$  leads -as in the case (25.41)- to the equation

$$(25.45) \quad \psi(x) = (a-x)(a-\alpha(x))\psi(x)$$

which is meaningless [1816, 233]<sup>(14)</sup>. Another exception was noticed, namely the case of (25.36) when the variable quantity  $x$  is missing. A further study of such exceptions was included in his [1817] -see 2.7.

Suppose now we have a differential functional equation where  $\psi(x)$  is of the first order, namely

$$(25.46) \quad \psi(\alpha(x)) = \frac{d^n \psi(x)}{dx^n} \quad \text{with} \quad \alpha^p(x) = x.$$

Replacing, as in (25.38),  $x$  successively by  $\alpha(x), \alpha^2(x), \dots, \alpha^{p-1}(x)$  we arrive at

$$(25.47) \quad \begin{aligned} \psi \alpha^2(x) &= \frac{d^n \psi \alpha(x)}{(d\alpha(x))^n} \\ \dots\dots\dots \\ \psi \alpha^p(x) &= \psi(x) = \frac{d^n \psi \alpha^{p-1}(x)}{(d\alpha^{p-1}(x))^n} \end{aligned}$$

Eliminating further all the quantities  $\psi \alpha^p(x)$  for  $p = 1, \dots, p-1$ ,  $\psi(x)$  is finally to be determined by

$$(25.48) \quad \psi(x) = \frac{d^{np} \psi(x)}{(d\alpha^{p-1}x \dots d\alpha x dx)^n}$$

[1816, 237-8]. In the same way other equations of the type (25.46) were solved by being reduced to differential equations<sup>(15)</sup>. The arbitrary constants in the solutions of the latter equations were determined so as to satisfy the given equation [1816, 235-240].

The paper ended with a lengthy solution of the equation

$$(25.49) \quad [\psi x]^2 + \left( \frac{\psi x d\psi x}{dx} \right)^2 = \left( \psi \left( x + \frac{\psi x d\psi x}{dx} \right) \right)^2$$

which was related to a geometrical problem regarding the nature of a curve so that its normal has certain properties<sup>(16)</sup>. This problem was in fact solved in Babbage's letter [H.S. 2.41, 25 Sept. 1815]. This was the second complicated problem of such na-

ture that Babbage undertook to solve two years after he had had his disappointment with the one related to the hyperbola. Herschel had assisted Babbage by pointing out that the two problems lay in fact upon similar principles [Babbage 1815, 393]. When he received Babbage's solution he wrote "I wish I had a little more room, to send you a geometrical solution of your problem about the normal and ordinate. Yours is neat enough" [H.S. 20:27, 7 Oct. 1815].

Omitting the complicated details, we will confine to mention that Babbage's originality lay in observing that the quantity  $\psi x d\psi x/dx$  does not change when for  $x$  we substitute  $x + \psi x d\psi x/dx$ . Putting consequently  $t$  for  $\psi x d\psi x/dx$ , (25.49) was reduced to

$$(25.50) \quad [\psi(x+t)]^2 - [\psi(x)]^2 = t^2,$$

$t$  considered as a constant quantity. Finally,  $\psi(x)$  was determined by means of a differential equation in  $\psi$  [1816, 253-6]<sup>(17)</sup>. In fact, Babbage's technique in this problem—apparently discovered around 1812-1813 [2.4, (11)]—had motivated Herschel's solution of equation  $\psi^2(x) = x$  [see (24.28) and 2.4, (13)].

Apart from few omissions regarding the inverse function (as in the case of (25.29)) and certain printing errors, the main weakness in Babbage's procedure, as in the material exposed so far from his [1815], lies in his adding new assumptions in the course of a proof and his incomplete theory on the generality of a solution. He was perfectly aware of the latter, as it is obvious from the discussion which followed the solution of the general case (25.33): "This reasoning is certainly plausible, and such a solution is undoubtedly a very general one; still, however, there are reasons which incline me to believe, that other solutions exist of a yet more general nature" [1815, 409; See also (6) above].

In 1815 very few were to acknowledge Babbage's abstract and general procedures. Both his methodology and the illustrations he offered sounded rather artificial and with little scope for useful applications. It is not thus very surprising that the review<sup>(18)</sup> of his [1815] in 1816 was not a very enthusiastic one. His second essay [1816] sounded more promising including in the end the interesting example of the normal (25.49). In what we discussed so far we have a first view of Babbage's foundations of the calculus of functions, a branch that was soon to bring up to the surface new intricacies such as those involving the con-

tinuity of functions or the properties of the inverse function. But of most importance was his study of functional equations of order higher than the first which we shall discuss together with few elements of his theory of functional equations in two variables.

## 2.6 Babbage on functional equations of order higher than the first; equations in two variables: 1815-1816.

Babbage was in frequent contact with Herschel during the period 1813-1815. As soon as he had an interesting result from his functional enquiries, he would immediately communicate it to Herschel and would be delighted to receive his comprehensive answers which dealt more with Babbage's theories on functions than with Herschel's own enquiries in the calculus of operations and finite differences. Not all the methods which Babbage discovered were published [see (12) below]. Many theories proved to be erroneous and Herschel was critical enough to perceive where exactly the error lay<sup>(1)</sup>.

When Herschel wrote to Babbage his first remarks on the possibility of solving equations of order higher than one [see 2.4, (5), (8)], Babbage set off to apply Lagrange's series method [1.4, (9)] for the solution of " $\varphi^2 x = \chi$ ", where  $\chi$  was apparently a rational and integral function of  $x$ . Letting  $\varphi x = \Lambda_1 x + \Lambda_2 x^2 + \dots$ , with  $\Lambda_1, \Lambda_2, \dots$ , constants, he substituted this value in the preceding equation, determining  $\varphi x$  by comparison of terms. He illustrated this method by the example

$$(26.1) \quad \varphi^2 x = \frac{x}{1-x}$$

which by the series method gave as solution

$$(26.2) \quad \varphi x = \frac{2x}{2-x}.$$

He realised, however, that this solution, (26.2), was "not sufficiently general"<sup>(2)</sup>. This is the earliest attempt to solve an equation in  $\varphi^2 x$  and Babbage's unique application of Lagrange's method.

From July 1813 -when (26.2) was deduced- up to July 1814 Babbage did not produce any work on functional equations. After

receiving Maule's letter in May 1814 (2.5, (3)) he set working hard on functional equations of order higher than the first. While engaged with the solution of

$$(26.3) \quad \psi^n y = Ay,$$

where  $A$  a given function, he asked Herschel to explain the following "troublesome paradox". Putting  $\psi y$  for  $y$  in (26.3) he deduced:

$$\psi^n \psi y = A \psi y = \psi^{n+1} y$$

and taking  $\psi$  of both sides of (26.3) he got  $\psi \psi^n y = \psi Ay = \psi^{n+1} y$ , "hence  $\psi Ay = A \psi y$   $\psi = A$  is one solution hence  $A^n = A$  which is absurd" [H.S, 2:24, 4 July 1814].

"Your paradox is easily explained" wrote Herschel in his [H.S, 20:17, 4 Aug. 1814]. "It does not follow that because  $\psi A(y) = A \psi(y)$  that therefore  $\psi = A$ , although the converse is true. In fact the  $\psi A = A \psi$  is an indeterminate funct<sup>n</sup>  $\psi^n$ . admits ... of an  $\infty$  ns of Relations between  $\psi$  and  $A$ . Thus  $\psi = A$ ,  $\psi = A^{-1}$ , are both equally solutions of it, and an  $\infty$  variety of others. Now of these, -the form of  $A$  being given- there is but one relation between  $\psi$  and  $A$  which will give such a form to  $\psi$  as will satisfy  $\psi^n = A$ . Now, neither  $\psi = A$ , nor  $\psi = A^{-1}$ , is the proper solution to be taken, as the former would give  $A^n = A$  or  $A^{n-1} = 1$  and the latter,  $A^{-n} = A$  [or]  $A^{n+1} = 1$ , both false. I think the paradox is now explained, is it not? Obs.  $\psi^n = A$  is the general solution of  $\psi A = A \psi$ . -This embraces  $\psi = A$ , and  $\psi = A^{-1}$ ".

This is a sample of errors committed by Babbage in his letters and of Herschel's immediate response<sup>(3)</sup>. In his reply of 22 September 1814 [See 2.5, (4)] Babbage was to incorporate most of his theory on functional equations as solved by means of the transform (25.1)- or  $\psi x = \phi^{-1} f \phi x$ - invented after Maule's (25.4). Herschel admitted that his friend's solutions were indeed "elegant" [H.S, 20:20, 25 Oct. 1814]. At that time Herschel himself was engaged with the solution of equations of the form (26.3) and showed deep interest in Babbage's first lengthy letter on this subject. Postponing Herschel's comments for a while, let us see Babbage's main results which were almost identically reproduced in his [1815].

The first method that was presented in his [1815, 410] for the solution of

$$(26.4) \quad \psi^2 x = x$$

was identical to that offered by Herschel in his [1814] but no

reference to him was made by Babbage, to the latter's disappointment<sup>(4)</sup>. Omitting the second, we will confine to the third method as based upon the transform (25.1), or,

$$(26.5) \quad \psi x = \varphi^{-1} f \varphi x.$$

According to (26.5) we have

$\psi^2 x = \varphi^{-1} f \varphi \varphi^{-1} f \varphi x = \varphi^{-1} f^2 \varphi x = x$ , for arbitrary functions  $\varphi$  and  $f$ . But if we let  $f$  to be a particular solution of (26.4), we have that  $f^2 \varphi x = \varphi x$  or  $\varphi^{-1} f^2 \varphi x = \underline{\varphi^{-1} \varphi x} = x$ , therefore (26.5) stands for the general solution of (26.4) where now  $f$  is a particular solution and  $\varphi$  arbitrary [1815, 411]<sup>(5)</sup>.

In order to illustrate this method, Babbage wrote that, by observation  $a-x$  and  $x/(ax-1)$  being particular solutions of (26.4), we have that

$$(26.6) \quad \psi x = \varphi^{-1}(a-\varphi x) \text{ and } \psi x = \varphi^{-1}\left(\frac{\varphi x}{a\varphi x-1}\right)$$

will also satisfy (26.4). From each of the cases (26.6), "by assigning particular values to  $\varphi$ , new values of  $f$  may be determined, and these in their turn will furnish new forms of the function  $\psi x$ " [1815, 411]. For example, let  $f(x) = 1-x$ . It follows that  $f^2(x) = 1-(1-x) = x$ , hence, by (26.5),  $\psi x = \varphi^{-1}(1-\varphi(x))$ . If  $\varphi(x) = x^2$  then  $\psi x = \sqrt{1-x^2}$  is another particular solution of (26.4). Plenty of similar examples are found in [1815, 411-13; 1816, 234-5; 1820, 1-6] including the one given above.

We notice two omissions at this stage. The first regards Babbage's reluctance to mention the series method- by means of which particular solutions can be obtained without the necessity of guess-work. Most probably, despite his early use of Lagrange's method, he felt that it is rather messy in its calculations to be referred to in his published work. The second omission regards the lack of proper attention as to the definition of the inverse function, partly restored in his [1816]. Babbage noticed then very briefly that though  $\varphi \varphi^{-1} x = x$  holds always true,  $\varphi^{-1} \varphi x = x$  holds only for some values of  $\varphi^{-1}$ . He added: "without attending to this circumstance, our conclusions may become erroneous [...]. This remark, which is of some importance, extends to the conclusions in my former Paper [1815] and the whole of the subsequent enquiries" [1816, 191].

From this statement it becomes clear that he confined in his study only to functions  $\varphi^{-1}$  such that



$$(26.7) \quad \varphi\varphi^{-1}x = \varphi^{-1}\varphi x = x,$$

and so, in this case, his procedure as in (26.4) is thoroughly sound. Still, though the erroneous silent assumption by both him and Herschel that the inverse of a function is unique is now settled, there is still a vagueness as to the "multiplicity" of the values of  $\varphi^{-1}$  that satisfy (26.7). This vagueness was considerably clarified by De Morgan and more formally established by Murphy in the late 1830's [chap. 3]('6).

A final comment on Babbage's procedure so far concerns the statement he made after illustrating his method for the case (26.4) with the example (26.6) above. This comment is due to Herschel and was partly discussed by Babbage in his [1817]. The former wrote in his [H.S., 20:28, [Nov. 6 1815]]: "In the sol<sup>n</sup>  $\varphi^{-1}f\varphi(x)$  you say, and I remember we used to think that "by assigning particular values to  $\varphi$ , new values of  $f$  may be determined, and these in their turn will furnish new forms of the function  $\varphi(x)$ ". This is however not the case as a moment's consideration will convince you. True, for every general form (essentially different) you must have an essentially different particular sol<sup>n</sup>, not included under [...] other general one". On the effect of Herschel's letter we will comment in 2.7.

The next case that was considered was (26.3) in the form

$$(26.7)' \quad \varphi^2 x = \alpha x.$$

Application of the transform (26.5) leads directly to the equation

$$(26.8) \quad f^2 \varphi x = \varphi \alpha x$$

which is a particular case of (25.33), in other words an equation of the first order in  $\varphi x$ , solvable either by Babbage's standard method or by "the very elegant one of Laplace"('7). Thus, the solution of (26.7)' will be (26.5) where  $\varphi$  is determined by (26.8) relative to  $f$  and  $\alpha$  but while  $f$  is a "perfectly arbitrary function, except that neither  $fx$  nor  $f^0 x[f^2 x]$  must be equal to  $x$ : from not attending to this circumstance, I was at first led into several errors; the reason of these two restrictions is, that in the first case we at once determine  $\varphi x$  to be equal to  $x$ , and in the second, we in fact make  $\alpha x = x$ , neither of which are necessarily true" [1815, 413]. I guess that by "arbitrary" Babbage implied that function  $f$  is in fact a known function in (26.8) [see also De Morgan 1836, art. 156].

In fact, by means of a slightly different application of the

transform (26.5) all equations of order higher than the first are reducible to first order ones solvable either by Babbage's early method or by Laplace's. Let, for example, equation

$$(26.4) \quad \psi^2 x = x$$

and let  $y = \phi x$  in (26.5). Then, the latter,  $\psi x = \phi^{-1} f y$ , substituted for  $\psi x$  in (26.4), will give

$$(26.9) \quad \phi^{-1} f^2 y = \phi^{-1} y,$$

a first order equation relative to  $\phi^{-1} y$  [See (25.5)]. Thus according to (25.14) [where  $x$  stands for  $y$ ,  $\psi$  for  $\phi^{-1}$  and  $\alpha$  for  $f^2$ ] (8) the solution of (26.9) will be

$$(26.10) \quad \phi^{-1} y = \bar{\chi}(y, f^2 y),$$

where  $\chi$  an arbitrary function and  $f^2$  (the equivalent of  $\alpha$  in (25.10)) a particular solution of (26.4), or

$$(26.11) \quad f^4 y = y$$

[1815, 414-5] (9).

Among the most general cases tackled by the second application of the transform (26.5) was

$$(26.12) \quad F(x, \psi x, \psi^2 \alpha x, \dots, \psi^n \nu x) = 0.$$

Babbage let  $\psi x = A^{-1} \phi A x$  and assumed  $A x$  to be such a function that  $A x = A \alpha x = \dots = A \nu x$  by problem (25.29). Substituting next this value of  $\psi x$  in (26.12), and further denoting  $A x$  by  $y$ , he reduced (26.12) to

$$(26.13) \quad F(A^{-1} y, A^{-1} \phi y, \dots, A^{-1} \phi^n y) = 0$$

which he claimed to be of the form

$$(26.14) \quad F(x, \psi x, \psi^2 x, \dots, \psi^n x) = 0$$

[1815, 416-7]. In fact, since  $A$  is known, Babbage's claim is not totally invalid. However, had he had not substituted  $y$  for  $A x$ , he would have had instead of (26.13),

$$(26.15) \quad F(x, A^{-1} \phi A x, \dots, A^{-1} \phi^n A x) = 0,$$

which in fact was the step prior to (26.13) in his procedure, and by putting  $\psi$  for  $A^{-1} \phi A$ , (26.15) would have been readily reduced to (26.14) (10). Thus, the solution of (26.12) depended upon the solution of (26.14).

Assuming next  $\psi x = \phi^{-1} f \phi x$  in (26.14) the latter equation is reduced, by further replacing  $x$  by  $\phi^{-1} x$ , to

$$(26.16) \quad F(\phi^{-1} x, \phi^{-1} f x, \phi^{-1} f^2 x, \dots, \phi^{-1} f^n x) = 0$$

"which is an equation of the first order relative to  $\phi^{-1}$  and may be solved by the methods in the beginning of this Paper, or, by means of the method given by Mr. Herschel, to which we have already alluded" [1815, 417; see also (7) above]. Next he specu-

lated on the arbitrariness of  $f$  in (26.16). Obviously  $f$  can not be such that  $f(x) = x$  or  $f^2(x) = x$  for then the generality of the problem is limited. The only value we can assign to  $f$ , wrote Babbage, is to suppose a particular solution of the given problem (26.14) [1815, 418].

The last problem concerned an equation of the form:

$$(26.17) \quad F(x, \psi x, \psi \alpha(x, \psi x)) = 0.$$

On lines similar to those followed above, he put

$$(26.18) \quad \alpha(x, \psi x) = \varphi^{-1} f \varphi x$$

solving it for  $\psi$ . The solution of (26.18) was denoted by

$$(26.19) \quad \psi x = \alpha^{1,-1}(x, \varphi^{-1} f \varphi x).$$

By some further substitutions, based upon (26.18) and (26.19), equation (26.17) was reduced to a first-order one relative to  $\varphi^{-1}$  solved as (26.16) above [1815, 420] (11).

As we remember from 2.4, at this stage both Babbage and Herschel were not yet suspicious about the validity of the latter's extension of Laplace's method to which Babbage referred in his [1815]. "I feel highly flattered by the honorable mention you make of me in several parts of your paper—far beyond my merits" wrote Herschel, pleased after reading Babbage's paper in his [H.S., 20:28 [Nov. 6 1815]]. The disastrous discovery took place in 1816.

However, in 1817 Herschel was happy to find out that his method could still be useful for cases such as (26.14). He wrote that by putting " $x=u_z$  and  $fx=u_{z+1}$ , it will follow that  $f^2x=fu_{z+1}$ , but since  $fu_z = u_{z+1} \dots fu_{z+1} = u_{z+2}$  and so on". Thus, putting  $\varphi^{-1}u_z = u_z$  in (26.16) the latter is reduced to

$$(26.20) \quad F(u_z, u_{z+1}, u_{z+2}, \dots, u_{z+n}) = 0.$$

"Thus the solution of the equation  $F(x, \psi x, \dots, \psi^n x) = 0$  is still directly reducible to differences [...] but I was puzzled about discovering what to make of the arbitrary constants. I am glad my bacon is saved so far" [H.S., 20:47, 1 Aug. 1817].

In his letters to Herschel in the autumn of 1815, Babbage dealt extensively with the case

$$(26.21) \quad F(x, \psi(x), \psi \alpha(x, \psi(x)), \psi \beta(x, \psi(x)), \dots) = 0,$$

which is a generalization of (26.17), not included in his [1815].

In brief, his theory was based on the substitution of  $u$  for  $\psi(x)$  in (26.21). Deriving next  $\psi$  from

$$(26.22) \quad F(x, u, \psi \alpha(x, u), \dots) = 0$$

he let  $\psi(x, 0) = \chi(x, u)$  or  $\psi x = \chi(x, \psi x)$ ; the solution of the latter

equation gives  $\psi x = \chi(x, u)$  a function that does not change when  $u$  becomes  $\alpha(x, u)$  [H.S. 2:41, 25 Sept. 1815].

In reply, Herschel wrote: "Good works are the only means of literary salvation, and I am afraid that little good can be worked out of your general method" [H.S. 20:27, 7 Oct. 1815]. He expressed several objections on Babbage's method, raising the question of whether  $x$  and  $u$  are independent variables or not and examining the consequences in both cases. Having shown the weaknesses of his friend's procedure, he burst into a harsh criticism:

I have remarked in your functional theories a strong tendency to cut the cable of definition and dart off under a press of sail into the offing of an unknown and most obscure subject. Believe me, functional equations are not easy: they are not to be resolved by wholesale in this way. Many an error will rise up in judgement against you when received as an undoubted truth, and consecrated by negligence— and many a fine theory will elude your grasp [...]. It is always so with the inventor and ought to be no discouragement. — My huge developments are for the most part (I find) abridged expressions of intuitive (or at least self-evident) truths. Some few of them however (as the devel. of  $\psi^n(t)$  where  $\psi(t) = e^t - 1$ , for instance) remain pretty things but no miracles.

Babbage admitted that this theory was erroneous, but added: "I do not think that error lays where you imagined" [H.S. 2:43, 28 Oct. 1815]. Babbage sent his [1815] to Herschel and in reply the latter wrote: "You must know, I like your paper better than I expected, which perhaps it is not very polite to say, and yet may be looked upon as a concealed compliment which is worth a dozen compliments" [H.S. 20:28, [Nov. 6 1815]]. Babbage was delighted: "I was much flattered by your bestowing so much attention on my functional theories". Apologising next about his fallacies in the previous letter, he wrote: "According to my natural disposition I sent you my first thoughts on this new point or at least the first which I could put into language...." [H.S. 2:45, 9 Nov. 1815] (12).

Babbage's [1816] was devoted mainly to equations in two variables. Most of the techniques used are borrowed from his former paper [1815]. Among these techniques was that of analogy from partial differential equations, extensively discussed in his [1817]. First he presented an extension of his theory for first-

order equations in one variable to those of two variables starting from the most simple case

$$(26.23) \quad \psi(x,y) = \psi(ax, by),$$

which is the equivalent of (25.5) in his [1815], up to the case that is equivalent to (25.31) [1816, 184-197]. As first-order equations in two variables do not present any particular interest or difficulty, we will deal only with (26.23), focusing next on equations of order higher than one. Novelties in Babbage's notation will be introduced only at the specific instances where required.

We notice two methods, in fact, applied to the case (26.23), both drawing on those introduced in [1815]. According to the first, he put in (26.23)

$$(26.24) \quad \psi(x,y) = \varphi(fx, f_1y).$$

Thus, the solution of (26.23) was reduced on lines fairly similar to those followed in (25.12)-(25.13) to the solution of the equations

$$(26.25) \quad f(x) = fax, \quad f_1y = f_1by$$

both of which are of the form (25.5). Thus, the general solution of (26.23) will be (26.24) where  $f, f_1$  particular solutions of (26.25) and  $\varphi$  "perfectly arbitrary". Or, let  $f(x,y)$  be a particular solution of (26.23). Then, according to (25.7), we have that

$$(26.26) \quad \psi(x,y) = \varphi f(x,y)$$

is another general solution of the same problem [1816, 184-5].

Let for example

$$(26.27) \quad \psi(x,y) = \psi\left(-x, \frac{1}{y}\right).$$

According to the first method, we have to find particular solutions of (25.25), or of  $f(x)=f(-x)$  and of  $f_1(y)=f_1(1/y)$ . By speculation we have  $f(x)=x^2$  and  $f_1(y)=(y^2+1)/y$ , thus, according to (26.24) we have:

$$(26.28) \quad \psi(x,y) = \varphi\left(x^2, \frac{y^2+1}{y}\right).$$

To illustrate the second method take the equation

$$(26.29) \quad \psi(x,y) = \psi(x^n, y^{1/n})$$

a particular solution of which is  $x^{\log y}$

[since  $f(x^n, y^{1/n}) = (x^{\log y^{1/n}})^n = x^{\log y}$ ]. Thus, according to

(26.26) the general solution of (26.29) will be  $\varphi(x^{1-\alpha}y)$ , where  $\varphi$  is arbitrary [1816, 185-6].

We now proceed to equations in two variables of order higher than one. The first group studied involved only functions of the form  $\psi^{n-1}(x,y)$  defined by

$$(26.30) \quad \psi^{n-1}(x,y) = \psi^{n-1-1}(\psi(x,y), y),$$

a notation introduced by Herschel in his [1814] [See (24.21); 2.4, (13)]. In the solution of the most simple case

$$(26.31) \quad \psi^{2-1}(x,y) = x$$

we have a first instance of analogy from partial differential equations. For Babbage, observing that the variable  $y$  is not involved, regarded it as a constant and claimed that it suffices to solve  $\varphi^2x = x$  ( or (26.4) ) and finally replace the arbitrary quantities which thus occur by arbitrary functions of  $y$ . Thus, since  $(b-x)/(1-cx)$  is a particular solution of (26.4), we have that

$$(26.32) \quad \psi(x,y) = \frac{b-x}{1-x \cdot xy},$$

where  $x$  arbitrary, will be a general solution of (26.31) [1816, 197-8]. In exactly the same way, he said, we can solve the general case

$$(26.33) \quad F(x, y, \psi(x,y), \psi^{2-1}(x,y), \dots, \psi^{n-1}(x,y)) = 0$$

[1816, 198-9].

A similar notation applies to equations with simultaneous functions, such as  $\overline{\psi^{2-2}}(x,y)$  defined as  $\psi(\psi(x,y), \psi(x,y))$  [1816, 183]. Take, again, the simplest case

$$(26.34) \quad \overline{\psi^{2-2}}(x,y) = 0.$$

Based on the form of this symmetrical equation, Babbage claimed that  $\psi(x,y)=fx-fy$  (where  $f$  is arbitrary) obviously satisfies (26.34). Or, since  $x-y$  is a particular solution of (26.34), then

$$(26.35) \quad \psi(x,y) = (x-y)\varphi(x, y),$$

where  $\varphi$  is arbitrary, can also serve as a general solution of (26.34) [1816, 200]. When the right-hand side of (26.34) equals a constant "a", then it follows easily that if we add "a" at the right-hand side of (26.35) we have a general solution of

$$(26.36) \quad \overline{\psi^{2-2}}(x,y) = a$$

[1816, 200-1].

The study of equations with simultaneous functions, such as (26.34), (26.35) or

$$(26.37) \quad \overline{\psi^{n \cdot n}(x, y)} = a(\psi(x, y))^b$$

raised interest in the properties of homogeneous functions, and a novel methodology based on these properties was put into action. But the notation of this theory is rather complicated and the respective methodology irrelevant to our study. For this reason we will omit a reproduction of it but simply mention, as an example, that by means of Babbage's theory it was proved that any of the quantities  $2xy$ ,  $x^2+y^2$ ,  $xy+y^2$ ,  $x^2-xy+2y^2$ , satisfied (26.37) for  $n=3$ ,  $a=8$  and  $b=4$  [1816, 203].

Among the most general problems tackled within this group of equations belongs

$$(26.38) \quad F(\psi(x, y), \overline{\psi^{2 \cdot 2}(x, y)}, \dots, \overline{\psi^{p \cdot p}(x, y)}) = 0,$$

where, according to the definitions given so far,

$$(26.39) \quad \overline{\psi^{p \cdot p}(x, y)} = \overline{\psi^{p-1 \cdot p-1}(\psi(x, y), \psi(x, y))}.$$

The first method applied to (26.38) was based on the intricate theory of homogeneous functions. By suitable substitutions, (26.38) was reduced to

$$(26.40) \quad F(\varphi u, \varphi^2 u, \dots, \varphi^p u) = 0$$

which is of the form (26.14) [1816, 211-12].

The second method amounted to a similar reduction, but by a different process. Babbage put  $x$  for  $y$  in  $\overline{\psi^{p \cdot p}(x, y)}$  obtaining in his notation

$$(26.41) \quad \overline{\psi^{p \cdot p}(x, y)} = \varphi^p(x) [y = x].$$

He consequently let  $x=y=u$  in (26.41) and then let  $u = \psi(x, y)$ .

By simple considerations (26.38) was accordingly reduced via (26.41) to

$$(26.42) \quad F(\psi(x, y), \varphi\psi(x, y), \varphi^2\psi(x, y), \dots) = 0.$$

Letting finally  $\psi(x, y) = z$ , (26.42) was transformed into

$$(26.43) \quad F(z, \varphi z, \varphi^2 z, \dots, \varphi^{p-1} z) = 0$$

again solved according to the case (26.14). Having determined  $\varphi$  from (26.43), the value of  $\psi$  was to be finally obtained from (26.41) for  $p = 1$  [1816, 212-13].

We notice that the special artefact in this case was the equation of a function in two variables to a function in one variable. Exactly the converse consideration was noticed in certain cases of his earlier work, particularly in Babbage's unpublished theory related to equation (26.22). This time he was much more careful than before and most probably this was due to his collaboration with Herschel during their correspondence in autumn 1815, extracts of which were quoted above ( (26.21),

(26.22)].

Up to this point we notice that Babbage's methodology amounted to successive reductions to simpler cases, and, as far as possible, to functional equations in one variable. The third group of problems discussed concerned symmetrical equations, such as,

$$(26.44) \quad x\psi^{1.2}(x,y) = y\psi^{2.1}(x,y),$$

dealt by an extension of the transform (26.5), namely of

$$(26.45) \quad \psi(x,y) = \psi^{-1}f(\phi x, \phi y).$$

Consideration of the symmetrical form of the equation (26.44) combined with the techniques involved in his theory of equations in one variable of order higher than one consisted his present methodology.

For example, changing  $x$  to  $\phi^{-1}x$  and  $y$  to  $\phi^{-1}y$  in (26.45), the new form of (26.44) after the substitution of (26.45) would be

$$(26.46) \quad \phi^{-1}x\phi^{-1}f^{1.2}(x,y) = \phi^{-1}y\phi^{-1}f^{2.1}(x,y).$$

It suffices, wrote Babbage, to find an  $f$  such that

$$(26.47) \quad f^{1.2}(x,y) = y \text{ and } f^{2.1}(x,y) = x$$

for then (26.46) would become  $\phi^{-1}x\phi^{-1}y = \phi^{-1}y\phi^{-1}x$  which is an identity. According to the problem (26.31) -which is in fact the case of (26.47)- it becomes evident that the result will be a function  $f$  symmetrical in  $x$  and  $y$ , such that, if we focus on  $x$  it is a particular solution of  $\phi^2x = x$ . In this case,

$$(26.48) \quad f(x,y) = a - x - y$$

fulfills the conditions (26.47) and thus according to (26.45) we have

$$(26.49) \quad \psi(x,y) = \phi^{-1}(a - \phi x - \phi y)$$

as a general solution of (26.44) [1816, 219-220].

The transform (26.45) was applied with the same success to other symmetrical equations which involved the variables  $x$ ,  $y$  and the functions  $\psi(x,y)$ ,  $\psi^{1.2}(x,y)$ , .... [1816, 220-223]. But in the case of (26.44) -devoid of the variables  $x$  and  $y$ - or of

$$(26.50) \quad \psi^{1.2}(x,y) = \psi^{2.1}(x,y),$$

the same substitution of (26.45) led again to (26.50) in  $f$  instead of  $\psi$ . If a particular value of  $f$  could be found, then (26.45) would provide us with a general solution. But it proved to be impossible to find any specific function so as to satisfy the given equation (26.50).

This was the first time that Babbage discussed the problem



of possibility for the solution of an equation. He showed that any function that satisfies (26.50) has to be necessarily symmetrical. He also "proved" that for any symmetrical function to satisfy (26.50) it follows that  $y = x$ , consequently "the equation is contradictory". "This train of reasoning", he went on, "I offer with considerable hesitation, well aware of the extreme difficulty of reasoning correctly on a subject so very general [....]. I thought it, however, right to mention this proof, that those who may seek for particular cases, might first enquire whether the equation be possible" [1816, 218-19].

Babbage's "proof", though not altogether sound -even for his own standards- is a crucial step towards laying the foundations of the study of the calculus of functions. He was followed by De Morgan who, on lines similar to those mentioned above, concluded his own study of (26.50) as follows: "This reasoning, is not as Mr. Babbage remarks, of a very sure character but it furnishes a strong presumption that all the solutions of the given equation are discontinuous" [1836, art. 319] (13).

Babbage proceeded to study equation (26.50) under the further condition that  $x = y$ . By simple considerations he was led to the conclusion that a particular solution could be found by problem (26.44) [1816, 225-7]. Having next tackled the problem (25.36) [1816, 229-235] the rest of the paper dealt with functional differential equations, representative cases of which were studied in 2.5 (see (25.46), (25.49)). Among the problems tackled were

$$(26.51) \quad \psi^2 x = \frac{d\psi x}{dx},$$

which by means of the transform (26.5) was reduced to a differential equation [1816, 240-241].

The rest of the paper dealt with equations of the form

$$(26.52) \quad \psi(x, y) = \frac{d\psi(x, \alpha y)}{dx} \quad \text{with} \quad \alpha^p y = y$$

If  $p = 2$ , substituting  $\alpha y$  for  $y$  in (26.52) we have

$$(26.53) \quad \psi(x, \alpha y) = \frac{d\psi(x, y)}{dx}.$$

Differentiating (26.53) relative to  $x$  and eliminating  $d\psi(x, \alpha y)/dx$  from (26.52) and the result of the differentiation,

we are led to a partial differential equation in  $\psi(x,y)$ . <sup>There</sup> followed a consideration on the determination of the two arbitrary functions which resulted from the integration of the final equation so as to fulfil the original equation [1816, 245-6]. On similar lines (26.52) was solved for any  $p$  so as  $\alpha^p y = y$  [1816, 246-248].

Babbage illustrated this latter group of equations with many examples. Plenty of examples related to the case (26.52) were also incorporated in his [1820, 28-36]. He finally dealt with the equations

$$(26.54) \quad \psi(x, y) = \frac{d^n \psi(x, \alpha y)}{dx^n}, \quad \text{where} \quad \alpha^p y = y$$

and

$$(26.55) \quad \frac{d\psi(x, \beta y)}{dx} = \frac{d\psi(\alpha x, y)}{dy}, \quad \text{where} \quad \alpha^2 x = x, \beta^2 y = y.$$

The former gave rise to fractional and negative indices [1816, 248-9] while the latter was reduced to a partial differential equation illustrated by two examples [1816, 249-251] (14).

In the same letter where he had reminded Babbage of his own contributions in the development of functional equations, Herschel acknowledged briefly the originality of Babbage's work. In his [H.S. 20:40, 31 March 1817], quoted in the end of 2.4, he wrote:

The  $\phi^{-1}f\phi$  and its whole train of consequences rests with yourself (except a slight passing tribute to Maule), as well as the method of resolving functional Egr<sup>ms</sup> from particular solutions, etc-

De Morgan was to make extensive use of the transform (26.5) in the form  $\phi f \phi^{-1}$  in his [1836] (15). R.L.Ellis also acknowledged Babbage's application of this transform for the case of equation (26.14) [Ellis 1843b, 135]. What survived above all from Babbage's original techniques was his extension of his method of elimination, as in the case (25.2) which we will study further in 2.7.

2.7 Babbage's paper on analogy [1817]; Herschel's influential role in the shaping of this paper.

As soon as Babbage had ready the contents of his [1816], he announced to Herschel in a letter [H.S. 2:43, 28 Oct. 1815] his plan for another paper which would involve equations in more than one unknown functions and which would also include applications of the calculus of variations. He added that:

The three [papers] together will form the outlines of a calculus which at some future period rival the Integral one.

By 1816, confronted with unexpected difficulties, his plan was postponed indefinitely. He concluded his second essay by writing [1816, 256]:

To complete the outline of this new method of calculation it would be necessary to treat of equations involving two or more functional characteristics, and to explain methods of eliminating all but one of them: these lead to a variety of interesting and difficult enquiries, and will probably be of considerable use in completing the solutions of partial differential equations: it could also be proper to consider the maxima and minima of functions, and to apply to this subject the method of variations; these are points of considerable difficulty, and although I have made some little progress in each of them, I shall forbear for the present any further discussion on this subject.

Eventually this third paper, which would form a sequel to his [1815] and [1816], was never completed [Enros 1979, 187]. Instead, we have another paper, [1817], in which Babbage confined to provide some further comments in respect with the theory he had introduced so far. For example, he extended his method of elimination so as to treat cases in which the former method had failed. Moreover, he drew a comparison between differential and functional equations which led him to a discovery of new methods for the solution of the latter equations.

In fact, Babbage was interested in issues concerning mathematical discovery, such as induction, analogy and generalization, since the early 1810's. From 1816 onwards he started collecting material for a work on the philosophy of analysis. As he was to point out later, his [1817] was part of this wider work which

basically remained unpublished [1864, 429; 2.9]. So, to a great extent his [1817] was a combination of his earlier enquiries in the calculus of functions and of his parallel study of the philosophy of discovery.

However, as we shall show below, Herschel's own enquiries in the calculus of functions, and particularly the equation

$$(27.1) \quad \psi^n x = x,$$

were decisive in the final shaping of Babbage's paper. From late in 1814 up to early in 1817, the two men held an ardent mathematical correspondence instances of which were used so far in order to discern the origins of their researches, their mutual assistance and the degree of their

originality and success. At this stage we will mention Herschel's hints on the analogy between (27.1) and the algebraic equation

$$(27.2) \quad z^n = 1$$

which formed one of the objects of discussion in Babbage's paper.

In what follows in this section, we will make use of Babbage's sequel of Examples [1820] in order to illustrate his extension of the method of elimination which survived up to 1860. We will also comment briefly upon the analogy between differential and functional equations, already evident in his earlier work. In the course of our study of his [1817] we will have a chance to delve a little further into his correspondence with Herschel and notice the limits of the latter's influence on him. Of special interest is their mutual interest in equations of the form (27.1) which gave rise to the analogy between functional and algebraic equations. The starting point of our discussion will be Babbage's limited study of the analogy between (27.1) and (27.2), as in his [1817], followed by Herschel's unknown so far remarks which in fact had led to Babbage's respective research.

In his [1817, 205] Babbage pointed out "the similarity of the relations of the roots of unity to the solution of the functional equation  $\psi^n x = x$ ". Let  $r_1$  be any root of (27.2) other than 1. Then it is known, he wrote, that for  $n$  a prime number  $r_1, r_1^2, \dots, r_1^{n-1}$  will all be different roots of (27.2). On the same lines, if  $\alpha x$  is a particular solution of (27.1), then  $\alpha x, \alpha^2 x, \dots, \alpha^{n-1} x$  will satisfy (27.1) for  $n$  prime. Let, for example,  $n = 3$ . Then, as  $\alpha x = 1/(1-x)$  satisfies (27.1), the following functions

$$(27.3) \quad \alpha x = \frac{1}{1-x} \quad \text{and} \quad \alpha^2 x = \frac{x-1}{x},$$

including  $\alpha^3 x = x$ , will be different solutions of (27.1) [1817, 205-6].

However, introducing an arbitrary function  $\phi$ , (27.3) will accordingly give by the transform (26.5), or

$$(27.4) \quad \psi x = \phi^{-1} \alpha \phi x,$$

the functions

$$(27.5) \quad \phi^{-1}\left(\frac{1}{1-\phi x}\right) \quad \text{and} \quad \phi^{-1}\left(\frac{\phi x-1}{\phi x}\right)$$

which are no more irreducible to each other. For if we make  $\phi x = 1/x$ , the latter of (27.5) will give  $\alpha x$ . This was a disadvantage which Babbage did not discuss any further. In addition, he expressed his doubt as to how far the generalization of  $\alpha x$ , and  $\alpha^2 x$  as given above in (27.5) contained all possible solutions [1817, 206].

He went on to make some further observations which "may throw light on the generality of the solutions of such equations". He claimed that every solution of (27.1) for  $n = 3$  or  $n = 2$  will evidently satisfy (27.1) for  $n = 6$ . Therefore, "the complete solution of  $\psi^6 x = x$  should [...] contain all forms of  $x$  which satisfy the equations  $\psi^3 x = x$  and  $\psi^2 x = x$ ". Moreover, if any function  $\alpha$  satisfies

$$(27.6) \quad \psi^3 x = \beta x,$$

where  $\beta$  a particular solution of  $\psi^2 x = x$ , it will also satisfy

$$(27.7) \quad \psi^6 x = x.$$

Indeed, putting  $\alpha^3 x$  for  $x$  in (27.6) we have

There  $\alpha^3 \alpha^3 x = \alpha^6 x = \beta \alpha^3 x = \beta \beta x = \beta^2 x = x$ , hence  $\alpha$  satisfies (27.7).  
 followed an attempt for generalization for the case  $n = abc \dots$ ,  
 where  $a, b, c, \dots$  are all prime numbers [1817, 206-7].

Equations of the form

$$(27.8) \quad \psi^n x = f x$$

were one of the main objects of discussion in the correspondence between Babbage and Herschel. Dealing with the equation

$$(27.9) \quad \psi \alpha \psi \beta y = F y,$$

Babbage wrote to Herschel that it can either be solved by means of the transform (27.4) or reduced to the form (27.8) [H.S, 2:27, 22 Sept. 1814]. In reply Herschel introduced his symbolic nota-

tion " $f^{1/n}(x)$ " for the solution of (27.8). According to this notation the solution of (27.9) in the "more reduced form"  $\phi f \phi = F$  will be

$$(27.10) \quad \phi = f^{-1}(fF)^{1/2}$$

without providing any explanations. ( Apparently he wrote:  $\phi f \phi = F \longrightarrow f \phi f \phi = fF \longrightarrow (f\phi)^2 = fF \longrightarrow f\phi = (fF)^{1/2} \longrightarrow f^{-1}f\phi = f^{-1}(fF)^{1/2}$ , hence (27.10).) (1) And in a parenthesis he added "Observe here the analogy between the functional and exponential indices" [H.S., 20:20, 25 Oct. 1814].

Herschel went on to mention that he had obtained the solution of (27.8) -where  $\psi$  now stands for  $\phi$  and  $x$  for  $y$  -as:

$$(27.11) \quad \phi(y) = {}^0A(y) + {}^1A(y) + \dots + {}^\infty A(y)$$

where " ${}^1A(y)$  is deducible from  ${}^{1-1}A(y)$  by a functional equation of the 1<sup>st</sup> order, and  ${}^0A(y)$  is derived from a particular solution of  $\phi^n(y)=y$ ". He then asked Babbage: "Is not this something analogous to the resolution of  $x^n=a$  provided we know the particular solutions of  $x^n = 1$ ".

In fact, Herschel had shown that the solution of (27.8) depends upon a particular solution of (27.1) and this led him inevitably to the observation of the relation between  $z^n=a$  and (27.2). Further on the direct analogy between (27.1) and (27.2) he commented in his [H.S., 20:28, [Nov. 6 1815]]. Criticizing Babbage's claim which followed (26.6), he stressed the necessity to obtain irreducible solutions of (27.1) when generalizing the particular ones. He thus presented the following solutions :

$$(27.12) \quad "\phi^{-1}\{a + [1]\phi(x)\}, \dots, \phi^{-1}\{a + [n-1]\phi(x)\}"$$

which are "all irreducible general solutions of  $\phi^n(x) = x$ , where  $[1], \dots, [n-1]$ , are the  $n-1$  roots of  $1 + z + \dots + z^{n-1} = 0$ " adding that "if you give  $\phi$  a particular form and take the result as a particular sol<sup>n</sup> to set out with you fall upon a general sol<sup>n</sup> comprehended in the same identical form".

It is obvious from the above quotations that Babbage's study of the similarity between (27.1) and (27.2) was largely inspired by his friend's suggestions. Herschel was often to remind Babbage both of the analogy between functional and exponential indices, as well as of the symbolic procedures he often followed in his papers and letters -as in the case of (27.10). However, Babbage was reluctant to use his friend's favourite method of separation of symbols. Before we return to Babbage's paper on analogy [1817] it would be interesting to note few occasions

where Herschel talked with admiration of this latter method<sup>(2)</sup>.

Early in 1816, anxious to hear of an analytical problem which Babbage had solved geometrically, Herschel wrote in his [H.S. 20:32, [Febr. 4 1816]]:

.. it is the language of symbols which fails us here -and as a poet may have wonderfully elevated thoughts wh. yet refuse to be embodied in words, even so I suppose the analyst may have his conceptions which pass the flaming bounds of quantity and operation.

He then proceeded to exhibit to Babbage his solution of

$$(27.13) \quad \psi \alpha \psi(x) = f(x)$$

"which depends upon a mere notation and which requires no particular solutions". Putting  $\psi = AB$  in (27.13), where  $A = \alpha^{-1}$ , and separating the variable  $x$  from the rest, he deduced

$$AB\alpha AB = f \longrightarrow AB^2 = f \longrightarrow B^2 = A^{-1}f \longrightarrow B = (\alpha f)^{1/2}$$

as in (27.10). Thus, he said,

$$(27.14) \quad \psi(x) = \alpha^{-1}(\alpha f)^{1/2}x$$

is the solution of (27.13), adding: "Now  $(\alpha f)^{1/2}$  is by def<sup>n</sup> any sol<sup>n</sup> of the eq<sup>n</sup>  $\psi^2(x) = \alpha f(x)$  and we have a direct way of solving this without knowing any particular solution" [see (24.22), (24.24)].

Herschel's comments below, regarding the Lacroix translation, motivated Babbage to react for the first time in connection with the method of separation. Herschel wrote in his [H.S. 20:35, 14 July 1816] about Lacroix:

English readers in general may be let into a few secrets which have hitherto been contraband in this country (owing of course to our laudable hatred of innovation and Napoleon Buonaparte) such as the principles of the Calcul des fonc. Generatrices ... - the method of separating symbols of operation from those of quantity, which I have done much to my satisfaction, and which I think will sell the book- the little that we know of Eq<sup>ns</sup> of diff<sup>ces</sup> (wh. Lacroix has given in a very bad way).... and also a very little about functional equations of the 1<sup>st</sup> order [...] or such simple ones as can be reduced to Eq<sup>ns</sup> of fin. diff<sup>ces</sup>.

Babbage was amused by reading this letter. In reply [H.S. 2:65, 20 July 1816] he wrote:

At first reading this I was not quite certain that you did not use the verb to sell in the technical sense in which it is

used at Cambridge; but upon further investigation this appears not to be the case. Now though this subject is very interesting to you and I thought it would give me much pleasure to see what you have said on the subject; Yet a Cambridge tutor might think it just as paradoxical as if you were to propose to him a new mode of travelling with speed by "Separating the carriage from the horses which draw it "....

From the above quotation it is pretty obvious that Babbage had not paid too much attention to Herschel's numerous emphatic remarks and illustrations of this method. From the comment that follows below, it seems moreover that he did not feel very confident with this method and this must have been another reason -besides lack of interest- for not making use of it. He told Herschel in that letter that a fellow from Caius College observed that Babbage's solution of

$$(27.15) \quad \psi x = \psi a x$$

is false since, dividing both sides of it by  $\psi$  and  $x$ , it follows that

$$(27.16) \quad 1 = a \quad (3)$$

which is obviously absurd. From the tone of Babbage's letter it is rather clear that he mentioned this erroneous demonstration as a joke. But, at the same time, he wouldn't run the risk to apply a method whose principles were rather shaky at that time<sup>(4)</sup>.

Returning to the issue of analogy - as discussed in Babbage [1817]- we will illustrate his method of vanishing fractions for those cases of equations of the form (25.33) in which elimination had been impossible; namely for equations which are either "homogeneous relative to the different forms of the unknown function", or those "which are symmetrical relative to the same quantities" [1817, 203]. In that paper he demonstrated his method encapsulated in the principle "Whenever the method of elimination apparently fails, the real value of the vanishing fraction will give the general solution of the equation", mainly via the equation

$$(27.17) \quad \psi x + f x . \psi a x = f_1 x$$

where  $f, f_1$  known functions and where

$$(27.18) \quad a^2 x = x$$

[1817, 199-202] <sup>(5)</sup>.



We will illustrate Babbage's method with two examples that appeared in his [1820] but which are also briefly mentioned in his [1817]. The first concerns equation

$$(27.19) \quad \psi x = \psi \frac{1}{x} ,$$

which is of the form

$$(27.20) \quad F(\psi(x), \psi(\alpha(x))) = 0 ,$$

where  $\alpha(x) = 1/x$  and  $\alpha^2(x) = x$ . Moreover, (27.19) is symmetrical relative to  $x$  and  $1/x$  so substitution of  $\alpha(x)$  for  $x$  will again give the same equation (27.19). By assuming

$$(27.21) \quad \psi x = a\psi \frac{1}{x} + b ,$$

Babbage in fact rendered (27.19) non-symmetrical. Now, elimination of  $\psi(1/x)$  is possible if we interchange  $x$  and  $1/x$  in (27.21) giving

$$(27.22) \quad \psi x = \frac{b}{1-a} .$$

The only value of  $\psi x$  which satisfies (27.19) will be deduced from (27.22) if we make  $a = 1$  and  $b = 0$  [for then (27.21) is reduced to (27.19)]. For these values,  $b/(1-a)$  becomes a "vanishing fraction" whose value is "any constant"  $c$  [1820, 10].

In order to find a more general solution of (27.19) than  $\psi x = c$ , Babbage let by analogy from (27.21),

$$(27.23) \quad \psi x = a\psi \frac{1}{x} + v\phi x$$

introducing thus an arbitrary function  $\phi$ . On similar lines as above, elimination of  $\psi(1/x)$  from (27.23) will give us

$$(27.24) \quad \psi x = (a\phi \frac{1}{x} + \phi x) \cdot \frac{v}{1-a^2} .$$

It suffices to determine the fraction  $v/(1-a^2)$  for  $v=0$  and  $a=1$ . Regarding  $a = 1 + 0$  he deduced in a rather unrigorous way<sup>(6)</sup>

$$\frac{v}{1-a^2} = \frac{0}{1-(1+2.0+0^2)} = \frac{0}{-2.0+0^2} = \frac{1}{-2+0} = -\frac{1}{2} .$$

Changing finally the function  $\phi$  in (27.24) he obtained

$$(27.25) \quad \psi x = \phi x + \phi \frac{1}{x} = \chi \left( x, \frac{1}{x} \right) ,$$

where  $\chi$  an arbitrary symmetrical function of  $x$  and  $1/x$  [1820, 10-11].

It is of interest to notice that (27.19) is also of the form (25.5) whose general solution was already deduced in his [1815] to be (25.14) or  $\psi x = \bar{\chi}(x, \alpha(x))$  which in fact coincides with (27.25) without the complications required above. However, the method illustrated above for symmetrical equations, is the only general method in his theory so as to deal with symmetrical equations or with those which are homogeneous relative to the forms of  $\psi$ .

Let us briefly see another application of this method; this time to the equation

$$(27.26) \quad (\psi x)^2 + \left[ \psi \left( \frac{n}{2} - x \right) \right]^2 = 1$$

which is the equation (14.30) that concerned the problem of decomposition of forces. Letting  $\psi_1 x = (\psi x)^2$  he claimed that (27.26) is a case of the more general equation

$$(27.27) \quad \psi_1 x + a \psi_1 \left( \psi \left( \frac{n}{2} - x \right) \right) = 1 + u \psi x.$$

Eliminating  $\psi_1 \left( \frac{n}{2} - x \right)$  as in (27.23) he arrived at

$$(27.28) \quad \psi_1 x = \frac{1}{1+a} - \frac{\phi x - a \phi(n/2 - x) \cdot u}{1 - a^2}$$

which was determined for  $a = 1$  and  $u = 0$  as in (27.24). Changing  $\psi_1 x$  in  $(\psi x)^2$  and  $\phi x$  in  $2\phi x$  he got from (27.28) that

$$(27.29) \quad \psi x = \left[ \frac{1}{2} \phi x + \phi \left( \frac{n}{2} - x \right) \right]^{1/2},$$

where  $\phi$  is arbitrary [1820, 14-15] ('?').

Babbage's method, illustrated by (27.26), remained in fact prominent up to 1860 when Boole noticed that for the case of symmetrical equations either the above method applies or a more general method like that by Laplace<sup>docs</sup> [1860, 224]. According to Laplace's method the solution of (27.26) was deduced in 1.4 to be

$$(27.30) \quad \psi x = \left[ 1/2 + C \left( x - \frac{n}{4} \right) \right]^{1/2},$$

where  $C$  an arbitrary symmetrical function of  $x$  and  $\pi/2-x$ . Boole proved that solution (27.29) is included in (27.30) [1860, 226-7]. In fact, by another method, only hinted at in his [1817], Babbage claimed that the solution of (27.26) will be found to be

$$(27.31) \quad \psi x = \left\{ \frac{1}{2} + \left( 2x - \frac{\pi}{2} \right) \chi \left( x, \pi/2-x \right) \right\}^{1/2}$$

which, under a slight modification of  $\chi$ , is exactly (27.30) [1817, 209].

The solution (27.31) was mentioned to Herschel in Babbage's letter [H.S, 2:70, (?) Jan. 1817]. In reply Herschel wrote: "When we meet we will talk about your sol<sup>n</sup> of [the] Eq<sup>n</sup>  $(\phi(x))^2 + (\phi(\pi/2-x))^2 = 1$  -it seems a most unpleasant one" [H.S, 20:38, [Jan. 30 1817]]. Herschel's remark sounds a bit unfair for, had he had solved equation (27.26) by Laplace's method, he would have arrived at a solution of that form. In general Herschel sounded a bit hostile towards Babbage's paper [1817] entitled: "Observations on the analogy which subsists between the calculus of functions and other branches of Analysis". Three months later he wrote to him:

I see a new paper of yours in the Phil. Trans. mentioned in Tilloch: you have chosen a [very] queer subject, I should as soon have thought of writing a paper on the use of gunpowder in mathematical reasoning as analogy or induction or whatever it is<sup>(8)</sup>.

In 1818 Herschel commented further on Babbage [1817]. He wrote in his [H.S, 20:53, 10 March 1818]:

I observe in your third paper (on Analogy) that in the solutions you give of  $\psi x + \phi x = f_1 x$  and  $\psi x - \phi x = f_1 x$ , one contains one arbitrary function (the former) and is not the complete solution. The other appears to contain two, and I believe is complete, but yet the complete solution cannot contain more than one irreducible arbitrary function in either case<sup>(9)</sup>.

Herschel went on to discuss

$$(27.32) \quad \phi x + \phi \phi x + \dots + \phi \phi^{n-1} x = 0,$$

ending with the following suggestion: "I wish you would think about the complete solutions of functional equations."

It is true that Babbage attempted to delve into the generality of the solutions of functional equations in general in

his [1817] but he did not meet the standards expected by his friend and, apart from the extension of his method of elimination, the rest of his paper hardly deserves any notice, with the exception of another paradox which will be mentioned in the end of this section.

Drawing on <sup>the</sup> analogy between partial differential and functional equations in many variables he distinguished

1. The complete solution which contains as many arbitrary functions as the nature of the given functional equation will admit,
2. The particular case which contains solutions that are less general than the complete, and
3. The particular solution which may or may not contain arbitrary functions [1817, 210 ; Dubbey 1978, 81].

Again based on the techniques for the solution of differential equations he said that by means of a particular solution, a linear functional equation with variable coefficients of the form  
 (27.33)  $F\psi x + Ax F\psi ax + Bx F\psi a^2x + \dots + X = 0$   
 can be reduced to an equation devoid of the last term  $X$  [1817, 208-9]. This technique, as we saw in 2.5, was already applied in his [1815] for the solution of the equation (25.25) (10).

Another analogy worth of notice concerns the method of integrating factor which renders a differential equation a complete differential. He illustrated the analogous method for the solution of various functional equations. We will focus, however, on the first case, namely of (27.17), or,

$$(27.34) \quad \psi x + fx.\psi ax = f_1x, \quad a^2x = x.$$

He multiplied (27.34) by  $\phi x$ , observing that the first side,

$$(27.35) \quad \phi x \psi x + \phi x.fx.\psi ax$$

will be symmetrical relative to  $\psi x$  and  $\psi ax$ , if we make

$$(27.36) \quad \phi x = fax.\phi ax.$$

Babbage observed that (27.36) is impossible unless

$$(27.37) \quad fx.fax = 1.$$

Indeed, substituting  $ax$  for  $x$  in (27.36) we have

$$(27.38) \quad \phi ax = fa^2x.\phi a^2x = fx.\phi x$$

which, combined with (27.36) leads to (27.37). He went on straight after (27.37) to say that [a solution of (27.36) is]

$$(27.39) \quad \phi x = \frac{1}{(fx)^{1/2}}$$

without any explanations. If we multiply (27.36) by  $\phi x$  -and

regard that  $\phi$  also satisfies (27.37)- then we obtain  $(\phi x)^2 = fax$ , hence  $\phi x = (fax)^{1/2} = (1/f(x))^{1/2}$  or (27.39). So, (27.39) is indeed a particular solution of (27.36) and for this value of  $\phi$  since the left-hand side of (27.34), (27.35) becomes symmetrical, its right-hand side

$$(27.40) \quad f_1 x / (fx)^{1/2}$$

must also be symmetrical, therefore equal to  $f_2(x, \phi x)$ . Having rendered (27.34) symmetrical, it rested to solve it by the method of vanishing fractions [1817, 213-214] (11).

Our next remark concerns an equation which, like the case of (26.50), seemed not to admit of any solution, namely

$$(27.41) \quad \psi x = c\psi - \frac{1}{x}$$

which is contradictory unless  $c = \pm 1$ . Indeed, if we substitute  $1/x$  for  $x$  in (27.41) and multiply the equation thus deduced with (27.41) we arrive at  $c = \pm 1$ .

However, if we solve

$$(27.42) \quad \psi x = c\psi x^n$$

by Laplace's method we shall find, wrote Babbage,

$$(27.43) \quad \psi x = c \frac{-\log \log x}{\log^n}$$

Hence, for  $n = -1$  we have the solution of (27.41) by (27.43) for any value of  $c$  [1817, 211-12].

Babbage tried to explain this paradox by assuming that  $\psi x$  represents "an inverse operation which admits of several values". Hence, (27.41) can be possible only if we take different forms of  $\psi$  for different parts of the equation. "This solution may perhaps appear unsatisfactory, it is however only proposed as one which deserves examination, and I shall be happy if its insufficiency shall induce any other person to explain more clearly a very difficult subject" [1817, 212].

This paradox was beyond Babbage's or Herschel's powers to be explained. As the reader may expect it was De Morgan who first attempted to clarify it in his [1836, art. 72]. However, a full account -including Babbage's and De Morgan's respective statements- was given by J. Graves in 1836. Graves delved further than De Morgan in matters of discontinuity and limits. Omitting his lengthy discussion we will confine to his remark: that Babbage put forward a "remarkable Paradox" which amounts

briefly to the vaguely stated then perception that functional equations could not be solved solely by algebraic methods<sup>(12)</sup>.

The fact that Herschel found the subject of Babbage's paper a "queer" one is a bit surprising, since he had been the one to stress so often issues of analogy to Babbage. However, the truth is that Babbage's [1817] was not very successful in its presentation and too many valuable explanations were omitted as well as detailed illustrations which are to be found only in his Examples [1820]. What above all is surprising, is that while Babbage was the one to be obsessed with functional equations, Herschel did not cease to discuss related topics in his letters including issues of the calculus of variations and an extension of the transform  $\psi x = \varphi^{-1} f \varphi$  beyond the calculus of functions. Herschel's work during the period 1817-1822 will be the object of discussion of the next section.

## 2.8 Herschel's mathematical work: 1817-1822.

Around July 1816 Herschel set off to compose a book on algebra. Unsure about its title he wrote to Babbage that the book "is intended to be a complete course of the essential part of the pure analysis" [H.S, 20: 35, 14 July 1816]. Late in that year he got engaged with Spence's manuscript essays, which were in an unfinished state, and undertook to edit them [H.S, 20:37, 24 Dec. 1816]. Stimulated by Spence's work, he became active again in mathematical research engaged with his work on algebra, the edition of Spence's essays, finite difference and functional equations, exponential functions and Encyclopedia articles<sup>(1)</sup>.

Herschel was delighted with Spence's work. Early in 1817 he wrote to Babbage:

Spence's paper's have set me mad [...]. I ... struck upon an unfinished Essay full of the most beautiful properties of strange transcendents of the form

$\int dx/x \int dx/x \dots \int dx \varphi(x)$  analogous to the general properties of  $\log t$

transcend<sup>te</sup>. I devoured the Essay with avidity -The field it opens is immense. I mean to recommend its publication and everything else on [the] same subject I can find, in the strongest terms- Meanwhile an idea has struck me by which I can at last come in contact with your investigations, and by connecting the theory of integral

transcendants with that of functional equations. I hope to be able to prove the real and extensive utility of the latter theory<sup>(2)</sup>.

While he wrote that letter he obtained a "very beautiful" theorem which he communicated to his friend. Let  $\alpha$  be a function such that

$$(28.1) \quad \alpha^n(x) = x$$

(Herschel was soon to call such a function "periodic" - see (5) below). Then, if  $D=d/dx$ , he claimed that the theorem

$$(28.2) \quad D\alpha(x)D\alpha(\alpha x) \dots D\alpha(\alpha^{n-1}x) = 1$$

might be useful in Babbage's theory of differential functional equations. He added that the proof of (28.2) for  $n=2$  comes out "very elegantly" from the equation

$$(28.3) \quad \bar{\chi}(x, \alpha(x)) = 0.$$

In fact, the "theorem" (28.2) can be obtained directly from (28.1) by differentiation. Let  $n=2$ . Then it follows that

$$(28.4) \quad \frac{d\alpha^2 x}{dx} \frac{dx}{d\alpha x} = \left[ \frac{dx}{d\alpha x} \right] = 1,$$

or, in Herschel's notation,

$$(28.5) \quad D\alpha(\alpha x)D\alpha(x) = 1.$$

What could be further implied from (28.2), and which Herschel omitted at this stage to say, is that  $\alpha'$  or  $da/dx$ , where  $\alpha$  satisfies (28.1), is a solution of the equation

$$(28.6) \quad \varphi x. \varphi \alpha x. \dots \varphi \alpha^{n-1} x. \varphi \alpha^n x = 0 \quad (3).$$

In March 1817 Herschel announced to Babbage that he had obtained the general solution of the equation

$$(28.7) \quad \psi(x) + \psi(\alpha x) + \dots + \psi(\alpha^{(n-1)}x) = c = n\psi(k)$$

in the integral form

$$(28.8) \quad \int dx \bar{\chi}(x, \alpha x, \dots, \alpha^{n-1}x) F(\alpha x, \alpha' x, \dots, \alpha^{(n-1)}x),$$

where  $F$  a known function and  $\chi$  arbitrary. He added in his letter: "There are besides quantities of functional equations of which I have got solutions by similar methods"<sup>(4)</sup>.

Spence's Mathematical essays were edited in 1820. At [1820, xxviii] Herschel mentioned that a particular case of (28.7) was found among Spence's unfinished essays. In fact, Spence had been interested in functional equations in his early work and used Lagrange's method of series [See (22.5)]. In a similar manner an even more general form of (28.7) can be solved by the series

method [De Morgan 1836, art. 236]. In the Notes appended to these essays, Herschel referred to his own solution of equation (28.1) for  $n=2$  [1820, 153] and then acknowledged Babbage's notation and "ingenious methods" in various instances [1820, 154-9] (5).

He ended these Notes with the enquiries he had made in respect with (28.1)-(28.2). Extending his study of periodic functions he proved that

$$(28.9) \quad a''(x)/a'(x),$$

where  $a'(x) = da(x)/dx$ , is a particular solution of the differential equation

$$(28.10) \quad \frac{d}{dx} [\varphi(x) + \varphi(a(x)) + \dots + \varphi(a^{n-1}(x))] = 0$$

[1820, 166; See also De Morgan 1836, art. 219].

Early in 1817 Herschel was engaged with another enquiry worth noting. The stimulus came from a paper on the calculus of factorials by Bromhead which was sent to him by Babbage. The latter's letter contained Bromhead's theorem

$$(28.11) \quad f(xe^h) = f_x \times (Pf_x)^{\frac{h}{1}} \times (P^2f_x)^{\frac{h^2}{1 \cdot 2}} \times \dots$$

where 'x' is the symbol of multiplication, followed by these comments: "Bromhead has sent me the enclosed paper and wished you to look at it. It is very systematic and the analogies are very pretty. The theorem [(28.11)] is a curious one. I have got a very good proof of it from Taylor's theorem which I will not send you as you may easily investigate it yourself" [H.S. 2:76, 29 March 1817] (6).

Herschel responded immediately incorporating his investigations and related comments in two letters. In the first [H.S. 20:41, 3 April 1817] we read:

I rec<sup>d</sup> yesternight [...] your parcel containing Bromhead's paper which I should like better if it could not be derived in toto from the known theory of the diff<sup>l</sup> calculus, and if I had not broken short off about 3 years ago in a similar investigation for that very reason, viz. that no result can be deduced from the factorial that cannot be deduced from the diff<sup>l</sup> calculus [...]. I wonder this has not struck Bromhead. Your remark on Taylor's theorem is perfectly correct and contains I think the true view of the subject.



Herschel went on to take the  $\log_{arithm}$  of both sides of (28.11), providing the definition of the symbol P of factorial derivation via log and D. Since, as he himself had admitted, his procedures in that first letter were slightly erroneous we will provide the right definition of P as given in his second letter of 16 April, namely, that if  $D=d/dx$ , then:

$$(28.12) \quad Pf(x) = \log^{-1} D(\log f \log^{-1}) \log x$$

[H.S. 20:42, 16 April 1817]. As the version of (28.12) in his previous letter had the same form we return to that letter to see Herschel's comments. The form (28.12) strongly reminded him of Babbage's transform  $\phi^{-1} f \phi$ . He thus wrote in [H.S. 20:41, 3 April 1817] straight below (28.12):

...by which by the way the use of your substitution  $\phi^{-1} f \phi$  appears not to be limited to the soln of functional equations, and I already see new applications and new modifications of it rising up on every side. It seems to point to a functional calculus among the relations of quantity instead of quantities themselves which will beat Bromhead in generality....

In the second letter he incorporated the general formal theory which was an outcome of the formula (28.12) deduced directly from the given theorem (28.11). "Let D denote any arbitrary law of derivation, i.e. let Df denote any arbitrary change in the nature of not contradictory to itself and let P denote another law of derivation dependent on D by the equation":

$$(28.13) \quad Pf = \psi^{-1} D(\psi f),$$

"or  $Pf(x) = \psi^{-1} D(\psi f)(x)$ , D being regarded as operating on the symbol of operation which immediately follows it ...." [see H.S. 20:42, 16 April 1817; on the laws concerning D see his 1814 in 2.4]. He then provided the iterated values of (28.13) for  $P^n$  and  $D^n$  and next assumed the relation

$$(28.14) \quad Pf(x) = \psi^{-1} D(\psi f \psi^{-1}) \psi(x)$$

which readily gives

$$(28.15) \quad P^n f(x) = \psi^{-1} D^n(\psi f \psi^{-1}) \psi(x).$$

He observed that "If  $\psi$  denotes log and D denotes differential, P will stand for Bromhead's Factorial derivation. I have deduced all his results from his equation [(28.12)]".

In the postscript of his second letter, Herschel distinguished as in his [1814] between symbols of derivation D,  $\Delta$  which "operate on the form of function" and  $\phi$ ,  $\psi$  "symbols of operation

(performed on quantity being understood)". He added "I think the derivation P and D may be called correlative. If D denote a symbol of operation, P does so too. In this case  $D(pqrf\psi\theta\chi) = Dpqrf\psi\theta\chi$  and the general correlative equation [(28.15)] becomes

$$[(28.16)] \quad Pnf = \varphi^{-1} Dn\varphi f \text{ or } Pn = \varphi^{-1} Dn\varphi$$

which is your old substitution. Thus you see, this calculus of derivations expanded will include your calculus of functions as a particular case. Think of this."

Bromhead's paper on factorials was not published, for reasons partly explained <sup>below</sup>. However, around 1817, Bromhead contributed a short article on the "Differential calculus" for the Encyclopedia Britanica cited as [1824]. This article included a sketch for the proof of (28.11) [See (6) above] followed by these comments [1824, 572]:

... a theorem entirely analogous to that of Taylor, and presenting a factorial calculus on principles similar to the differential. It is not, however, necessary to deduce the values of  $Pf(x)$ , by making it the subject of distinct investigations, since Mr. Herschel, on seeing the formula [(28.11)], has discovered an expression for  $Pf(x)$  by means of  $Df(x)$  [(28.12)], and observes, that the factorial calculus so harmonizes with the differential, that either may be established, and the other deduced from it.

Thus, due to Herschel's wider investigations in respect with Bromhead's factorial calculus, the latter's were unfortunately never published<sup>(7)</sup>. Moreover, neither Herschel's own researches were published. Besides generalizing Bromhead's results, Herschel effected an application of Babbage's transform  $\varphi^{-1}f\varphi$  outside the realms of the functional calculus. This contribution, foreshadowing Murphy's and Gregory's application of similar operator transforms in the domain of the calculus of operations and differential equations [3.3, 4.3], remained unknown in 19<sup>th</sup> century in England up to our days by being concealed in his own manuscripts and in a few letters cited above.

Herschel was delighted with his discoveries and tried to stimulate Babbage's interest in the calculus of functions which had faded by that time. In his [H.S, 20:47, 1 Aug. 1817] he wrote: "As to the application of the calculus of variations to functional properties, I have done the business, I believe, completely, but as I expected, I am quite at a loss to interpret the

results obtained. They give arbitrary functions in the solutions and what this means I cannot comprehend." He went on to discuss the problem

$$(28.17) \quad \int dx F(x, \phi x, \phi ax) = \text{Max or Min}$$

which he reduced to a differential functional equation and tried to explain certain contradictions which were presented. His letter ended by: "I have new views of all things in my head [.....] and I want to have a conference about this calculus of variations, as well as twenty other functional things".

Late in 1817, Herschel conceived the idea of "circulating functions" and produced a general method for integrating a class of finite difference equations with these functions as coefficients [H.S. 20: 51, 24 Nov. 1817]. A "circulating function",  $P_x$ , was defined as

$$(28.18) \quad P_x = aS^{(n)}_x + bS^{(n)}_{x-1} + \dots + kS^{(n)}_{x-n+1},$$

where  $S^{(n)}_x$  stood for

$$(28.19) \quad \frac{a^x + b^x + \dots + k^x}{n},$$

and where  $a, b, \dots, k$  were the roots of  $z^n - 1 = 0$  [1818, 147-149]. He suggested this definition by reasoning as follows: "If we give to  $x$  [in (28.18)] the several values 0, 1, ... to infinity, in succession, the first  $n$  values of  $P_x$  will be in their order  $a, b, c, \dots, k$  after which the same set of quantities will be reproduced in the same order by continuing the substitution, and so on to infinity. The function  $P_x$  may be called in this case a circulating function, and the same name (with less propriety however) may be extended to the case when the coefficients are variable. The system of the coefficients  $a, b, c, \dots, k$  may be called a period [1818, 149]. Index  $(n)$  in  $S^{(n)}_x$  was called the "period of circulation", and it was in fact omitted in the definition (28.18), as assumed.

Symmetry and condenseness were the outstanding characteristics of Herschel's symbolism. His notation-drawing slightly on Babbage's own method for denoting symmetry in his functional calculus - was to be praised highly in the latter's [1827, 345] in the course of discussing the importance of the "principle of representing any one quantity indifferently out of a given number".

Omitting explanatory details, instances of Herschel's original notation are the following

$$(28.20) \quad \ddot{S}_x^{(p)}, P_{x,y}^{(m,n)}, \alpha_{x,y}^{(1,0)} S_{x-1}^m S_y^{(n)}$$

[1818, 150-3, 164-5].

The basic problem solved in this paper was the integration of the "circulating equation"

$$(28.21) \quad u_x + {}^1P_x u_{x-1} + {}^2P_x u_{x-2} + \dots + {}^mP_x u_{x-m} = {}^{m+1}P_x$$

where  $u_x$  the unknown function under determination and  ${}^1P_x$  circulating functions, defined by (28.18), of circulation period equal to  $n$  [1818, 154-9]. This method resulted from a combination of Herschel's early work on functional and finite difference equations and his study of Laplace's work on recurrent series. Laplace is in fact mentioned in the course of the lengthy and complicated solution of problem (28.20) [1818, 155], but no specific reference is provided.

The lack of illustration of his method by examples is striking. In fact Herschel admitted this by saying: "I am unwilling to occupy the pages.... with examples of the application of the processes here delivered to the various problems in pure and mixed mathematics where they afford either a remarkable simplicity in the result, or great neatness in the investigation. Such instances occur frequently in the evaluation of continued fractions and other similar functions where the denominator (or other elements) recur in a certain order" [1818, 166]. Concluding his reasoning he decided to subjoin merely one example "of the integration of a circulating equation of the second order, with constant coefficients, by way of illustration of the methods themselves". This example concerned the solution of the equation

$$(28.22) \quad u_x - (aS_x + bS_{x-1})u_{x-1} + (aS_x + bS_{x-1})u_{x-2} = 0$$

where the circulating period of  $S_x$  is 2 [1818, 166-168].

Before switching from pure mathematics to other interests, Herschel contributed another paper on exponential functions and summation of series in the Edinburgh Philosophical Journal in 1820, four problems in the Repository in 1819 and finally a paper "On the reduction of certain classes of functional equations to equations of finite differences" in the Transactions of the Cambridge Philosophical Society in 1820, published as [1822] -see Enros [1979, 196-7]. In what follows we will confine to his [1822] giving an account of the most representative results.

Herschel considered first the problem of finding the "most general form" of a function  $\psi(x,y)$  such that

$$(28.23) \quad \psi(x,P)=\psi(x,Q),$$

where  $P$  and  $Q$  are two given functions of  $x$ . By very simple considerations -fairly close to those followed by Babbage in the simple problems of his [1815] and [1816]- he was led to the solution

$$(28.24) \quad \psi(x,y)=f(x)+(y-P)(y-Q)\chi(x,y)$$

where  $f$  and  $\chi$  are arbitrary functions. Verification was suggested by substitution of (28.24) in (28.23) [1822, 78].

Two more cases, such as (28.23), followed and then Herschel's methodology was put into action. He proposed the solution of the functional equation

$$(28.25) \quad \psi(x,x)-\psi(x,0)=a.$$

Assuming

$$(28.26) \quad \psi(x,y) = \phi(x, h + \frac{y}{x})$$

the proposed equation was immediately reduced to the finite difference equation

$$(28.27) \quad \phi(x,h+1)-\phi(x,h)=a$$

which integrated gives  $\phi(x,h)=ah+C$ , where  $C$  "any function which does not change when  $h$  changes to  $h+1$ ". Applying the considerations of problem (28.23), he claimed that, despite any objections probably raised against the "generality" or "legitimacy" of his method, the solution of (28.25) is

$$(28.28) \quad \psi(x,y) = a\frac{y}{x} + f(x) + y(y-x)\chi(x,y),$$

where " $a$ " constant and  $f, \chi$  arbitrary functions [1822, 79-81].

Then followed the case

$$(28.29) \quad F(x,\psi(x,P), \psi(x,Q))=0.$$

further generalized in the next problem for any number of given functions  $P, Q$  etc of  $x$ . Assuming

$$(28.30) \quad \psi(x,P)=\phi(x,h) \text{ and } \psi(x,Q)=\phi(x,h+1),$$

equation (28.29) was reduced again to a finite difference equation in  $\phi$  in an independent variable  $h$ . The whole difficulty of the problem was how to determine  $\psi(x,y)$  from (28.30) after  $\phi$  was obtained.

Herschel conceived of an auxiliary function  $\theta(x,y)$  such

that when  $y=P$  it will vanish, and when  $y=Q$  it will become unity. By the same consideration as in problem (28.23), he arrived at the final result

$$(28.31) \quad \psi(x,y) = \varphi(x, h+\theta(x,y), C)$$

where  $\varphi(x,h,C)$  the integral of the finite difference equation  $F(x, u_n, u_{n+1})=0$ .  $\theta(x,y)$  determined as

$$(28.32) \quad \theta(x,y) = \frac{y-P}{Q-P} + (y-P)(y-Q)\chi(x,y)$$

and  $C$  as

$$(28.33) \quad C = f(x) + (y-P)(y-Q)\chi_1(x,y),$$

where  $f, \chi, \chi_1$  arbitrary functions [1822, 81-83].

For the general case of (28.29) for any number of given functions  $P, Q, R, S$  etc, Herschel appealed to "a very elegant theorem proposed by Lagrange as a formula of interpolation" in order to define  $\theta(x,y)$ . We omit the lengthy calculations since the procedure is identical with that followed in the previous case [1822, 83-5]. Herschel examined one more case, the solution of

$$(28.34) \quad F(x, \psi(\alpha x, \beta x), \psi(\alpha_1 x, \beta_1 x), \dots) = 0$$

where  $\alpha, \beta$  etc given functions of  $x$ . Assuming for  $\psi(x,y)$  the form (28.31), the problem was reduced to the determination of  $\theta(x,y)$  via  $\theta(\alpha x, \beta x)=0$  and  $\theta(\alpha_1 x, \beta_1 x)=1$ . Putting  $\alpha^{-1}x$  for  $x$  and so on in the above equations, the problem was reduced to the general case of (28.29) [1822, 85].

We notice that Herschel omitted any reference to the probable multiplicity of values of  $\alpha^{-1}$ , which is rather surprising at this advanced stage of his work. For intricacies regarding inverse functions were already discussed in Babbage's paper [1816] to which he referred as a stimulus for his present work [1822, 77].

As with his [1818], this paper ends with a unique example in which his method was applied. The proposed equation being  $\psi(1/x, x) = 2\psi(x, x^2)$ , the solution was found to be  $\psi(x,y) = C2^{\theta(x,y)}$  where

$$\theta(x,y) = \frac{x(y-x^2)}{1-x^3} + (xy-1)(y-x^2)\chi(x,y) \text{ and } C = f(x) + (xy-1)(y-x^2)\chi_1(x,y),$$

according to (28.32) and (28.33) respectively. The simplest function which satisfied the above given condition was

$$\psi(x,y) = 2(xy-x^3)/(1-x^3)$$

if  $x_1$  are regarded to vanish and  $f(x)$  equals unity [1822, 86-7].

Herschel conceived the scheme of his last work, which in fact is a combination of Babbage's work on functional equations in two variables -as in the specific cases where  $y=x$  [see (26.41)]- and Herschel's own interest in finite difference equations, early in 1818 [H.S. 20: 55, 17 May 1818]. Since then, both he and Babbage produced very little mathematical work. One of the main reasons for their lack of interest in analysis -which had characterized most of their early work- was the fact that their general and abstract extension of French mathematics was not favourably accepted. At least not from Barlow, who most probably was the reviewer of their essays on analysis. Barlow did acknowledge Herschel's analytical efficiency but had a different vision of mathematics than that of the members of the Analytical Society. The following is a quotation from the review of Herschel's [1818]:

....we believe it to be impossible within any moderate limits to render his processes intelligible to our readers. We have some doubt indeed, whether the memoir itself would be sufficient for its purpose<sup>(8)</sup>.

Herschel included some elementary applications of his circulating functions in his Examples [1820, 137]. But neither the theory of his [1818], nor that of his [1822] were to be reproduced in later works or further studied by English analysts. The only noticeable exception is once more De Morgan. His huge article on the calculus of functions [1836] comments upon the most representative cases tackled in Herschel's and Babbage's papers and books studied so far, including references to their Memoirs<sup>(9)</sup>.

By 1818 the fruitful correspondence between Herschel and Babbage faded away. Very minor references are to be found in their letters in connection with their mathematical work during the period 1818-1822. In fact, by 1822 they both ceased working in mathematics. Herschel worked mainly in astronomy, and in 1820, together with Babbage, they helped to found the Astronomical Society of London [Enros 1979, 198]. The main reason for their lack of interest in mathematics was "their inability to find positions which could have fostered their mathematical interests

and the hostile reception their mathematics was given" (Enros 1979, 208]. In fact, mathematics was not a profession in early-19<sup>th</sup> century England, to the great disappointment of the reformers of Cambridge mathematics<sup>(10)</sup>.

## 2.9 Babbage on the language of the calculus of functions: 1816-1822.

Prior to his entrance at Trinity College in Cambridge, Babbage took a course in classical studies under the guidance of an Oxford tutor. He recalls in his Passages from the life of a philosopher [1864] that having heard accidentally "of an idea of forming a universal language" he decided "to write a kind of grammar, and then to devise a dictionary" [1864, 25-6]. He added:

Some trace of the former, I think, I still possess: but I was stopped in my idea of making a universal dictionary by the apparent impossibility of arranging signs in any consecutive order, so as to find, as in a dictionary, the meaning of each when wanted. It was only after I had been some time at Cambridge that I became acquainted with the work of "Bishop Wilkins on Universal Language"<sup>(11)</sup>.

Babbage was not the only English mathematician, in the period under study, who had been interested in both ordinary and mathematical language. Boole will be another vivid example, as we shall see in chapters 4, 7 and 8. Unfortunately we do not have any further information on Babbage's early linguistic or other epistemological influences, apart from the few hints included in his [1864]. As far as his calculus of functions is concerned, we read in his [1816, 179-180] that he viewed it as an "instrument of discovery in the more difficult branches of analysis". He held that in a more mature state, this calculus "shall unveil the hidden laws which govern the phenomena of magnetic, electric, or even of chemical action". In this respect Babbage reminds us of a fairly similar position held by Laplace in connection with his generating functions [1.5, 1.7, 1.8].

Babbage concluded his second essay [1816, 256] by claiming: ...the doctrine of functions is of so general a nature, that it is applicable to every part of mathematical enquiry, and seems eminently qualified to reduce into one regular and uniform system



the diversified methods and scattered artifices of the modern analysis; from its comprehensive nature, it is fitted for the systematic arrangement of the science, and from the new and singular relations which it expresses, it is admirably adapted for further improvements and discoveries.

This quotation gives evidence of his tendency towards a kind of unity via abstraction, generalization and systematization. In this sense we could suggest that he viewed the language of the calculus of functions as a kind of universal language of the mathematical and physical sciences. It would be unfair, though, not to mention that Herschel shared a fairly similar view in respect with the D- operator calculus which, as he claimed in a letter, included in fact Babbage's functional calculus<sup>(2)</sup>. Taking under consideration our previous enquiries, particularly [2.4, (8)], we see that Babbage's tendency for abstraction and generalization was motivated by Herschel's early studies of the finite difference and functional calculi. This tendency was to revive remarkably in the cases of Murphy, Gregory and Boole in the late 1830's [Chapters 3-4].

Another passage from Babbage's book [1864, 428-9] offers us some further hints on his early concerns in the philosophy of signs to which we referred briefly in 2.7. He wrote that while at Cambridge he directed his reading to the original papers of the great discoverers in mathematics so as to trace the course of their minds and to "observe whether various artifices could not be connected together by some general law". He went on to say:

The writings of Euler were eminently instructive for this purpose [...]. It appeared to me that the highest exercise of human faculties consisted in the endeavour to discover those laws of thought by which man passes from the known to that which was unknown. It might with propriety be called the philosophy of invention. During the early part of my residence in London, I commenced several essays on Induction, Generalization, Analogy, with various illustrations from different sources. The philosophy of signs always occupied my attention [...]. Most of the early essays I refer to were not sufficiently matured for publication, and several have appeared without any direct reference to the great object of my life. I may, however, point out one of my earlier papers in the "Philosophical Transactions for 1817".....

As we mentioned in 1.8, Euler's writings were considerably influenced by Condillac's semiotic philosophy. Most probably Babbage had read the same books by Euler as Herschel did in the early 1810's. It is interesting to record the latter's view of Euler in 1813. He wrote to Babbage:

The Exercises [sic] du Calcul Integral I find a most useful work, particularly in what regards Euler's definite integrals. For I confess that I find Euler too long-winded. It's particular cases worked out before he comes to the general one, however well adapted to lead the reader on step by step to the spirit of his method, clog the progress of a reader who is not wholly unacquainted with that method, for which reason I prefer the more systematic arrangement and condensed form of the modern publications<sup>(3)</sup>.

Herschel's comment on Euler reminds us vividly of Gergonne's comment upon Lacroix's work on algebra [see 1.8]. It also sheds some further light on what Babbage might have admired in the mathematical presentation in Euler's books. All the issues hinted at so far, such as analogy, generalization and induction, remind us of our discussion in 1.8 on the connection between French mathematics and semiotics at the turn of the century. Parenthetically, Babbage's appeal to the discovery of the "laws of thought...." in the previous passage provides us with a link between our present study and our further discussion in 8.9 on the analogous aims of Boole and Grättry<sup>(4)</sup>.

But focusing now only to our work so far, a question arises: Was Babbage at all influenced by French semiotic philosophers? As we shall see below, he was to talk with admiration of Degerando in an essay written around 1820. In that essay, [1827], he did also refer to another work which was apparently written under the prevailing influence of late-18<sup>th</sup>-century semiotics in France. However, we have no evidence as to when Babbage became acquainted with French semiotics, which books he read or the extent of any influence derived from such works.

From his work on the philosophy of signs only two essays—beside his [1817]—were published [see (8) below]. The first was titled "Observations on the notation employed in the calculus of functions" [1822], whereas the second had the promising title "On the influence of signs in mathematical reasoning" [1827]. In both

papers-read to the Cambridge Philosophical Society in 1820-1821- he showed a deep concern with the "powerful influence" of the brevity and compactness that the language of the calculus of functions exerted on analytical reasoning [1822, 63-4; 1827, 331-2].

Babbage's starting point in [1822] was equation (23.1) or

$$(29.1) \quad f^{n+m}(x) = f^n f^m(x)$$

where  $n, m$  can be regarded as fractional or negative numbers. Among the "curious results" that followed from that definition was that

$$(29.2) \quad f^0(y) = y \text{ and } (29.3) \quad f(f^{-1}(x)) = x$$

derived already in [Herschel 1813a; see (23.2)]. Babbage claimed that the number of functions possessing the property (29.3) depends on the nature of  $f$ , and he gave the example of  $f(x) = x^n$ ,  $n > 0$  [1822, 62-5]. At page 66 he stressed the importance of the latter observation as:

several errors have arisen from not attending to it, and because that particular form of  $f^{-1}$  which gives  $f^{-1}fx = x$  possesses peculiar properties:  $f^{-1}(x)$  is then the inverse function of  $fx$ ; and if we have the equation  $f(x) = y$  we may indicate its resolution thus  $x = f^{-1}(y)$ .

Here Babbage is slightly more precise than in his [1816, 191] where he was vague as to the number of the forms of  $f^{-1}$  which satisfy both (29.3) and  $f^{-1}fx = x$  [see (26.7)]. On similar lines he went on to establish the connection between positive and negative indices in functions with two variables. Based upon the definition of simultaneous functions (26.39), he provided as the equivalent of (29.1)

$$(29.4) \quad \overline{\psi^{n,n}}(\overline{\psi^{m,m}}(x,y), \overline{\psi^{m,m}}(x,y)) = \overline{\psi^{n+m,n+m}}(x,y)$$

for functions with two variables. To define  $\overline{\psi^{0,0}}(x,y)$  he put  $n=0$  in (29.4) and replacing  $\overline{\psi^{m,m}}(x,y)$  by  $u$  he derived

$$(29.5) \quad \overline{\psi^{0,0}}(u,u) = u.$$

Also, by putting  $m = 0$  and  $n = 1$  in (29.4) he obtained

$$(29.6) \quad \overline{\psi^{1,1}}(\overline{\psi^{0,0}}(x,y), \overline{\psi^{0,0}}(x,y)) = \overline{\psi^{1,1}}(x,y).$$

Equations (29.5) and (29.6) define  $\overline{\psi^{0,0}}(x,y)$ . It is evident that this function "may have different values like all other inverse functions" [1822, 66] (\*).

He then proceeded to define  $\overline{\psi^{1,-1}}$ . This time he was based on the definition of  $\overline{\psi^{n,m}}(x,y)$  - given by (26.30) - according to

which we have the properties

$$(29.7) \quad \psi^{n+1}(\psi^{m+1}(x,y), y) = \psi^{n+m+1}(x,y)$$

and

$$(29.8) \quad \psi^{n+1}(x, \psi^{1+m}(x,y)) = \psi^{n+m+1}(x,y).$$

By suitable substitutions he was led to

$$(29.9) \quad \psi^{1+1}(x, \psi^{1-1}(x,y)) = \psi^{1+0}(x,y) = y;$$

hence " $\psi^{1-1}(x,y)$  expresses such a function of  $x$  and  $y$  that when substituted in  $\psi(x,y)$  for  $y$  reduces it to  $y$ " or, if  $\psi(x,y) = u$ , we have

$$(29.10) \quad y = \psi^{1-1}(x,u) \text{ and } x = \psi^{-1+1}(u,y)$$

[1822, 67]. As we have seen in 2.6, Babbage had introduced notation  $\psi^{-1+1}$  in his [1815] -(26.19)- without any clarification<sup>(6)</sup>.

The rest of [1822] concerns problems relevant to the condensed notation  $\psi^{n+m}(x,y)$  such as "How many times is the symbol  $\psi$  repeated in  $\overline{\psi^{n+n}}(x,y)$  ?". These problems were formulated in a finite difference equation as follows: Let  $u_n$  be the answer to the above question. Then, by simple considerations, we have the equation

$$(29.11) \quad u_{n+1} = u_n + 2^n$$

which has as integral

$$(29.12) \quad u_n = 2^n + c.$$

Putting  $n = 1$  in (29.12) we have  $1 = 2 + c \longrightarrow c = -1$ . Therefore, by (29.12) we have that  $\psi$  occurs in  $\overline{\psi^{n+n}}(x,y)$ ,  $2^n - 1$  times [1822, 68-9].

In his [1827, 331-2] he chose the equation

$$(29.13) \quad \overline{\psi^{9+9}}(x,y) = \psi(x,y)$$

so as to illustrate "the power of a well contrived notation to condense into small space, a meaning which would in ordinary language require several lines or even pages". By the method sketched above it can be easily computed that  $\psi$  occurs in (29.13) 512 times<sup>(7)</sup>.

Despite its length of 53 pages and its promising title, Babbage's paper on the influence of signs [1827] is not very profound. His epistemological views are illustrated by rather elementary mathematical problems, and almost one third of the paper is devoted to trivial arguments on the advantages of symmetrical notation. The paper opened with the remark that the differential calculus was in a state of confusion due to its complicated symbolism and its variety of methods. A remedy for this would be the revision of the language of analysis and the estab-

lishment of general principles [1827, 326].

A nominalist, like Gergonne, Babbage pointed out the crucial role of definition in mathematics: "In Geometry definition is the beginning of any enquiry; in metaphysical science, it is frequently the result of one" [1827, 326-327] <sup>(a)</sup>. An admirer also of Lagrange, Babbage argued for the superiority of algebra over geometry. In [1827, 332] he wrote straight after the example (29.13):

The power which we possess by the aid of symbols of compressing into small compass the several steps of a chain of reasoning, whilst it contributes greatly to abridge the time which our enquiries would otherwise occupy, in difficult cases influences the accuracy of our conclusions: for from the distance which is sometimes interposed between the beginning and the end of a chain of reasoning, although the separate parts are sufficiently clear, the whole is often obscure. This observation furnishes another ground for the preference of algebraical over geometrical reasoning, and is one which had not escaped the notice of Lagrange.

Another argument on the advantages of algebraic over ordinary language concerned the convenience offered by the former in such an arrangement of its signs that "that quality on which the whole force of our reasoning turns shall be visible to the eye". For example, by the sign

$$(29.14) \quad \psi\left(x, \frac{1}{x}\right)$$

we denote any symmetrical combination of  $x$  and  $1/x$ . Whereas, though the same function  $\psi$  can be a function of

$$(29.15) \quad \frac{x}{x^2 + 1},$$

a form deducible from (29.14), this form does not convey at once the symmetry conveyed by the original expression (29.14) [1827, 328-9].

The merits of algebraic notation were among the epistemological issues of French semiotic philosophers. Babbage quoted with approval from Degerando [1800] -[1.8, (18)]- and wrote after him in [1827, 332]:

The closer the succession between two ideas which the mind compares, provided those ideas are clearly perceived, the more accurate will be the judgement that results; and the rapidity of forming this judgement, which is a matter of great importance, inasmuch as the quantity of knowledge we can acquire in a great measure depends on it, will be proportionally encreased.

Finally, another advantage of the language of algebra was that of preserving the quantities on which we are reasoning undetermined until the conclusion. This principle of the various degrees of indeterminateness, illustrated by a problem of probability, was attributed to Lagrange, while Carnot's own formulation of it was fully quoted in a footnote [1827, 339-345; see also 1.8 ].

The next 13 pages covered a discussion of the three stages distinguished by Babbage for the solution of a problem. These were:

1. The translation of the data into the language of analysis by the right choice of symbols,
2. The actual solution as based on mathematical theories where the difficulties are principally of a combinatorial nature, and
3. The final step of the interpretation of the results [1827, 345-357]. Of some interest is Babbage's emphasis at the difficulties presented in the last step of interpretation. Putting forward the equation of the decomposition of forces (27.26), he claimed that one has first to deduce the most general form of the solution and then work out the particular value required according to the initial conditions of the problem. He went on to criticize Laplace's procedure in the Mécanique Céleste by which the solution obtained was restricted in its form, paying, nevertheless, his respects to Laplace's mathematical contributions [1827, 357-8] (9).

Babbage's final concern was for symmetry [1827, 358-377]. In fact, his discussion on the various species of symmetrical notation is of a more or less superficial level and rather unsatisfactory. He argued that there is no model for imitation among the systems of symbolical reasoning used even by the most eminent mathematicians. Referring to Lagrange's work on mechanics, he praised him for the choice of his symbols which "so much facilitates the process of reading and comprehending analytical

formulae". At the same time he mildly criticized Lagrange for not paying the proper attention to the principles of the language in which he "clothed" his own "theory of the differential calculus" [1827, 361-2]. This is an interesting instance in the final part of the essay but unfortunately Babbage is rather vague and too general in his critical review of Lagrange's symbolism.

Lastly he referred to the language of hieroglyphics used in astronomy for the representation of the planets as "the next stage in the progress of the art, to the mere picture of the event recorded; and the signs it employs, in most cases, closely resemble the things they express" [1827, 374]. He then went on to say that in algebra, "although the principle has not been pushed to its extreme limits, the grounds of its observance are the same; the associations, are by its assistance, more easily and more permanently formed, and the memory most effectually assisted".

Cuvier's book Le Règne Animal was cited in a footnote as an example of a non-mathematical work in which the proper choice of signs assists the memory [1827, 374, fn]. At another instance he argued that the "advantage of selecting in our signs, those which have some resemblance to, or which from some circumstance are associated in the mind with the thing signified" has scarcely been stated with "sufficient force" [1827, 370]. All these references are strongly reminiscent of Condillac's philosophy and its impact in the mathematization and classification of the sciences. However, it is difficult to say how far Babbage's views were borrowed from semiotic philosophers or simply coincided with those advocated by certain of them, as, for example, by Dégérando.

According to Pycior [see (8) above] Babbage's philosophical essays started getting into shape after a stimulating encounter with D. Stewart in 1819. This claim may indeed apply to certain of those essays that remained unpublished. Our own conclusion from our earlier study of his mathematical work, his papers [1817], [1822] and [1827], as well as the few hints included by him in his [1864], is that the latter essays were written as an outcome of his early concerns in the process of discovery and of his speculations on the advantages of the language of functions during the period 1813-1820.

Summing up Babbage's calculus of functions and its language, we would like to stress the following:

- a) The constant motivation provided to him by Herschel, particularly the latter's assistance in matters of notation, abstraction (e.g. by the insistence in the use of 'the method of separation of symbols'), analogy (as between functional and algebraic equations) and generality (as in the case of general solutions of functional equations),
- b) His early linguistic influences, and his plan for a universal language which coincides with the respective one of certain French mathematicians and philosophers as well as of later English mathematicians such as Boole,
- c) His method of reduction of complicated problems to simple ones, his theory of functional equations as based on the assumption of particular solutions, as well as his extensive use of the transform  $\phi^{-1}f\phi$ ,
- d) His insistence on symmetry and compactness of algebraic notation after Lagrange and in accordance with semiotic philosophers. It should be stressed also that Babbage did not follow Arbogast's method of separation of symbols [see 2.7].

Despite their close collaboration and mutual influence, Babbage and Herschel worked on distinctly different lines. The latter's work on D- operator calculus was to be further extended and applied by mid-19<sup>th</sup>-century analysts. Babbage's contributions in the domain of symbolical methods were to be of considerable impact mainly in the shaping of De Morgan's work [3.5-3.8]. Most probably the best epilogue to this chapter is the following quotation from De Morgan's lengthy treatise on the calculus of functions [1836], which in fact is the only British work where Herschel's and Babbage's mathematical works blend together. De Morgan wrote in his introductory article:

those who have invented the little we know on functional equations, little only as compared with the view it gives of the possibility of more, have followed the subject where it led, seizing every relation which presented itself, without inquiring whether the conditions of solution were possible or not. And they were right in so doing, the general interest of the Science being considered; nor do we know whether any benefit would have accrued from deferring to follow out a view which struck them until any requisite preliminary for complete solution was obtained<sup>(10)</sup>.



## Chapter 3

Cambridge mathematics: 1820-1837; Murphy and De Morgan on the calculi of operations and functions.

### 3.1 Introduction.

In chapter 2 we studied in length Babbage's and Herschel's enquiries in the realms of Lagrangian algebras during the 1810's. Together with Peacock these analysts endeavoured to introduce Continental analysis at Cambridge University [2.1 text and (2)]. Lagrangian algebras and operator methods were in fact to flourish as areas of investigation in the domain of differential equations from the late 1830's up to the 1850's [Koppelman 1971; chapters 4-5]. Our concern in this chapter is with the intermediate period 1820-1837. The following questions are considered:

1. How far were the projects put forward by the Analytical Society fulfilled?
2. What was the degree of impact of Babbage's and Herschel's work on analytics in Cambridge?
3. What were the consequences of the updating of the Cambridge curriculum in mathematical research?
4. What were the main stimuli for the sudden interest in the study of differential equations by symbolical methods in the early 1840's?
5. And lastly, what formed the theoretical background for the study of the calculus of operations and its rapid development in the 1840's and 1850's?

Our present enquiries focus in and around Cambridge University during the transitional period 1820-1837, which saw first a switch from analytics to applied mathematics and experimental physics, and gradually a new switch towards algebra, functional and operator methods and theoretical physics. All the mathematicians under study are Cambridge graduates; with the exception of De Morgan who lectured in University College of London - holding none the less regular touch with the educational reforms at Cambridge - all the others played a considerable role in the instruction of mathematics and mechanics at Cambridge.

Directing first our attention to the educational reforms at

Cambridge at that time, we will provide answers to certain of our questions through consulting the most popular questions in the *Tripes* and the prevailing textbooks on mechanics. Further research will be carried out next on issues concerning the calculus of operations, as founded by Murphy in 1837, and those concerning the instruction of algebra and calculus of functions as put forward by De Morgan in the mid 1830's. More specifically the contents of this chapter are as follows.

Section 3.2 examines the most important aspects of Cambridge education in the 1820's and 1830's with special focus on Whewell's and Airy's textbooks on mechanics. Our main concern is with the development of the theory of the earth's shape and the first attempts to solve the earth-figure equation (13.32). The gradual prominence of Laplace's theory of attraction in the 1830's is recorded, and the limited impact of the reform suggested by Herschel and Babbage in the late 1810's is pointed out.

Section 3.3 covers Murphy's mathematical investigations between 1830 and 1839 with special focus on his paper on distributive operations [1837]. Most important for the development of the calculus of operations in the 1840's were Murphy's studies of inverse operations, as well as, of non-commutative ones. In this respect this section forms a most significant link between our study of French operator methods [1.6] and our following study of Gregory's, and particularly of Boole's, sound establishment of the calculus of operations in 1839-1844 [chapter 4].

The rest of the chapter is devoted to De Morgan, with special focus on the origins, contents and partial effects of his largely ignored treatise "On the calculus of functions" [1836], published in Encyclopedie Metropolitana. This treatise, composed under the stimulus of Babbage's and Herschel's work on functional equations, aimed to provide a unity in the study of the subject. De Morgan's foundational study of inverse functions was to have a considerable impact both on his later work on algebra and on his calculus of relations studied in chapter 6<sup>(1)</sup>.

Section 3.4 covers De Morgan's views on algebra and his wider epistemological concerns during the period 1828-1835 with special focus on his review of Peacock's Algebra [1830] as in his [1835a]. Section 3.5 focuses on the scope and content of the treatise on functions [1836]; a more detailed study follows in stages. Section 3.6 deals with the most important issues of De

Morgan's study of the foundations of the calculus of functions. His own method for the solution of functional equations is illustrated, first for the case of one variable in 3.7, and then for that of two variables in 3.8. The last section touches upon the effects of his [1836] in the shaping of his later work on algebra, including also comments upon his textbook on the calculus [1842c], the first full-length British work to break from the Lagrangian trend and to introduce elements of Cauchy's limit-based calculus<sup>(2)</sup>. The chapter includes a brief commentary on De Morgan's few contributions towards the diffusion rather than the development of the calculus of operations<sup>(3)</sup>.

### 3.2 Cambridge mathematics and mechanics: 1820-1837; the earth-figure equation.

Following on the lines engraved by Woodhouse early in the century, the Analytical Society undertook to diffuse Continental analysis in Cambridge early in the 1810's. While its Memoirs [1813] remained virtually unread, the Lacroix translation in 1816 was a first major step towards the realization of its projects [2.1-2.3; 2.7]. Textbooks, in general, were the basic tools a potential reformer would use in order to diffuse his views and influence the program of education at the University. But influential power could also be gained through the role of a moderator in the so-called Tripos. Through an unconventional choice of exam problems, the moderator could establish a temporary or permanent reform<sup>(1)</sup>. A first vivid example is Peacock who introduced in 1817 questions of purely analytical character<sup>(2)</sup>. Having had his share in the Lacroix translation, he produced his own sequel of Examples [1820] on the calculus. As a result, by 1820, when both Babbage and Herschel had lost their influence in the University, fluxions and fluents were totally excluded from the curriculum [Becher 1980b, 13-14].

The introduction of the Continental notation in the late 1810's was a decisive step which facilitated the introduction of Continental mathematics. But this step did not guarantee the success of the projects put forward by the Society<sup>(3)</sup>. While Babbage and Herschel directed their studies towards other domains [2.8-2.9], interest in analytice at Cambridge faded away. Peacock, a defender of pure mathematics in general, remained ex-

exercizing his influence as a lecturer and tutor up to the late 1830's [see (2) above]. But, on one hand, Peacock did not share Babbage's and Herschel's ardent interest in analytics -showing indifference to their latest enquiries [2:8, (8)]- and on the other hand, he was "no match" for Whewell, the leading figure in Cambridge education from 1820 onwards [Becher 1980b, 14].

Though not a member of the Analytical Society, Whewell shared some of the views advocated by its members, but strongly objected to certain of them. Instead of abstract and general methods, he fostered a blend of analytical and geometrical ones, as useful for their application in physics. In other words, it was not pure, but "applied" or "mixed" mathematics that prevailed at Cambridge while he exerted his influence- with few, but noticeable exceptions<sup>(4)</sup>. The mathematician and astronomer G.B. Airy, senior wrangler in 1823, would share most of Whewell's beliefs and collaborate with him in the establishment of his so-called "liberal education"<sup>(5)</sup>. The latter program, based upon a stable curriculum; the updating of Newtonian mechanics, the orientation towards experimental physics rather, than theoretical, and the concern for elementary treatises on mechanics, evidently limited the possibilities towards original research on the lines engraved by the Analytical Society, at least up to the mid 1830's.

Concerned with the updating of the foundations of mechanics, Whewell's ultimate plan was to prepare the students for a study of Laplace's Mécanique. In the domain of physical astronomy, the intermediary between Whewell's elementary textbooks and Laplace's work was Airy's Mathematical Tracts [1826]. It should be noticed though, that Laplace's methods were absent in this book which, nevertheless, was recommended by Whewell only to the best students. The scene started changing in the 1830's, when the Laplace coefficients, initially introduced in [Whewell 1830], featured prominently in Murphy's Electricity [1833c] and in Pratt's Mechanical Philosophy [1836]<sup>(6)</sup>. The latter's exposition of the theory of the earth's shape, a branch of fluid mechanics which was at the forefront of research in Cambridge at that time, was in many ways an improvement over Airy's old-fashioned and poor presentation in his Tracts. In 1836, Airy's student A.J. Ellis gave the first solution of the earth-figure equation. In 1839 a question posed at the Tripos concerning this equation, would be a main stimulus for the switch of emphasis

from applied to pure mathematics, more precisely, towards interest in the study of D-operator methods [4.1-4.5].

The period under study in this section is the transitional period between Herschel's and Babbage's analytical investigations in the 1810's, and the revival of interest in operational and functional calculi by Murphy [3.3], De Morgan [3.4-3.9], Gregory [4.4] and Boole [4.5] in the late 1830's and early 1840's. We will concentrate on the prevailing textbooks on mechanics with special emphasis on the earth-figure equation, concluding with its solution by Ellis and O'Brien in the late 1830's. A most valuable source of information, both in respect with the degree of influence of Babbage's and Herschel's work and in connection with the rise of interest in the earth-figure equation, are

the collections of Cambridge problems posed during the period 1820-1837(?) .

We start our account with some representative questions on pure mathematics posed at the Tripos exams in the 1820's and early 1830's. Finite difference and differential equations were introduced by Peacock in 1817 and 1819 respectively, [Cambridge 1836, 104, 110-1]. From 1820 onwards, ordinary and partial differential equations featured prominently. Among the methods for solution requested were the methods of variation of constants and of total differentials. As an example we take <sup>the</sup> equation

$$(32.1) \quad \frac{d^2u}{dv^2} + u + \Pi = 0$$

prominent, as we saw in 1.4 and 2.2, in problems of physical astronomy. This equation, posed in 1821, 1828 and 1831 [Cambridge 1831a, 16, 139; 1837, 110, 112, 116] was poorly studied in Airy's Tracts [see (32.5) below]. Up to the mid 1830's, the level of difficulty of such questions did not supersede that involved in (32.1), where  $\Pi$  often stood for a simple trigonometric function in  $v$ . It is worth noting that most questions were of a purely theoretical nature [see Cambridge 1831a, 10, 78, 142, 162-4, 171-2].

Among problems concerning expansions in series, one of the most characteristic examples was the case of

$$(32.2) \quad (a^2 \pm 2ab\cos\theta + b^2)^n, \quad n = -2 \text{ or } -1/2$$

required to be expanded in a series of  $\cos\theta$  in 1818 [Cambridge 1836, 109] and in 1830 [Cambridge 1831a, 190]. Notice that as with

(32.1), problem (32.2) was involved in the theory of physical astronomy [see formula (13.3) which gives the potential function  $V$  in respect with (32.2)]. From the late 1820's onwards, we trace many problems requiring expansions in finite differences, all of them included in Lacroix [1816] and Herschel's Examples [1820]. It is worth while recording few of them.

In 1821 we notice a question of a rather elementary level concerning the proof of a formula closely allied to Ivory's (22.7) [Cambridge 1831a, 2]. In 1829 the proof of Brinkley's expansion formula (22.15) was demanded [Cambridge 1831a, 171]. The proof of Lagrange's theorem was required in 1830 and 1831 [ibid, 196; 1831b, 55-6], as well as the expansion of  $(e^x - 1)^{-1}$  in 1830, and expansions concerning the Bernoulli numbers in 1834 [Cambridge 1831a, 199; 1837, 142-5].

In general, the prominence of Herschel's work becomes gradually evident, particularly in the early 1830's. Among finite difference equations as such we would like to mention

$$(32.3) \quad u_x u_{x+n} = a(u_x + u_{x+n}) \dots$$

studied by Herschel in 1813 [2.3, (14), (i)]. This problem was posed in 1828 [Cambridge 1831a, 162]. But Herschel's triumph is connected with functional equations. As he had foreseen in 1816, the functional equations that prevailed in the exam papers were solvable by Laplace's method of reduction to finite difference ones [see the passage<sup>(e)</sup> on the Lacroix translation in 2.7]. As a representative case of this group of equations is

$$(32.4) \quad f(x^2) - f(x) = m$$

posed in 1830 [Cambridge 1831a, 191; 1837, 323]. In the solution of (32.4) given in [Cambridge 1830, 45-6] we read the following comment on Herschel's var<sup>y</sup>ely cited paper [1822; 2.8]: "For a more extensive application of this method, see a very ingenious paper by Mr. Herschel in Vol 1 of the Cambridge Philosophical Transactions" [ibid, 46]. A propos, Babbage's own method, as in his [1820, 12], is <sup>found</sup> only once in [Cambridge 1830, 37-40].

In other areas of the integral calculus, we trace problems concerning the calculus of variations and definite integrals from 1828 onwards [1831a, 142, 161, 167, 203]. The latter gained prominence in an advanced level in the 1840's [Cambridge 1849]. The appearance of all these problems mentioned so far were due primarily to Peacock's initial reform in the late 1810's, and also to Whewell's criterion of potential application of most of

these problems within questions of geometrical nature or in mechanics. For example, many problems regarding the solution of functional equations by Laplace's method were stated verbally in connection with the theory of curves [Cambridge 1831b, 53, 55]. But Whewell's most direct influence was that, contrary to Peacock's move towards pure mathematics, he enriched the exam papers with a variety of questions in applied mathematics. We have traced many questions required to be answered both "geometrically and analytically"; see for example the demonstration of the attraction formula (13.1) in 1828 [1831a, 139, 144]. Moreover, certain problems required only geometrical proofs [1831a, 36, 108]. This tendency gradually diminished in the 1830's. However, during this decade a classical problem posed was drawn from Airy's presentation of the earth-figure equation in his Tracts [1826].

But before we refer to this kind of exam problems, let us introduce Whewell's and Airy's work within mathematics and mechanics. As a mathematician, Whewell was not a major figure<sup>(9)</sup>. In a period when the French sought for exact solutions and were replacing Lagrangian calculus by Cauchy's limit-based calculus, Whewell favoured approximate techniques including divergent series. However, despite an apparent influence from the "analytical revolution", he approved neither of Lagrange's mathematization of mechanics, nor of his algebraic foundations of the calculus. Progressive and conservative at the same time, when in the late 1830's he brought up the concept of limit, it was Newton's version which he implied and not Cauchy's [Becher 1980b, 14-16, 27-30; Grattan-Guinness 1985a, 95, 99-101].

In the realms of mechanics, Whewell gave priority to traditional methods looking forward to simplified versions of Laplace's approach. In his Treatise on Dynamics [1823] he wrote that those who will simplify Laplace's Mécanique "will deserve to be considered as real benefactors to the commonwealth of science". However, he discouraged original research saying that if a mathematician did not pay attention to the work of his predecessors<sup>(10)</sup> "he might easily, in following a favourite path of research, spend too much of his time in inventing, solving and generalizing particular problems" [1823, v]. A concise display of his views on mathematics and mechanics is in his "Preface" to Mechanical Euclid [1837], a book devoted to practical illustra-

tions of mixed mathematics within mechanics and hydrostatics. The name of Euclid in the title was a synonym of "a coherent system of exact reasoning" [1837, i] <sup>(11)</sup>.

Despite the lack of any major contributions in the domain of mathematics and mechanics, Whewell's views on the nature of science, and on the role of history were considerably influential <sup>(12)</sup>. Focusing on physical astronomy we switch to the work of his disciple Airy. Though he had a greater potential as a mathematician than Whewell, Airy orientated his studies towards experimental physics and applied mathematics. Even when in 1866 he composed a treatise on differential equations, Airy emphatically showed his concern with applications rather, than with abstract theory [1866, 38; chapter 9]. His originality as an analyst was projected in his [1827]. In this paper he amended the defects of Laplace's demonstration of (13.8) offering a lucid deduction of (13.8) and its consequences (13.10)-(13.11) which played a most crucial role in Laplace's theory of the earth's shape [1827, 382, 385-300]. He further defended Laplace's potential theory against Ivory's criticism [1827, 379-380; Grattan-Guinness 1985a, 102].

But, restricted by Whewell's limiting programme Airy did not carry on his research on Laplace's lines [Becher 1980b, 33]. Not only his Tracts [1826], but also the article he contributed for the Encyclopedia Metropolitana <sup>(13)</sup> on the "Figure of the earth" [1845] were totally devoid of Laplace's methods. Aware of this striking omission he offered an apology for excluding Laplace's own exposition and methods on the ground that, having included them, his treatise would be rendered "unintelligible" [1845, 192]. The material of this article is almost identical to that included in his Tracts [1826]. Under slight modification and inclusion of new material, Airy's textbook was successfully reedited in 1831, 1839 and 1858. But despite his general approval of it, Whewell was opposed to the updating of the Tracts as it disturbed the stability of the curriculum <sup>(14)</sup>.

The Tracts opened with a brief study of equation (32.1), or

$$(32.5) \quad \frac{d^2u}{d\theta^2} + n^2u + \theta = 0,$$

" $\theta$  being a function of  $\theta$  and constants only". Multiplication by  $\cos n\theta$  and integration by parts gave the general solution of



(32.5) in  $\theta$  and  $\cos n\theta$  [1826, 1-2]. Specific cases of  $\theta$ , such as  $\theta = -a$  or  $\theta = b\cos(m\theta + D)$ , where  $a, b, m, D$  constants, were studied; among them the case of  $\theta = 0$  which gives readily

$$(32.6) \quad u = a\cos n\theta + b\sin n\theta,$$

where  $a$  and  $b$  are constants [1826, 2-3]. Contrary to Woodhouse, Airy avoided to illustrate the method of variations of constants confining himself only to a pertinent remark in [1826, 5].

There followed a study of the problem of two bodies which required the solution of (32.5) for  $\theta$  a complicated function of both  $u$  and  $\theta$ . Remarking that no direct method of solution is known for the general case, he stressed that (32.5) can always be solved if  $\theta$  is a function of  $\theta$  only. "This suggests the method of solving by successive substitution. Find a value of  $u$  in terms of  $\theta$ , which is nearly the true one... and a more approximate value of  $u$  found..." [1826, 27]. Thus, like Whewell, Airy favoured approximate techniques and did not bother with general, exact solutions. His exposition, particularly of the three-bodies problems, is inferior to that by Woodhouse [1818], a fact which Airy admitted in the "Preface" [1826, iv]<sup>(15)</sup>.

Airy's treatment of the theory of the figure of the earth resembled that put forward by Clairaut in his [1743]. Considering the earth as an heterogeneous fluid body, Airy deduced by the help of geometrical diagrams and the theory of conic sections Clairaut's condition (13.12) for the fluid mass of the earth to be in equilibrium<sup>(16)</sup>. If  $e$  corresponds to the ellipticity of the earth,  $c$  to the radius of the spheroid<sup>(17)</sup> and  $\rho$  to its density, then differentiation of (13.12) relative to  $c$  reduces condition (13.12) to the form:

$$(32.7) \quad \frac{d^2 e}{dc^2} + \frac{2pc^2}{\int_c \rho c^2 dc} \frac{de}{dc} + \left[ \frac{2pc}{\int_c \rho c^2} - \frac{6}{c^2} \right] e = 0$$

[1826, 99-100]. By change of the variables (32.7) corresponds to Clairaut's (13.13) and to Laplace's (13.21). Airy remarked that if  $\rho$  is given in terms of  $c$  then it is always possible to integrate (32.7) by series. However, no attempt is traced in his work to deduce the solution of (32.7).

Below he undertook to "find the ellipticity of the Earth on any assumed law of density of the strata". Assuming (13.12) and differentiating it twice in respect with  $c$ , he obtained (32.7). "Now, when  $e$  is given in terms of  $c$ , we must substitute it in

this last equation [(32.7)], and by integration find  $e$ . The expression will contain two arbitrary constants" found by substitution in (13.12) and its reduced form after the first differentiation. Thus (32.7) can assume a simpler form if we let

$$(32.8) \quad \int_0^r \rho c^2 = p, \quad p e = u;$$

we then have

$$(32.9) \quad \frac{d^2 u}{dc^2} - \frac{6u}{c^2} - u \frac{c}{p} \cdot \frac{dp}{dc} = 0$$

[1826, 107].

As an "Example" he let

$$(32.10) \quad \rho = A \cdot \frac{\sin qc}{c},$$

$A$  and  $q$  constants, commenting as follows: "As this gives a density diminishing from the center to the surface, it is probable that it will pretty nearly represent that of the earth". On substituting in (32.9) he was led to

$$(32.11) \quad \frac{d^2 u}{dc^2} - \frac{6u}{c^2} + q^2 u = 0$$

"the complete solution of which is":

$$(32.12) \quad u = C \left( \sin(qc + C') + \frac{3}{qc} \cos(qc + C') - \frac{3}{q^2 c^2} \sin(qc + C') \right)$$

$C$ ,  $C'$  being arbitrary constants [1826, 107-8]. <sup>There</sup> followed the determination of  $C'$  and  $C/A$  by means of equation (13.12) and then calculations of  $e$  under further assumption on  $q$  [ibid, 108-110].

Equation (32.11) was to be from 1826 onwards the standard form of the earth-figure equation within both physical and mathematical context. In deducing (32.11) from (32.9) Airy most probably drew on Laplace's choice of (32.10), or (13.27), as the law of densities<sup>(18)</sup>. However, he neither bothered to explain how one comes to formulate (32.10) -as that would involve Laplace's potential theory- nor how one concludes (32.12) from (32.11). These two omissions, as we shall see, puzzled students who attended Airy's classes even when ten years later both defects were to be amended in other works. For, in both the updated editions of Tracts and in his article [1845], Airy's treatment of the

theory of the earth's shape remained virtually unchanged. On the fragment discussed so far see also his [1845, 187-9].

What was demanded in the Triplos in connection with the theory of the earth's shape? The representative question posed in the 1830's required i) the reduction of (32.11) given the equation (13.12) and the law of densities (32.10), and ii) the determination of the ellipticity  $e$  from (32.11) given the latter's solution (32.12) [Cambridge 1837, 319-320].

The first question to isolate (32.11) from its physical context and to demand the solution of (32.11)-given the solution of the general form of the earth-figure equation (13.33)-was posed by T. Gaskin in 1839. Gaskin was the second moderator after Peacock to diverge considerably from the standard curriculum and the first to attract attention to symbolical methods. Postponing the discussion of Gaskin's revolutionary step for 4.2, let us record the first attempt towards a direct solution of (32.11) made by A.J. Ellis in 1836.

While Gaskin's name is traced in few 19<sup>th</sup>-century works on differential equations [4.2], Ellis remained totally unknown since his solution was written by him only on the first page of his copy of Airy's Tracts [1831] which bears the date "18 April 1836". A wrangler in 1837, Ellis was still a student when Gaskin - wrangler in 1831- was for the first time an examiner in 1835. A comparison of their solutions <sup>probably</sup> suggests that Gaskin was <sup>probably</sup> aware of Ellis's own solution. In any case, both analysts made use of the method of variation of parameters.

Ellis let

$$(32.13) \quad qc = x$$

reducing thus (32.11) to

$$(32.14) \quad \frac{d^2u}{dx^2} - \frac{6u}{x^2} + u = 0.$$

Assuming first  $u$  to be the solution of (32.14) devoid of the middle term, he obtained according to (32.6) for  $n = 1$

$$(32.15) \quad u = a \cos x + b \sin x.$$

Regarding  $a$  and  $b$  now as arbitrary functions of  $x$ , he differentiated (32.15) and by substituting in (32.14) he arrived at

$$(32.16) \quad A \sin x + B \cos x = 0$$

where  $A$  stood for

$$(32.17) \quad \frac{d^2b}{dx^2} - 2 \frac{da}{dx} - \frac{6b}{x^2}$$

and B for the same form where a and b are inter-  
changed. Condition (32.16) led additionally to the relations

$$(32.18) \quad A = 0 \quad \text{and} \quad B = 0$$

by means of which, and (32.17), a and b were to be determined.

In analogy with the middle term of (32.14), Ellis assumed a and b to be polynomials of the form

$$(32.19) \quad a = a_1 + \frac{a_2}{x} + \frac{a_3}{x^2}, \quad b = b_1 + \frac{b_2}{x} + \frac{b_3}{x^2}$$

Combining (32.17), (32.18) and (32.19) he determined a to be

$$(32.20) \quad a = a_1 + \frac{3b_1}{x} - \frac{3a_1}{x^2},$$

obtaining a symmetrical form for b. Substituting the values of a and b in (32.15), he arrived, after a change of the constants

$$(32.21) \quad a_1 = c \operatorname{sinc}', \quad b_1 = c \operatorname{cosec}',$$

at the known solution (32.12).

Ellis's proof, as we saw, is a straight-forward application of the method of variation of constants, which was introduced and illustrated by Woodhouse two decades earlier. Equation (32.11) can also be directly integrated by series [4.3]. However, none of these simple methods ever featured in textbooks of physical astronomy in connection with the equation (32.11) in England. We would like to mention though, the only method traced so far in textbooks up to the 1870's introduced by O'Brien in 1840. A third wrangler in 1838, O'Brien was most probably like Ellis a non-exception in feeling dissatisfied with Airy's exposition of the theory of the earth's shape. Like Pratt, mentioned below, O'Brien developed this theory on Laplace's lines in a simplified manner in his Mathematical Tracts [1840] <sup>(19)</sup>.

Having deduced (32.11) in the form

$$(32.22) \quad \frac{d^2\eta}{da^2} + (q^2 - \frac{6}{a^2})\eta = 0,$$

O'Brien assumed, without offering any explanation, the transform

$$(32.23) \quad \eta = \frac{1}{a^2} \int_0^a a' \int_0^a a' J' da'^2$$

where  $J'$  a function of  $a'$ . The effect of this transform was to reduce (32.22) to the form

$$(32.24) \quad \frac{d^2 J}{da^2} + q^2 J = 0,$$

which is easily solved, and hence  $n$  can immediately be deduced by (32.23) in the usual form (32.12) [1840, 53-4].

Like all the other methods to be discussed in the next two chapters, both Ellis's and O'Brien's solutions depend upon the form (32.24) [see also (13.25), (14.10), (22.2)]. However, the main defect of these two methods lies in that none of them can be applied to the more general form of the earth-figure equation (13.33) or (14.8). In fact, none of these methods was applied by English and Irish analysts. The difference of approach between physicists and mathematicians in regard with both the earth-figure and the Laplace equations will be discussed in chapter 9.

Returning now to Ellis's annotated copy of Tracts [1831], we see in his own handwriting a reference at page 170 to [Pratt 1836, 256-7] on the choice of (32.10) as the law of densities. J.H.Pratt, a third wrangler in 1833, was the first to present in a lucid and concise manner the theory of attractions and of the earth's shape on Laplace's lines. His treatment, first exposed in his Mechanical philosophy [1836] -cited in [1.3,(4),(7),(8)]- was edited in an expanded and modified form under the title A treatise on attractions, Laplace's functions and the figure of the earth [1860]. This book was successfully edited for a fourth time in 1871.

Focusing here on the first version [1836], we will draw on its "Preface" where Pratt explains the reasons why he composed his treatise, comparing his approach with those that prevailed in his time. He referred to Woodhouse, Poisson, Whewell and Airy as the authors of the most important books on mechanics used in Cambridge University but, acknowledging the latters' contributions, he expressed his objection towards their ambiguous, "neither strictly geometrical, nor strictly analytical" expositions. Claiming that in no way geometry should be banished from the curriculum he nevertheless added [1836, iii-v]

that we should pay more regard to system than we hitherto have done; if our course is to be geometrical let us adhere to geometry, if analytical to analysis; if we are to admit both (the

preferable course) let us keep our systems well apart; and not have our course of reading confused, here analysis and there geometry.

Pratt went on to explain why he devoted part of his treatise to Laplace's attraction theory [1836, vii]:

After calculating the attraction of spherical and spheroid bodies of homogeneous mass I have proceeded to the more general investigation of the attraction of a body differing but little from a sphere in form, with a view to the calculation of the Figure of the Earth in a future part of the work. This has led me to introduce Laplace's Coefficients, a subject unknown in our University course till introduced a few years since by Mr Murphy in his Treatise on Electricity. I have followed Laplace's course, and not the inverse method of Mr Murphy. The frequent occurrence of the equation

$$[(32.25)] \quad \frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = 0$$

in physical investigations makes it highly desirable, that a knowledge of the profound analysis of Laplace should be made as familiar as possible to the higher class of students in the University. For this reason I have introduced, in as concise and at the same time as clear a manner as I was able, the principal properties of the Coefficients of that great analyst, breaking up and arranging the subject in the form of propositions.

It was Whewell who first introduced elements from Laplace's potential theory, as modified by Poisson. However, he presented his account not in a Cambridge book but in the Encyclopedia Metropolitana article on "The theory of electricity" [1830] under the title "On the mathematical theory of electricity compared with experiment". Providing the expansion (13.7), he called the coefficients  $Q^{(1)}$  after Laplace. Ivory's equation (22.6) was also introduced under his name [1830, 144, 146; 1.3, (5); 2.2]. Whewell showed interest in the "Laplace coefficients"  $Q^1$  as connected within "very curious analytical investigations" but, as expected, he orientated his account towards the experimental [1830, 143-152; Grattan-Guinness 1985a, 106].

But the first strictly mathematical treatment of the Laplace coefficients was given by Murphy in his Electricity [1833c] in the most complete and abstract manner. R. Murphy, a third

wrangler in 1829, contributed many mathematical papers during the period 1830-1837 stimulated principally by problems of physics [see 3.3]. Despite his strictly theoretical account, characterized above all by a tendency towards generalization -exceptional, perhaps unique, at his time- Whewell approved of his work since his results could be confirmed experimentally [see (20) below; Grattan-Guinness 1985a, 106].

Murphy's presentation opened with the definition of a polynomial of order  $n$  in one variable so that "if multiplied with any polynomial of an order less than  $n$ , the integral of the product taken between 0 and 1 will be zero" [1833c,3-4], preparing the ground for a rigorous proof of the orthogonality property of Laplace's coefficients [1.3,(8)]. Further specifying this general polynomial, he came to introduce the "Legendre Coefficients" -called so later on- and the related ordinary differential equation mentioned above in [1.3,(5);1833c,14]. Following thus a reverse order from that of classical expositions, he dealt first with the Laplace equation in one variable generalizing next to the case of two variables. Introducing the Laplace coefficients by means of (13.7), he proved that they satisfy equation (13.6) and went on to determine the most general trigonometrical function that satisfies (13.6) and hence can be expanded in terms of Laplace's coefficients. The preliminary account ended with the orthogonality property [1833c, 16-24; 1.3, (7)].

Murphy's strictly analytical study of the Laplace coefficients foreshadows their development as a branch of analysis late in the century [see Whittaker 1902]. Being ahead of his time, Murphy's work revived in Todhunter [1875], a treatise which improved over Pratt [1860]. The solution of the Laplace equation, either in 3 variables (32.25) or (13.2), or in 2 variables (13.6), in finite form presented with insurmountable difficulties. Both Pratt [1836,161] and O'Brien [1840,7,14] declared that this equation could only be approximately solved. Hargreave was the first to provide a finite solution in 1841 and by 1846 onwards English and Irish analysts tried hard to improve over Hargreave's procedure by applying symbolical methods (5.8).

This section was devoted principally to a study of the period 1820-1837 through textbooks on physical astronomy and exam papers, emphasis paid on Herschel's prominence as a textbook writer. We would like to add that though Lacroix [1816] and its

sequel of Examples [1820] were used at Cambridge at that time ; still, contrary to their initial design to serve as a background on the calculus for the average student, they were recommended by Whewell, together with Airy's Tracts, only to the best students [Becher 1980b,32,34]. We should note though, that there were few new textbook writers on the calculus, such as J.P.Higman -third wrangler in 1816 and a member of the Analytical Society- and H.Coddington - senior wrangler in 1820 who contributed various books on optics. The former included elements from the theory of limits in his [1825], while the latter followed Arbogast's *method* of expansion of multinomials in series by his calculus of derivations . Other textbooks of an elementary level on the calculus and on analytical geometry <sup>are mentioned in</sup> [Becher 1980b,19-22]. Finally, as far as algebra is concerned, Wood's Algebra [see (11) above] was in most accordance with Whewell's program. On Peacock's and De Morgan's work on algebra see [3.4-3.9].

In connection with physical astronomy and electricity the 1830's saw a considerable development. Laplace's and Poisson's potential theory gained gradually priority over Newtonian mechanics. Particularly the theory of the earth's shape was one of the most important concerns within Cambridge physicists; it formed the model, conceptually and mathematically, for the theories of electricity, magnetism and heat within fluid mechanics. Whewell and Airy focused on the experimental aspect of this theory carrying out various experiments in order to measure the density of the earth. Apparently, students were not satisfied with Airy's presentation of this theory, among them A.J. Ellis, Pratt, O' Brien, Gaskin and W. Thomson<sup>(20)</sup>. This insufficiency asked for modifications in textbooks and exam questions and part of the theory of the earth was soon to be studied within strict mathematical context [chapter 4].

This transitional period saw new capable wranglers who influenced the educational program at Cambridge and who contributed in the development and diffusion of various branches of pure mathematics and theoretical physics. We conclude our account by providing a list of the most important ones in connection with this thesis. In the 1820's we notice De Morgan and R.Murphy, wranglers in 1827 and 1829 respectively, as well as the textbook writer J.Hymers, second wrangler in 1826 who marked an era in the history of the college [Becher 1980b, 20-21; 4.2]. In



the 1830's we have an abundance of young mathematicians and physicists, the date of graduation inserted in a parenthesis: T.Gaskin (1831), S.Earnshaw (1831), J.H.Pratt (1833), P.Kelland (1834), S.S.Greatheed (1835), A.J.Ellis (1837), D.F.Gregory (1837), J.J.Sylvester (1837), M.O'Brien (1838). Finally, in the early 1840's we have R.L.Ellis (1840), A.Cayley (1842), W.Thomson (1845), and the textbook writer J.Pearson in 1848 [Becher 1980b, 23; chapters 4-5].

While research on the lines of Babbage and Herschel<sup>(21)</sup> was restricted to the minimum in the 1820's, and while Newtonian mechanics gained prominence over Laplace's Mécanique, gradually the emphasis on applied mathematics switched to pure mathematics and this was largely due to the updating of the curriculum and the initiative of the examiners. The results of this rather temporary reform - which saw its climax in the early 1840's - will be examined in detail in what follows in this thesis through a study of symbolical methods during 1837-1860.

### 3.3 Murphy on distributive operations: 1833-1837.

In 3.2 we mentioned R.Murphy in connection with the Legendre and the Laplace coefficients as they featured in his Electricity [1833c]. That chapter, opening with a definite integral property, was based upon his earlier enquiries in the general properties of definite integrals whose role he had perceived to be of considerable importance in the theories of the "propagation of heat" and of the "distribution of electricity" [1830,429]. Drawing from the work of Français and Servois, he paid particular attention to the property of distributivity, which characterised definite integral operations, isolating its study within pure mathematical context in his [1833b,354]. And four years later he devoted his "First memoir on the theory of analytical operations" [1837] solely to a strictly theoretical study of distributive operations in a most general and abstract manner.

In many ways Murphy's work was quite ahead of his time. However, due to illness and poverty he died after a rather unfortunate career in 1843 at the age of 37, without providing a second part either for his [1833c] or for his [1837]<sup>(1)</sup>. In the sequel to his [1837], Murphy would most probably include applications of his theory in the realms of definite integrals - a sub-

ject which had occupied almost the whole of his earlier work- and in that of differential equations- which also featured in his papers at certain instances. The main novelties in the first memoir which was published <sup>in 1837</sup> lay in his foundational study of inverse distributive operations, improving thus over the study of Servois [1814] and Sarrus [1822] [see 1.6], and in his original introduction of the "transmutation" of non-commutative, distributive operations.

Due to its implicit effect in the development of operational methods, Murphy's paper [1837] has been given recently significance and a brief outline of it has been published<sup>(2)</sup>. However, a detailed study of this lengthy paper, as well as of its background and its connection with the work of Murphy's predecessors and contemporaries, is missing. In what follows, we give a brief outline of Murphy's early mathematical papers, tracing in so doing the germs of his [1837], his early influences and concerns, his implicit connection with Babbage and Herschel, presenting finally the main results of his [1837] hinting at their evaluation and impact or lack of influence. We conclude with a few words on his later work pointing out some subtle, distant links between him and his predecessors and contemporaries.

Murphy's early papers, published in the Cambridge Philosophical Transactions between 1830 and 1835, deal with algebraic expansions in series and definite integral transforms<sup>(3)</sup>. His algebraic manipulations, characterized by a strong tendency towards abstraction and generalization, can be seen as a revival of the analytical trend put forward by the Analytical Society which had remained stagnant for over a decade. But, contrary to the initial work of Babbage and Herschel, both the origins and the applications of Murphy's enquiries were physically orientated. In this sense, his work, lying at the borderline between pure and applied mathematics, is a characteristic product of the updating of the Cambridge curriculum, of Whewell's orientation towards applied mathematics, marked at the same time by Murphy's own tendency for original research.

In his first paper "On the general properties of definite integrals" [1830], Murphy built gradually his theory for transforming definite integrals into series and for the summation of such series in integral form. His procedures are rather complicated, and apparently, of no direct application in the rest of

his work. Of interest is to note his generality, as for example in his determination of the definite integral of a totally arbitrary function  $f(x)$  between  $x=a+h\sqrt{-1}$  and  $x=a-h\sqrt{-1}$  [1830,430-1]. His second result consisted in expressing the definite integral of  $f(a.x^*)$  between  $x = 0$  and  $x = \infty$  in the form of an infinite series with general term

$$(33.1) \quad (-1)^{n+1} \cdot \frac{1}{n^n} \frac{d^{n-1}f(a)}{d(\log a)^{n-1}}$$

where  $a$  is a constant and  $f$  arbitrary [1830, 431-2].

Murphy applied his theory to the Riccati equation (14.3), or

$$(33.2) \quad \frac{du}{dt} + Au^2 = Bt^m,$$

in definite integral form<sup>(4)</sup>. His procedure is complicated and we omit its details. We would like only to mention that it was based upon a theorem which resembles closely a theorem of summation introduced by Herschel in his [1814, 449].  $S$  and  $\Phi(h)$  standing for two specific series in powers of  $x$  and  $h$  respectively, Murphy's theorem is of the form:

$$(33.3) \quad \frac{x}{2} \int_n \frac{\Phi(h)e^{\frac{x^{1/a}}{h}} + \Phi\left[\frac{1}{h}\right]e^{hx^{1/a}}}{h} = S \log(-1)$$

$h$  varying from  $-1$  to  $1$  [1830,442]<sup>(5)</sup>. By means of (33.3), the solution  $u$  of (33.2) was found in terms of  $S$  in definite integral form [1830,443]. One can compare (33.3) with Herschel's theorem

$$(33.4) \quad S \left[ \lambda_x \frac{f(th^x) \pm f(th^{-x})}{2} \right] = \frac{F(h^D) \pm F(h^{-D})}{2} (f \log(-1) : \log t),$$

where  $f(t)=G_t(\lambda_x)$ ,  $S$  the symbol of summation and  $D = d/dt$ <sup>(6)</sup>.

Murphy never referred to Herschel's work in his papers. However, it seems quite probable that he had been through it as the similarity between the two theorems is really striking. Still, the procedures are radically different. It is worth noting that Murphy showed deep concern with differential equations which played a significant role within mechanics. At the same time, it is rather surprising that his solution of the Riccati equation in finite form escaped the notice of English analysts, most particularly of J.W.L.Glaisher whose [1881] was wholly devoted to equation (33.2), its transforms and the definite integrals which

satisfy them [see 1.4, (4); chapter 4].

Murphy's next paper, "On the resolution of algebraical equations" [1833a], primarily dealt with the determination of the roots of algebraic equations. However, the applications of the initial theory concerned the direct proof of known theorems of analysis and the determination of definite integrals. Thus, the content of this paper, with its slightly misleading title, lies between algebra and analysis<sup>(7)</sup>. Murphy himself noticed that by saying [1833a, 153]:

In concluding these few observations on Algebra, we may observe, that in whatever department of Analysis we seek principles, simple in their announcement, and general in their applications; so far as we succeed, something useful is acquired for analysis, nor can any branch of Mathematics, however humble, be deemed unworthy in this way .....

There  
A followed three memoirs "On the inverse method of definite integrals" [1833b, 1835a,b], which constitute a single paper as their sections are consecutively numbered [see (3) above]. Among his concerns were applications of his theory in problems of electricity, including a study of another class of second order differential equations closely allied to the Legendre equation,

$$(33.5) \quad tt' \frac{d^2u}{dt^2} + (m+1)(1-2t) \frac{du}{dt} + n(n+1+2m)u = 0$$

where  $t'=1-t$  [1835b, 335]. Equation (33.5) was solved in definite integral form for  $m = 0$  [1835b, 338-342]<sup>(8)</sup>.

Before we proceed to a study of his [1837] let us focus on his [1833b] where distributive operations are for the first time introduced in English mathematical literature. Murphy acknowledged Fourier, Poisson, Cauchy and Gauss for their extensive study of the "direct calculus of Definite Integrals". However, it would be "advantageous", he stressed, "to possess an inverse method" as well. Thus, "objects of importance in the pure, and physical mathematics would be attained". He added [1833b, 353-4]:

Euler and Laplace have valued the interpolated differential coefficients of fractional orders, for such functions as may be simply represented by Definite Integrals of a peculiar form. If we extend this principle to all functions of operation of the distributive kind\*, (that is, such whose action on the whole, is the sum of the

actions of the parts) and if we can represent a Definite Integral of the proper form, this view will be complete.

With the asterisk<sup>k</sup> Murphy referred in a footnote to the papers of Servois and Français in the Annales where the notion of distributivity was first formulated [see 1.6]. As it becomes evident from other scattered general references, he was fairly well acquainted with the principal French journals of that time. The Annales are also mentioned in [1833a, 125]. It is thus possible that Murphy was also aware of Sarrus [1822] for reasons which will see below. Murphy went on to say [1833b, 354]:

Again, the phenomena of the physical sciences generally result from an infinite number of the elementary actions of the particles forming the system under consideration. Such an inverse calculus would conduct us from the observed phaenomenon, to the laws of the elementary actions.

This quotation reminds us of Babbage's claim that the inverse calculus of functions would "unveil the hidden laws" which govern the physical phenomena [1816, 179-180; 2.9]. Perhaps this is one of the far but rare coincidences of views in the history of science. However, it gives evidence of the subtle proximity of views between Murphy and those of Babbage and Herschel who, after leading a purely theoretical research in the calculus of functions, tried -in their manuscripts- to apply this calculus in other branches, such as the calculus of variations [see 2.7-2.9].

In the Notes appended to the end of his [1833b], Murphy dealt for the first time with the concept of a distributive operation introducing the notion of the "appendage", a notion equivalent to Sarrus's "fonction complementaire" [1822, 297; 1.6]. He first let  $\psi$  denote the operation which changes  $x$  to  $x+h$ . Accordingly  $\psi^n$  effects the change  $x+nh$  and  $\psi^{1/n}$  changes  $x$  to  $x+h/n$ . Letting further  $F(\psi)$  stand for the operative function

$$(33.6) \quad A_m \psi^m + A_n \psi^n + \dots,$$

he assumed that we are given a function  $\phi(x)$  which subjected to "the above operations, is by the inverse calculus reduced to the form  $\int_t f(t).t^x$ ; the result of the operation  $F(\psi)$  will be the sum of the partial results of the same operation on each element  $f(t).t^x.\delta(t)$ ". He further remarked that

$$(33.7) \quad \psi^n(t^x) = t^{x+nh} = (th)^n.t^x,$$

hence

$$(33.8) \quad F(\psi).t^x = F(th).t^x$$

and finally, by the assumption on  $\varphi(x)$  and (33.8), we have

$$(33.9) \quad F(\psi).\varphi(x) = \int_t f(t).F(th).t^x$$

[1833b, 406-7].

He introduced the inverse of  $F(\psi)$ ,  $F^{-1}(\psi)$ , "so that the latter operation performed with the former neutralizes it", hence

$$(33.10) \quad F^{-1}(\psi).[Cem^x] = C.F^{-1}(emh).e^{mx}.$$

He then let  $m_1, m_2, \dots$  be the roots of

$$(33.11) \quad F^{-1}(emh) = 0.$$

Then, if  $A_1, A_2, \dots$  are arbitrary constants,

$$(33.12) \quad F^{-1}(\psi) [A_1 e^{m_1 x} + \dots] = 0,$$

or

$$(33.13) \quad F(\psi)(0) = A_1 e^{m_1 x} + \dots$$

Formula (33.13) expresses the "appendage" of  $F(\psi)$ , in other words the result of the operation of  $F(\psi)$  on zero. Without providing any clarifications on his procedure, he went on to say straight after (33.13) that this "appendage must be added to the operation  $F(\psi)\varphi(x)$ , since  $\varphi(x)$  is analytically to be treated as  $\varphi(x)+0$ ", meaning that (33.13) should be added to the right-hand side of (33.9). The notes ended with the corresponding expression of (33.9) for  $\psi = \Delta$  and  $\psi = d/dx$  [1833b, 407-8] (9).

In this condensed presentation Murphy implied that both  $\psi$  and  $F$  are distributive operations. He also assumed that the inverse of such an operation is distributive too. His process from (33.10) up to (33.12) is hardly clear, and no information is provided in connection with the form of the left-hand side of (33.11) or with the solutions of this equation [see (9) above]. All these obscurities were clarified in his new, lengthy exposition of the theory of distributive operations [1837].

Murphy distinguished between the subject,  $u$ , the operation,  $\theta$ , and the result of the operation on the subject,  $y$ , denoting their connection by

$$(33.14) \quad (u)\theta = y^{(10)}.$$

As "linear" was an operation  $\theta$  which obeys the formula

$$(33.15) \quad [x+\xi]\theta = [x]\theta + [\xi]\theta.$$

By "relatively - free" he called commutative operations, whereas by "relatively-fixed" non-commutative ones. Operation  $\psi$  affects a function  $\varphi(x)$  according to

$$(33.16) \quad [\varphi(x)]\psi = \varphi(x+h).$$

An immediate consequence of this definition was

$$(33.17) \quad [u](\psi-1) = [u]\Delta.$$

Letting  $d_x$  "denote the operation of taking the finite difference, and after dividing it by  $h$ , then putting  $h = 0$ ", he was led by letting the subject  $u$  stand for 1, to the "universally equivalent" relations between the symbols of operation  $\psi$ ,  $\Delta$  and  $d_x$ :

$$(33.18) \quad \Delta = \psi - 1, \quad d_x = \frac{\Delta}{h} = \frac{\psi - 1}{h}$$

where " $h$  is put  $= 0$ " [1837, 179-181].

Thus, like Français in (16.6), Murphy, omitting the subject, worked with equivalent operators. He denoted the composition of two operations  $\psi$  and  $f$  by  $\psi f$  defining it by

$$(33.19) \quad [u] \psi f = ([u]\psi)f^{(11)},$$

a definition equivalent to the usual formulation

$$(33.20) \quad (f\psi)u = f(\psi(u)).$$

Then, on lines similar to Servois in (16.20) he proved directly from (33.15) that "polynomial operations of which the parts are linear possess themselves the same character", and that "the compound [composition] of linear operations is also linear". Further, noting that  $\psi$  is linear, and therefore by (33.18)  $\Delta$  and  $d_x$  are linear too, he stated that

$$(33.21) \quad \text{"every function of a linear operation is itself of the same class of operations"}$$

[1837, 180-2]. Apparently, by "every function" in (33.21), he implied polynomial operations with terms which are compounds of  $\psi$ ,  $\Delta$  and  $d_x$ .

Murphy's prime concern was to establish a rigorous definition of  $e^\theta$ , where  $\theta$  is a linear operator. Thus he wished not only to vindicate the symbolical expression of Taylor's theorem, as in (22.9), but also to put the foundations for his consequent study of non-commutative operations. Among his results was his proof that for  $\theta, \theta'$  relatively -free operations it holds that

$$(33.22) \quad e^\theta e^{\theta'} = e^{\theta+\theta'}.$$

His procedure is worth quoting as the intermediate results were of importance in the gradual construction of his theory<sup>(12)</sup>.

Based upon the binomial theory, he proved inductively the formula

$$(33.23) \quad (\theta + \theta')^n = \theta^n + n\theta^{n-1}\theta' + \frac{n(n-1)}{2} \theta^{n-2}\theta'^2 + \dots + \theta'^n$$

for  $n$  positive integer. Deducing next from (33.18) that

$$(33.24) \quad \psi^n = (\Delta+1)^n,$$

he applied (33.23) for  $\theta = \Delta$  and  $\theta' = 1$  putting  $nh = k$  in the expansion which thus resulted. Further, assuming  $n$  and  $h$  to increase and to diminish infinitely respectively (while  $k$  remained constant),  $\Delta/h$  becomes  $d_x$  and Taylor's theorem appears in the form

$$(33.25) \quad \psi = 1 + hd_x + \frac{h^2 d_x^2}{2!} + \frac{h^3 d_x^3}{3!} + \dots$$

[1837, 182-3].

Murphy next considered  $\psi$ ,  $\psi'$ ,  $\psi_1$ , to be operations which change  $x$  to  $x + h$ ,  $x + h'$  and  $x + h + h'$  respectively, hence connected by

$$(33.26) \quad \psi\psi' = \psi_1.$$

Substituting these operations by their Taylor expansions (33.25), he obtained a relation "which may be verified by actually compounding the two polynomials of the first member" of (33.26) after the substitution. "In this act of verification the operations  $hd_x$ ,  $h'd_x$  have only such properties as are common to any two linear operations which are relatively free". Hence, combining (33.25) with (33.26) and consequently replacing  $hd_x$ ,  $h'd_x$  by  $\theta$  and  $\theta'$  respectively, he deduced

$$(33.27) \quad (1+\theta + \frac{\theta^2}{1.2} + \dots) (1+\theta' + \frac{\theta'^2}{1.2} + \dots) = 1 + (\theta+\theta') + \frac{(\theta+\theta')^2}{1.2} + \dots$$

as a theorem for any finite number of distributive and commutative operations  $\theta$ ,  $\theta'$ , ... [1837, 183-4].

The final steps towards the definition of  $e^\theta$  as an operation for any linear operation  $\theta$  had as follows. By  $\theta$  he denoted the operation

$$(33.28) \quad 1 + \theta + \frac{\theta^2}{1.2} + \dots$$

Assuming next (33.27) for a finite number  $n$  of operations  $\theta$ ,  $\theta'$ ,  $\theta''$ , ..., he let  $\theta = \theta' = \theta'' \dots$  determining thus  $\theta^n$  by

$$(33.29) \quad \theta^n = 1 + n\theta + \frac{n^2\theta^2}{1.2} + \dots$$

Putting further



$$(33.30) \quad \varphi = 1 + \frac{\theta}{m} + \frac{\theta^2}{1.2m^2} + \dots$$

$m$  a positive integer, he obtained "by the same principles" that

$$(33.31) \quad \varphi^m = \theta$$

and showed that (33.29) holds true for  $n$  rational [1837, 185].

Finally, letting  $\varphi^n$  stand for the right-hand side of (33.29), and  $\Omega$  for

$$(33.32) \quad 1 - n\theta + \frac{n^2\theta^2}{1.2} - \frac{n^3\theta^3}{1.2.3} + \dots$$

he showed, based upon (33.29), (33.32) and (33.27), that  $\Omega\varphi^n = 1$ , or, introducing a subject  $u$ , that

$$(33.33) \quad [f(x)]\Omega\varphi^n = [f(x)]\varphi^n\Omega = f(x).$$

The latter formula expressed the fact that  $\Omega$  and  $\varphi^n$  are "mutually inverse operations". Thus,  $\theta^{-n}$  is defined by (33.32), and (33.29) holds true also for negative integers [1837, 185-6].

Now, assuming  $\theta=1$ , the operation denoted by  $\theta$ , (33.28) stands for multiplying by  $e$ . Thus, by (33.29) it follows that

$$(33.34) \quad e^n = 1 + n + \frac{n^2}{1.2} + \dots$$

Murphy remarked that the properties of the series (33.34) "are common to those in a series where  $\theta$ , any linear operation, is put for  $n$ ". Hence "we may write the purely symbolical identity":

$$(33.35) \quad e^\theta = 1 + \theta + \frac{\theta^2}{1.2} + \dots;$$

thus the operator  $e^\theta$  is rigorously defined. Putting  $\theta = hdx$ , we have  $\psi = e^{hdx}$  and hence, by (33.35) and (33.25)-(33.27), formula (33.22) is established together with the validity of the symbolical form of Taylor's theorem. The last remark was that, "having seen in the course of the investigations ... the signification of the indices of operations when fractional negative or even purely symbolical of linear operations, it is easy to prove by similar steps that in all cases where  $\theta$ ,  $\theta'$  are relatively free", (33.23) holds true [1837, 186-187].

His next concern was evidently with the definition of the inverse of an operation  $\theta$ , a concept involved in (33.33) above. The definition of  $\theta^{-1}$  was given by

$$(33.36) \quad (u)\theta = y \longrightarrow (y)\theta^{-1} = u \longrightarrow (y)\theta^{-1}\theta = y.$$

However, no discussion of the uniqueness of  $\theta^{-1}$  or of mutually

inverse operations, according to (33.33) was to follow. It was deduced readily from (33.36) that

$$(33.37) \quad (\theta\theta')^{-1} = \theta'^{-1}\theta^{-1},$$

a property anticipated by Herschel in 1813 (see (24.18)), as well as the proposition that

$$(33.38) \quad \text{"If } \theta \text{ is linear, then } \theta^{-1} \text{ is linear too."}$$

For the proof of (33.38), Murphy let

$$(33.39) \quad [X]\theta = X_1, \quad [\xi]\theta = \xi_1.$$

Then, by means of (33.15), (33.36) and (33.39) it was shown that  $[X_1 + \xi_1]\theta^{-1} = X + \xi = [X_1]\theta^{-1} + [\xi_1]\theta^{-1}$ , hence (33.38) was proved [1837, 187-8; see also (16.23)-(16.32)].

Murphy went on to define the "appendage"  $P$  of  $\theta^{-1}$  in a more lucid and general way than that by Sarrus (16.30) or in his own earlier definition (33.13). Assuming  $\theta$  to be such that  $[P]\theta = 0$ , it followed that, if  $[X]\theta = y$ , then  $[X + P]\theta = y \rightarrow [y]\theta^{-1} = X + P$ , or since  $y = y + 0$  and  $\theta^{-1}$  distributive, that

$$(33.40) \quad P = [0]\theta^{-1}.$$

" $P$  will express a form", he wrote, "but its magnitude must be susceptible of an infinity of values, that is, it contains arbitrary constants which enter as multipliers" [1837, 188].

Replacing  $\theta$  successively by  $\psi^{-n}$ ,  $d_x^{-n}$  and  $\Delta^{-1}$ , he determined various forms of  $P$ , improving over his earlier treatment in (33.13). For example, by Taylor's theorem he obtained  $[0]dx^{-1} = C$ , thus  $[0]dx^{-2} = \dots = [C+0]dx^{-1} = \dots = Cx + C'$ , or inductively

$$(33.41) \quad [0]d_x^{-n} = A_1x^{n-1} + A_2x^{n-2} + \dots + A_n,$$

where  $C, C', A_1, \dots, A_n$  are constants [1837, 188-9; see also (9) above]. Murphy's "appendage" stands for what we call as the "kernel" of a linear operation [Smith 1984, 24].

Having concluded his foundational study of inverse, linear operations, he went on to study the function of compounds of non-commutative, linear operations, in other words their transmutation. Let  $\theta_x$  stand for an operation fixed relative to  $\psi_x$ ,  $\Delta_x$  and  $d_x$  which are given by (33.18). He first assumed  $\psi_x$  to operate on  $\theta_x$  denoting the result as

$$(33.42) \quad [\theta_x]\psi_x = \theta'_x = \theta_{x+n} = \overline{\theta_x\psi_x},$$

where the bar over a compound denotes that it has to be considered as one operation. By (33.42) he concluded that

$$(33.43) \quad [u]\theta_x\psi_x = [u]\psi_x\theta'_x,$$

or, omitting the subject  $u$ ,

$$(33.44) \quad \theta_x\psi_x = \psi_x\theta'_x = \psi_x\theta_{x+n} = \psi_x \overline{\theta_x\psi_x}.$$

On similar lines, replacing  $\psi_x$  suitably by means of (33.18) in terms of  $\Delta$  in (33.44), he found that

$$(33.45) \quad \theta_x \Delta_x = \Delta_x \overline{\theta_x \psi_x} + \overline{\theta_x \Delta_x}.$$

Moreover, it was consequently deduced that

$$(33.46) \quad \theta_x d_x = d_x \theta_x + \overline{\theta_x d_x}$$

[1837, 190-1]. The bar over the compound vaguely indicates associativity, but Murphy did not specify this law in his work.

By induction he obtained generalized formulae of transmutation. For example, (33.46) led by iteration to

$$(33.47) \quad \theta_x d_x^n = d_x^n \theta_x + n d_x^{n-1} \overline{\theta_x d_x} + \frac{n(n-1)}{1.2} d_x^{n-2} \overline{\theta_x d_x^2} + \dots$$

If  $\theta_x$  stands for quantity, he remarked, (33.47) is reduced to Leibniz's theorem [1837, 192]. In fact, Leibniz's theorem

$$(33.48) \quad \frac{d^n(uv)}{dx^n} = v \frac{d^n u}{dx^n} + n \frac{d^{n-1} u}{dx^{n-1}} \frac{dv}{dx} + \frac{n(n-1)}{1.2} \frac{d^{n-2} u}{dx^{n-2}} \frac{d^2 v}{dx^2} + \dots$$

was to be frequently applied in the theory of D-operator calculus from 1839 onwards [see 4.4; 5.3-5.7]. Murphy went on to show that the general formulae of transmutation, such as (33.47), holds true also for  $n < 0$  [ibid, 193].

He considered next the inversion of binomial operations  $\theta - \theta'$  for any linear operations  $\theta, \theta'$ . On lines almost identical to those followed by Sarrus, Murphy arrived at the latter's inversion formula (16.36) in the form

$$(33.49) \quad (\theta - \theta')^{-1} = \theta^{-1} + (\theta^{-1} \theta') \theta^{-1} + (\theta^{-1} \theta')^2 \theta^{-1} + \dots + (\theta^{-1} \theta')^n \theta^{-1}.$$

If  $\theta, \theta'$  are commutative, then  $(\theta - \theta')^{-1} = \theta^{-1} (1 - \theta^{-1} \theta')$  and  $(\theta - \theta')^{-1}$  is given by (33.49) in an infinite series. Among the applications of (33.49) was the deduction of Taylor's theorem [1837, 194-6].

He next assumed three linear operations  $\theta, i$  and  $k$  connected by the equation

$$(33.50) \quad \theta i = ik.$$

He called the symbol  $i$  the "intermediate", while  $\theta$  and  $k$  the "extreme operations" in the equation (33.50) and went on to show first how to find  $k$  or  $\theta$  given the other two operations [ibid, 196-200] and next, given the two extremes how to determine  $i$  [ibid, 200-2]. We focus on the first case which is of most importance.

Given  $i$  and  $\theta$  it follows immediately from (33.50) that

$$(33.51) \quad k = i^{-1} \theta i.$$

a formula similar to Babbage's transform  $\varphi^{-1}f\varphi$  or (28.11) <sup>(13)</sup>. By successive application of  $k$  to (33.50) he obtained

$$(33.52) \quad \theta^n i = i k^n$$

for  $n$  positive, negative or fractional. Formula (33.52) led to

$$(33.53) \quad f(\theta)i = i f(k),$$

where  $f(\theta)$ ,  $f(k)$  represent "aggregates of any similar" powers of the operations involved. It was thus proved that "intermediate operations", like  $i$ , "are also intermediate between any operations which are the same functions of the extremes" [1837, 196-7].

Let now  $\theta = \frac{d}{dx}$ ,  $i = e^{ax}$  and  $k$  required so as (33.50) holds true. By means <sup>of</sup> (33.46) it was readily found that

$$(33.54) \quad e^{-ax} d_x = (d_x - a) e^{-ax},$$

hence  $k = d_x - a$ . Formula (33.54) was generalized by (33.53) to

$$(33.55) \quad f(d_x) \cdot e^{ax} = e^{ax} f(d_x - a),$$

and letting  $i = e^{\varphi(x)}$  it was found on similar lines that

$$(33.56) \quad f(d_x) e^{\varphi(x)} = e^{\varphi(x)} f(d_x - \varphi'(x))$$

[1837, 198-9]. Formula (33.54) was initially given by Cauchy in 1827 as (17.42) after Brisson, and was independently deduced by Gregory in 1839 by means of Leibniz's theorem [see (44.14)].

Replacing  $a$  in (33.54) successively by  $0$ ,  $h$ ,  $2h$ , ...,  $(n-1)h$  and letting  $y = e^{-hx}$ , another symbolical formula prominent later on for its role in the solution of differential equations was deduced, namely

$$(33.57) \quad d_x(d_x - h)(d_x - 2h) \dots (d_x - (n-1)h) = d_y^n h^n e^{nhx} \quad (14).$$

While there is a striking absence of any applications in the realms of differential equations (as by Français's (16.13) or Sarrus's (16.37)), Murphy ended his paper with two applications of the formulae (33.54) and (33.57) in the realms of the differential calculus. Based first upon (33.57) and (33.23), and letting  $h = 1$  and  $y = e^x$ , he obtained the expansion of the binomial  $(a+b)^{ax}$ , where  $a$  and  $b$  are quantities, in the form

$$(33.58) \quad (a+b)^{ax} = a^{ax} \left[ 1 + d_y \frac{by}{a} + \frac{d_y^2}{1.2} \left[ \frac{by}{a} \right]^2 + \dots \right]$$

[1837, 202]. For, according to his remarks which followed (33.35), formula (33.23) holds true for any index  $n$  <sup>(15)</sup>.

Murphy's final application concerned the deduction of a formula which gives  $d^n u / dy^n$  in terms of  $d^1 u / dx^1$ ,  $(dy/dx)^1$ , and  $d^1 y / dx^1$  for  $i = 1, \dots, n$ . Two methods were displayed. In the first he made use of (33.54) in order to determine the expansion of

$$(33.59) \quad \frac{u_1 dx \dots u_n dx}{u_1 u_2 \dots u_n},$$

where  $u_i$  were functions of  $x$  or other operations fixed relative to  $dx$ . The expansion of (33.59) in powers of  $dx$  was the result of a lengthy combinatorial procedure. Putting in this complicated formula  $u_1 = u_2 = \dots = u_n = u$  and changing  $u$  to  $1/u$  he deduced the expansion for  $(dx u^{-1})^n$ . It now rested to replace  $u$  by  $dx/dy$  in order to obtain  $(dy)^n$  in terms of  $(dx)^1$ ,  $d^1y/dx^1$  and  $(dy/dx)^1$  as required. Applying his last formula to any subject  $u$  he could determine the derivative of a function of any order in terms of another variable. For example, for  $n = 3$ , he got

$$(33.60) \quad \frac{d^3x}{dy^3} = 3 \left[ \frac{dy}{dx} \right]^{-5} \left[ \frac{d^2y}{dx^2} \right]^2 - \left[ \frac{dy}{dx} \right]^{-4} \frac{d^3y}{dx^3}$$

[1837, 203-207].

The paper ended with formula (33.60), as a result of a totally different procedure. This time he drew on his [1833a] on algebraic equations, namely on the following rule:

"If  $\varphi(x)=0$  be an equation which contains only entire positive powers of  $x$ , and  $f(x)$  any other function of the same kind, of which the differential coefficient ... is  $f'(x)$ , then the value of  $f(x)$  will be found by taking the coefficient of  $1/x$  in the expression  $-f'(x) \cdot \log \varphi(x)/x$ "

(33.61)

[1833a, 133-4; 1837, 208]. Suitably choosing functions  $f(x)$  and  $\varphi(x)$ , he applied Taylor's theorem determining thus a new formula for  $d^nu/dy^n$  in terms of  $dx$  which for  $u=x$  and  $n=3$  led once more to (33.60) [1837, 209-210].

The impact of Murphy's paper [1837] was a rather subtle one. Very few of his original results were acknowledged and actually none of his methods was reproduced. Though his establishment of the properties of inverse operations (33.7) and (33.8) played a most significant role in the development of the D-operator calculus, these properties were taken for granted, and only Carmichael bothered to insist on their importance, acknowledging Murphy in his [1855]<sup>(16)</sup>. When Gregory first discovered Murphy [1837] in 1841, he incorporated formula (33.60) in his [1841, 30-31] drawing moreover on (33.61) [1841, 242]<sup>(17)</sup>. Additionally Boole acknowledged Murphy [1837] in his [1844; see 4.5, (5)].

The rest of Murphy's early work had even less impact. Apart from his study of definite integrals in his Electricity [1833c], for which he was acknowledged by De Morgan [1842c, 644], his work on definite integrals had, despite its originality, "no bearing on the main line of development in the subject" [Deakin 1981, 373; see also (3) above]. His work produced from 1837 onwards, was neither particularly original, nor of any impact [Smith 1984a, 27-29]. Under De Morgan's suggestion<sup>(18)</sup> he produced a Treatise on algebraic equations [1839] incorporating in it a collection of results by Sturm, Fourier, Lagrange and Gauss on algebraic equations. The book was enriched with methods and results concerning recurring series, continued fractions, approximate methods, certain of them drawn from his [1833b]. In the introduction he claimed that "no treatise with exclusively the same object has been published of late, as far as I know, either at home or abroad" [1839, iii].

We conclude our account on Murphy by commenting upon his paper "On the resolution of equations in finite differences" [1838] read for the Cambridge Philosophical Society in 1835. This curious paper combines concerns and methods on finite difference and functional equations which bear similarities with those displayed earlier by Lagrange, Monge, Laplace, Babbage and Herschel. It opened with the remark [1838, 91]:

With respect to those [equations] of higher degrees, scarcely anything has been done to assist in obtaining explicitly an algebraical expression for the unknown quantity\*. The utility of solutions for such equations, occurring, as they do, in the theory of chances, is more apparent by the proof which they afford of the expansibility of various kinds of successive functions on which some doubt has hitherto existed.

The asterisk<sup>†</sup> referred to a footnote which reads as "In the great work of Lacroix this subject is entirely passed over". A similar comment was included in Herschel's letter [see 3.3, (8)] in connection with the Lacroix translation in [1816, 2.7]. Like Herschel, Murphy got occupied with the solution of equations which determined the form of successive functions such as (24.11) or

$$(33.62) \quad u_{x+1} = \varphi(u_x)$$

[1838, 94, 96, 98-99]. Recalling Herschel's concerns in 1813,

Murphy opened the paper with an enquiry on the differential coefficient of the successive function (23.3), or of:

$$(33.63) \quad e^{e^{e^{\dots (x \text{ times})}}}$$

[1833, 92].

Murphy built gradually his theory so as to apply to the most general form of (33.62),

$$(33.64) \quad \varphi(x, u_x, u_{x+1}, \dots, u_{x+n}) = 0.$$

Through the transform

$$(33.65) \quad u_x = f(\gamma^x) = f(z),$$

where  $f$  and  $\gamma$  are unknown functions, he effected the reverse reduction from that employed by Laplace and Herschel, that is from finite difference to functional equations; he thus reduced (33.64) to

$$(33.66) \quad F(z, f(z), f(\gamma z), \dots, f(\gamma^n z)) = 0,$$

an equation equivalent to (26.16). Murphy's reverse procedure by means of logarithms, is reminiscent of Monge's method in [1780] (see 1.4 and De Morgan 1836, art.250-251]. Drawing next on (22.10) and Maclaurin's theorem, he applied Lagrange's method in solving (33.66) in series form [1838, 105-6; 1.4, (9); (22.5); (26.1)].

Murphy's procedure is more general and few numerical examples are given [1838, 98-101]. Among his applications we mention his determination of the finite product

$$(33.67) \quad \sin m \cdot \sin m \dots \sin m u_0,$$

$u_0$  known, in series form. Formula (33.67)- determined in the form of an infinite series- reminds us of Babbage's concern with finite continued products as in (24.1).

So, though not based on Babbage's and Herschel's work, Murphy followed the trend of the Analytical Society. In this he slightly preceeded De Morgan whose work is discussed in 3.4-3.9. While both mathematicians showed simultaneously interest in inverse operations and functions, the former's treatment is far more formal and rigorous than the latter's conceptual approach.

3.4 De Morgan on the instruction of algebra: 1828-1835; early epistemological influences and concerns.

Educational and other concerns motivated English mathematicians to justify negative and imaginary numbers<sup>(1)</sup>. The first

major attempt towards this direction was taken by Peacock in his Treatise on algebra [1830]. He based his symbolic algebra on the principle of the permanence of equivalent forms, according to which, identities holding true in arithmetic were bound to hold true for arbitrary symbols independently of interpretation <sup>(2)</sup>. This mode for algebraic extensions provided mathematicians with algebraic freedom in defining rules for operations; however, it had several weak points and often led to dubious results. W.R. Hamilton and Whewell raised objections towards Peacock's approach, obstructing thus the development of abstract algebra<sup>(3)</sup>.

De Morgan was impressed by Peacock's Algebra [1830] which influenced his own work on the foundations of algebra in the early 1840's. However, it took him five years to embrace Peacock's approach and to accept, critically, his principle of equivalent forms in his review [1835a] of Peacock's [1830]. From then onwards he gradually established his own approach which partly agreed with, and partly diverged from that of Peacock. By 1849 he had constructed a symbolic algebra defining the laws of its symbols independently of interpretation, often oscillating between meaningful and abstract algebra [Pycior 1983; 3.9].

Peacock's, De Morgan's and Gregory's work on algebra has been studied extensively in our century<sup>(4)</sup>. Most of the recent studies focus on showing -anachronistically- the insufficiency of their work in attaining the standards of modern abstract algebra. As far as De Morgan is concerned, the critical attitude of his contemporaries and his disinclination to stick to a purely symbolical approach, were among the reasons for his failing to accomplish his study in the modern sense. Contrary to most 20<sup>th</sup> century historians who viewed his work through the eye of a formalist<sup>(5)</sup>, Richards [1987] examined aspects of it purely through the angle of a historian, stressing his educational and historical concerns and his tendency for conceptual understanding.

All these recent studies have been consulted in our thesis. As our concern is orientated principally towards his own later work in logic, we will view De Morgan's work strictly through his own eyes, omitting any comparison with contemporary mathematics. Thus, instead of stressing any minor or major changes in his style, or his ambivalent attitude towards modern symbolic algebra, we will point out those issues which underlined his mature work within both algebra and logic. The core of our research in



this chapter is his treatise on the calculus of functions [1836] where most of the issues under consideration made their first, solid appearance [3.5-3.8]. We focus here on the early stage 1828-1835 during which he expressed his essential educational and epistemological views on algebra, hinting, at so doing, at the importance of logic as preliminary to the instruction of the higher branches of mathematics.

De Morgan graduated from Cambridge as a fourth wrangler in 1827, but pursued his career as a professor of mathematics at the newly formed University College in London<sup>(6)</sup>. Influenced by his tutors, Airy, Peacock and Whewell, he showed interest in promoting the work put forward by the Analytical Society [see 2.3,(4); 2.4,(7); 2.5,(13),(17); 2.6,(15); 2.8,(9); 2.9]. In this sense we can regard him as a Cambridge mathematician, or as a kind of a "satellite" of the Society [Richards 1987, 10]. Like Whewell, he had a singular historical approach<sup>(7)</sup>. That is, he was not concerned so much with inventing new methods, as with critically studying and extending the work of his predecessors, seeking for a continuity in the evolution of science and a unity in its methods. Admitting ambiguity or error in the course of mathematical progress, he bothered very little for rigorous demonstrations; the conceptual understanding of fundamental notions and the elucidation of first principles were among his prime concerns.

De Morgan enjoyed a high reputation as a teacher in London University [Howson 1982, 82-92]. Shrewd to perceive the drawbacks of the instruction of mathematics at his time, he devoted most of his early work to educational concerns, in the form of lectures, reviews and elementary textbooks and treatises. In fact, certain of his remarks on education are still valid in our days. For example, his book On the study and difficulties of mathematics [1831] saw a reprint in America as recently as 1943 [Mac Farlane 1916, 21-22; Pycior 1983, 213-216].

His early views on the instruction of mathematics bear evident signs of his influence from empirical philosophers, such as Locke and Condillac. This influence weakened as the time passed by, but there still exist instances from 1836 onwards where traces from his early background make their subtle appearance [3.5-3.6, 3.9]. Locke, Condillac and the latter's disciple, Lacroix, featured prominently in his [1831]. Moreover, his lec-

ture at the opening of classes in University College in 1828 [Pycior 1983, 212-3] and his lecture "Remarks on elementary education in science" [1830] reveal direct influences from Locke and Condillac. We see this in the passage which follows below in which he distinguished for the first time between the "science" and the "art" of arithmetic [1830,13]:

I would even go so far as to say, that the science of arithmetic is more easy than the art, and that the labour usually required for the attainment of practical correctness might be very materially lessened by the introduction of theoretical principles. It has been well observed by Condillac, in treating of this very subject, that a rule is like a parapet of a bridge; it may keep a careless passenger from tumbling over, but will not help him to walk forward.

De Morgan held that the principles of algebra and geometry have not obtained "their due importance in our elementary works on these sciences". He stressed in the "Preface" to his [1831], that he would not enter into any disputes on negative or imaginary numbers, but would follow, instead, the method adopted "by several of the most esteemed" Continental writers, "of referring the explanation to some particular problem, and showing how to gain the same from any other". The originality of his book lay principally in the illustration of those methods in an elementary and useful way.

The book was mainly devoted to suggestions and warnings regarding inductive proofs <sup>and</sup> the empirical treatment of negative numbers<sup>(e)</sup>, and to a discussion of the nature of mathematical truths as derived by observation. Before we point out few characteristic instances, we would like to add that a part of [1831] was devoted to a commentary on the instruction of geometry which motivated him to reveal his belief that logic was essential in mathematical reasoning. In fact, he regarded geometry easier than algebra, on the grounds that it involved symbols of a "less general nature" [1831,65]. Aiming at the establishment of rigorous deductive reasoning, he introduced the preliminary rules and notions of Aristotelian syllogism applying them to the demonstration of Pythagoras' theorem [1831, 68-76; 6.4-6.5].

Discussing the method of induction, he chose the binomial theorem as a heuristic example suggesting "Whenever a demonstration appears perplexed, on account of the number and generality

of the symbols, let some particular case be chosen, and let the same demonstration be applied" [1831, 61]. To justify induction, a mathematician's "most powerful" engine of demonstration, he quoted Laplace:

At first, people were afraid to admit the general consequences with which analytical formulae furnished them; but a great number of examples having verified them, we now, without fear, yield ourselves to the guidance of analysis through all the consequences to which it leads us, and the most happy discoveries have sprung from the boldness. We must observe, however, that precautions should be taken to avoid giving to formulae a greater extension than they really admit, and that it is always well to demonstrate rigorously the results which are obtained<sup>(9)</sup>.

In accordance with Condillac's dictums [1.8], he held that a student should construct a "syllabus of results, unaccompanied by any demonstration" in order to "acquire a correct memory for algebraic formulae" saving thus time and labour. The merits of classification of mathematical forms and the tabulation of results would be pointed out later on in his [1836, art. 9. 25; 3.6]. In [1831, 63] he argued that in this direction, both the preceptor and the pupil "will derive great advantage from the perusal" of Lacroix Essais [1828] and of Condillac La langue [1798]. He further suggested the study of French, recommending

Bourbon's textbook on algebra; De Morgan had translated the first chapters of the latter book in 1828 [Richards 1987, 12-3].

History always provided De Morgan with arguments to justify his claims. Considering the rules

$$(34.1) \quad +ax-b = -ab, \quad -ax-b = +ab,$$

he wrote that in the work of Diophantus we find a principle equivalent to (34.1) "admitted as an axiom without proof or difficulty. In the Hindoo [sic] works on algebra, and the Persian commentators upon them, the same thing takes place" [1831, 63]. In his review of Wood's Algebra he stressed again "but if it be absolutely necessary.... to force upon the student, at his first entrance into algebra, such equations as [(34.1)]; at least this ought to be done upon authority and no fictitious proof ought to be added" [1832, 280].

What he tried to convey was that at his first steps the student should rely empirically upon examples, the absence of proof

being far less damaging than the presence of an unsatisfactory one. He claimed that "a writer on algebra, who proceeds upon the principle of demonstrating whatever he asserts, should put the state of the argument fairly before the pupil; and this cannot be done until the latter has obtained some degree of preliminary knowledge". And switching from Wood's own "proof" of (34.1) to that of the binomial theorem, he added: "The author seems to think he is bound to give either a proof, or something that looks like one. We hold, that the less that which is not a proof, is made to look like one, the better" [1832, 281, 283].

Despite his critical remarks, De Morgan approved of Wood's treatise for Cambridge undergraduates. He held that the union of Wood's<sup>(10)</sup>, Peacock's and Bourdon's books on algebra "would comprise all that need to be read on this subject by any student, whatever rank he may hope to obtain on the tripos" [1832, 277-8]. Prior to this he commented on the freedom and power of lecturers and examiners to establish reforms at Cambridge.

The moderators, or examiners, who are usually younger masters of arts, and come to the matter with the newest ideas going, feel that great scope is allowed, and do not confine themselves to any book or system, further than may appear advisable to themselves. Hence any great improvement is of comparatively easy introduction; it only needs one moderator who does not fear the appearance of singularity<sup>(11)</sup>.

He added that it was "one individual" -implying Peacock- who effected the introduction of Continental notation, and hence of Continental mathematics, in 1817 [1832, 276]. De Morgan was a defender of Whewell's "liberal education". However, he soon realized that any strict adherence to it was an impediment in the development of pure mathematics, and, in the adoption of Peacock's algebra<sup>(12)</sup>. In 1835 he had endorsed Peacock's work and tried hard from then onwards to establish algebra as a basic component of liberal education.

De Morgan's review of Peacock's Treatise on algebra [1830] was written in two parts, cited here as [1835a]. However, only one third of his review dealt explicitly with Peacock's work. Above all he tried to call attention to the study and instruction of algebra, a branch of mathematics much neglected in English Universities. In 1835, he touched upon the notion of symbolical

algebra in his book The elements of algebra preliminary to the differential calculus [1835b]<sup>(13)</sup>. The title of this book is in full accordance with the following comments he included in his review [1835a, 97-8] in connection with the erroneous priority given to the calculus at Cambridge:

In speaking therefore of the greater part of our elementary treatises, we consider them as good for the instances they give, and no more; we have never seen independent power obtained by means of them. That which the student afterwards acquires he has to labour for fresh; he struggles with an algebraic principle while he is already deep in the Differential Calculus, and gets his first ideas of a common process of numbers out of his treatise on mechanics.

He further argued on the utility of algebra as prior to higher branches of mathematics saying [1835a, 101]:

Because algebra is considered an analytical art, it seems to have been imagined, that learners must be analysts from the very beginning. And the attempt has been to make them analysts by rule; and to make them masters of every case that may arise, by telling them the result to which some of their predecessors have come in a few cases, without insisting on any one of the principles by which these results were obtained.

He insisted that certain rules a student is forced to learn are hardly of daily application. He considered, for example, Laplace's Mécanique where "The numerical solution of equations occurs in a few instances; while many simple principles, not mentioned in our books, are to be applied in almost every page of the work mentioned" [1835a, 102].

Before discussing Peacock's algebra, he emphasized the importance of sound, logical reasoning. A beginner should first of all be concerned with logic "in its most exact form". Since logic is an easier science than algebra, he claimed that the student should be acquainted with it "before he can ever become a mathematician" [1835a, 293]. For, as he wrote in [1835b]:

The art of reasoning is exercised by mathematics, not taught by it. On the contrary there are principles of other branches of reasoning which are not employed in most branches of mathematics<sup>(14)</sup>.

In his review he assumed a random demonstration of the abstract form "A is B, B is C, C is D; therefore A is D" holding that the student should first focus on the nature of this deductive reasoning as a whole, and next on the proof of the specific hypothetical propositions "A is B", adding [1835a, 95]:

His reasoning will then be perfectly correct: for he must remember that reasoning is not the affirmation or negation of propositions, but the right deduction of them from one another; and that though the certainty of mathematical conclusions depends upon that of the fundamental propositions, the correctness of mathematical reasoning has nothing whatever to do with that circumstance<sup>(15)</sup>.

In an attempt to explain in simple terms the difference between arithmetic and algebra he wrote [1835a, 98-99]:

By arithmetic we add or subtract; by algebra we find out whether to add or subtract. The latter is therefore a science of investigation, without any rules except those which we may please to lay ourselves for the sake of attaining any desirable object. The hypotheses, the meaning of the symbols laid down, are in our own power: subject only to the great rule of all search after truth, that nothing is to be asserted as a conclusion, more than is actually contained in the premises<sup>(16)</sup>.

So he initiated his reader into the freedom of symbolic algebra but warned him, as in his [1831], against the erroneous deduction of general conclusions from particular premises. Elaborating over his earlier approach, he distinguished induction from another method of generalization peculiar to algebra. The latter method "does not arise from drawing conclusions wider than the premises will logically admit, but from arbitrary conventions by which terms in common use are made to signify less than their vulgar meaning implies, the algebraical meaning being always a part, and not the whole of common meaning" [1835a, 103]. Preparing the ground for Peacock's principle, he added:

To the mathematician we have supposed we present the algebraical meaning; to the common student we present at first the whole arithmetical meaning; and, when the necessity arises, we show him the convenience of restricting the sense he has hitherto used, of throwing away part of what was necessarily considered as implied in the word, retaining only those propositions which are true of the

restricted meaning, and of course rejecting those which are true only of the fullest sense of the word. It therefore follows that certain formulae may be chosen, not as consequences of any meaning given to the symbols, but as definitions of the symbols themselves.

Followed few elucidating examples. He discussed the arithmetical and algebraic meaning of the operations  $+$ ,  $-$  as in

$$(34.2) \quad a + b - b = a$$

[1835a, 103-4]. He then provided the identity

$$(34.3) \quad (a+b) \times (a-b) = axa - bxb^{(17)}$$

which holds true under the "material alteration" into

$$(34.4) \quad \sin(a+b) \cdot \sin(a-b) = \sin^2 a - \sin^2 b,$$

holding that these instances exemplify changes of meaning which do not alter the truth of particular theorems. He went on to interpret (34.3) geometrically introducing the concepts of length, angle and  $\sqrt{-1}$ , extending addition by defining the operation  $a + 'b$

$$(34.5) \quad a + 'b = (a^2 + b^2 + 2ab \cos(\alpha - \beta))^{1/2},$$

where  $\alpha$  and  $\beta$  are the angles that the lines  $a$  and  $b$  make with the given axis. He concluded by stating that "the methods and results of an extension have been matured, before the extension it self has been formally made" [1835a, 105-9].

He opened the second part of his review by calling attention to the crucial distinction between "extension" (as in the above examples) and "arbitrary alteration" into which a student may accidentally indulge. Speculating over the instruction of the former method, he wrote: "Our method would be therefore to wait for every extension until the period when it becomes necessary, that is, until some result of arithmetical algebra appears, which is not explicable on arithmetical principles" [1835a, 294]. This method, by which a student is presented with a formal definition, extension or complicated application of a previously known concept in elementary terms at the moment when is ready to confront it, is reminiscent of Euler and Lacroix [1.8, (6), (11); 2.9, (3)] and was followed by De Morgan in his [1836; 3.5-3.7].

De Morgan set off to consider Peacock's work in stages; first in connection with the elementary student (p. 298), next in connection with the advanced (p. 300) and finally "in a purely mathematical point of view, as the exposition of algebra in its most extended form" (p. 303). He pointed out the merits and defects of Peacock's approach in each of these cases, concluding

that Algebra [1830] should be regarded as a "high book at Cambridge" and as a "difficult, but logical" work on the whole [1835a, 300-309]. But before we proceed to De Morgan's own comments let us cite few instances from Peacock's Algebra, as quoted in [1835a, 301-2] from Peacock [1830, xxi]:

If we should rest satisfied with such assumed rules, for the combinations of symbols and signs by such operations as are perfectly independent of any interpretation of their meaning [...] we should retain in the results obtained all the symbols which were incorporated, without possessing the power of any further simplification.

Peacock introduced next arithmetic as the "science of suggestion" in his "symbolical algebra" as follows:

It is at this point that the essential connexion of algebra and arithmetic may be properly said to commence: for a science of mere signs and symbols must terminate in the consequences of their laws of combination, unless they can be associated by interpretation with real operations upon real magnitudes with specific representations: and it is with a view to such application of this science that we have considered [...] arithmetic or arithmetical algebra as the science of suggestion: that is, as the science whose operations and the general consequences of them should serve as guides to the assumptions which become the foundations of symbolical algebra<sup>(18)</sup>.

Later on in his book, Peacock formulated his "principle of the permanence of equivalent forms", hereafter cited as PEF:

Whatever form is algebraically equivalent to another when expressed in general symbols, must be true, whatever those symbols denote.

Conversely, if we discover an equivalent form in arithmetical algebra, or any other subordinate science, when the symbols are general in form though specific in their nature, the same must be an equivalent form when the symbols are general in their nature as well as in their form<sup>(19)</sup>.

"By this principle", wrote De Morgan, "we conceive Mr. Peacock to mean that, in extended algebra, the propriety of putting the sign = between two symbolical expressions is not de-



pendent upon the specific values of the expressions, or of the symbols they contain" [p 308]. He endorsed the first part of the principle as that on which "all the definitions are built", but raised doubts on mathematical and pedagogical grounds saying:

On any other supposition, we should hold this principle to a taking for granted of a nature wholly inadmissible in a pure science. What more does the student of ordinary algebra want, or on what do the ideas depend, which have led Poisson, Cauchy, and others, to reason only on convergent series, except doubts upon the principle of equivalent forms? But the difference between Mr. Peacock and the common algebraists is this, that whereas the latter assume the principle without giving their fundamental assumptions the necessary universality of meaning, Mr. Peacock constructs those fundamental assumptions with no other intention than to justify the use of the principle<sup>(20)</sup>.

The second part of the PEF amounted to the following: "If, in a process of any subordinate science, the limitations which prevent the general application of algebra do not happen to be introduced, the result is also one of algebra" [p 309]. Drawing on a geometrical example, he expressed the fear that "the student seeing definitions accompanied by a principle, which appears to be used independently, will be led to imagine that the principle is assumed of the definitions, instead of in the definitions" [p 309].

Despite these doubts, De Morgan considered Peacock's work as the "most original which have appeared in England, in pure mathematics, since the "Analytical Calculations" of Professor Woodhouse" [p 309]. Even 30 years later, these two works would feature prominently in his writings [3.9]. In the mean time he would use Peacock's principle and, notwithstanding his reference to Cauchy and Poisson, he would admit discontinuous series <sup>as well</sup> without problem [3.6;3.9]. In fact, he suggested only one modification in Peacock's approach:

If we were to recommend any alteration, it should be to abandon, in a great measure, the science of suggestion, except in the very early part of the work, where the methods of the two should either be placed in double columns, or the application to arithmetic [...] made a corollary of each theorem. But whether this be done or not, the work before us is one which the student must look forward to

reading, if he is really desirous of knowing the meaning of algebraical symbols in their widest sense<sup>(21)</sup>.

In the end of the review, De Morgan emphasized the importance of Peacock's work, the necessity to render the latter's approach accessible to students as well as the utility of a treatise to be composed on the etymology of mathematics. As these remarks strongly foreshadow his own immediate projects it is worth quoting them in full. We read at (p 310):

We are not without hope that elementary works on algebra will undergo some modification, with a view to rendering the reading of the present work easier. The higher parts of algebra, and all the applications of pure mathematics, require such a continual knowledge of the metaphysics of algebra, and comparatively so little acquaintance with the details which occupy the half of most of our works, that much time may be saved to the student by giving the first principles in a very different shape. If algebra will be hard, it must be hard; nothing is gained by substituting a science which is only easier, because it substitutes operations of the fingers for those of the head, and calling it algebra.

Reflecting further over the difficulties arising from the proper understanding of Peacock's terminology, he wrote:

To speak of symbols as things invented to obey rules, instead of being representations of quantity from the notion of which rules follow, certainly will introduce a very new set of idioms. [.....]. We mention this, which is more or less the accompaniment of all new expositions, to suggest the possibility of writing a treatise on the use of words in mathematics, and their connexion with symbols. Except perhaps Carnot, we know of no writer who has dwelt upon the meaning of his phrases. But we must perhaps wait until the general use of words, or logic, is considered as a necessary preliminary for mathematical studies<sup>(22)</sup>.

De Morgan contributed considerably on the general projects suggested in the concluding pages of his review. His own elementary algebra [1835b], which saw a second edition in 1837, formed a preliminary background to Peacock's symbolic algebra [see (13) above]. In 1839 his first book on logic was published, a proof of his claims above that logic was mostly useful to mathematicians

[see 6.4-6.5]. But, mostly relevant to the "treatise on the use of words in mathematics" suggested above, is the introductory part of his encyclopedia article on the "Calculus of functions" [1836]. For, as we shall see, prior to studying functional equations, he devoted some space to the elucidation of first principles and the etymology of this branch of mathematics displaying a blend of influences from Peacock, Babbage, Herschel, Carnot and, to a lesser extent, from Laplace, Arbogast, Condillac and Lacroix. The key notion in his foundational study was no other but that of "extension" which he had amply discussed in his [1831] and [1835a].

### 3.5 An introduction to De Morgan's treatise on the calculus of functions [1836]; plan of our study.

De Morgan's treatise on the "Calculus of functions", published in <sup>the</sup> Encyclopedia Metropolitana in 1836, is divided in 328 articles<sup>(1)</sup>. This division facilitates cross-references in his text and will serve in 3.5-3.8 as a means for citation. In this section we will comment upon the scope and content of De Morgan's lengthy work concluding with the plan of our study in the next three sections, and the main issues involved. We begin our account with De Morgan's opening statements; all the quotations and passages given below are drawn from the introductory art. 1.

De Morgan explains that he has to be very selective in the material chosen for exposition, for "To enter into a detail of the applications of the theory of Functions, would be to write a very large work". His wish is to present an "elementary" work, avoiding excessive generalizations and abstractions on the following grounds:

But in a work which professes to be so far elementary, that a student who has gone through the usual course of the Differential and Integral Calculus is supposed able to read it, nothing would be less useful than filling an Introduction on the Calculus of Functions with generalities of which particular cases have not been given<sup>(2)</sup>.

Artefacts which facilitate the reduction of a given complicated functional equation to a simpler one, he wrote, "is a matter of great interest, to be recorded for the future inventor".

But, to the "student", such information is "secondary", unless "the artifice throw light upon his first principles of notation, or add a general method to his power of combination". He distinguished between a researcher, or "inventor", and an "elementary writer", identifying himself with the latter. The difference between the two is the same as between "an invader and a colonist; the first must seize all he can, the second no more than is useful"<sup>(3)</sup>. Under the role of a "colonist" or "an elementary writer", the content of his "Treatise", or "Essay" -as he indifferently calls it- has as follows:

We have, therefore, confined ourselves principally to the elucidation of first principles, and the extension of the ideas which a student must be supposed to have on the subject of notation, together with such methods for the solution of functional equations as have succeeded in giving algebraic results. We have also given such methods of solution as have not yet been reduced to Algebra, in every case wherein we supposed either a useful view might be introduced, or in which we imagined it probable that the next step to be made would be a natural consequence of what has already been done.

In the course of this "elementary" essay, useful references to the work of all the previous contributors in this branch of mathematics are provided, in order to enable the "Mathematician" to see "where may lie the most probable help for any object he may have in view". Thus, while the treatise is devoted to a student of the calculus of functions, sufficient hints and references serve as a stimulus for the future researcher. De Morgan must have been aware of the lack of interest of his English contemporaries in this branch, for "there are perhaps in the whole of the Empire not twenty men who are likely to look into a work of reference with a view to see what has been done in the highest branches of the subject".

As we saw in [3.4.(22)], De Morgan had conceived the idea of an elementary work on the first principles and etymology of mathematics in his [1835a]. Indeed, on similar lines, he stressed now that among his goals was the elucidation of first principles, notation and mathematical procedures.

Our object will be gained if the student is led to such a knowledge of forms and familiarity with the results of general operations as

will render his grasp of ordinary mathematical language more intellectual and less mechanical<sup>(4)</sup>. The influence of signs on mathematical processes, or, to borrow a term, the etymological branch of Mathematics, is only incidentally a part of the subject-matter of most writings. But, of late years, it has been treated more for its own sake by the following writers, Babbage, Carnot, Cauchy, Herschel, and Peacock, into whose works the reader must look if he would judge of the present state of mathematical language.

In a footnote he provided references of the principal works of these authors, such as Carnot's Reflexions, including also Arbogast [1800]. Surprisingly, Babbage's philosophical paper (1827) is not cited. He concluded by apologising for the disorder of his presentation:

We have found it impossible to preserve as much connection as is desirable between the different articles of this Treatise. In a subject hardly yet considered fit for the student, and on which only one elementary work exists [by Babbage], of which we have any knowledge, the arrangement which the general methods of the Differential and Integral Calculus enables writers on those subjects to adopt, we soon found to be impossible. More officers must enter the service before functional equations can be drilled into discipline<sup>(5)</sup>.

Thus De Morgan commented upon the scope and content of his essay. These remarks show above all a deep educational and epistemological concern rooted in his experience of the inadequate instruction of mathematics -and, most particularly, of algebra [see 3.4]- in his country. However, as we shall see in 3.6-3.8, his pedagogical and philosophical aims were not carried out very successfully. In fact, the main part of the work was devoted to the solution of functional equations by means of various known or newly suggested techniques<sup>which were</sup> far from lucidly presented.

Despite a lack of direct reference to Condillac, De Morgan's work is implicitly influenced by the latter's [1798; see 1.8]. This<sup>is</sup> mainly perceived in the mode of exposition of the material studied. According to this mode, a student is first presented with a notion in the most simple manner possible. Then<sup>a few</sup> articles later, certain properties of that notion are introduced, followed by its proper definition and its accompanying "theorems". The

latter are quite often "demonstrated" empirically by means of elementary examples. The threads of this inquiry are to be resumed again and again in the course of the treatise where a more advanced study takes place and new, general results are given<sup>(6)</sup>.

For convenience we divide De Morgan's treatise in three parts, the core of each part to be studied in 3.6-3.8 respectively as follows: Part 1 consists of art.1-49 [1836,305-319]. It covers a general survey of the fundamental notions and notation of the calculus of functions. Part 2 covers art.50-261 [1836,319-372]. It deals amply with functional equations in one variable, a historical review of the subject, and related topics of analysis. Part 3 covers art.262-328 [1836,372-389]. It is devoted to functional equations in two variables.

Due to the length and complexity of De Morgan's exposition, we will be very selective as to the material chosen for discussion. We will avoid marginal topics such as Abel's or Fourier's theorems [art.90,225-229,302], differential or functional differential equations [art.143,211-217,223], discontinuity or arbitrary constants [art.178-191,259-261] or the historical review of functional equations [art.245-258]. In addition, we will take for granted any techniques put forward in the work of Laplace, Babbage, Herschel and Spence to which De Morgan repeatedly referred in his work<sup>(7)</sup>.

The following lists of subjects and issues will comprise our main object of investigation.

- (1) Analogy as a means for extension.
- (2) Peacock's PEF.
- (35.1) (3) The form-matter issue.
- (4) Relations between forms as a source for invention.
- (5) Tendency towards conceptual understanding.
- (6) Classification.

By means of these issues<sup>(8)</sup>, De Morgan was led to perceive a multiplicity of links between algebra and the calculus of functions. In addition, he was facilitated in his study of the properties of the following species of functions<sup>(9)</sup>:

- (1) Inverse.
- (2) Convertible [Commutative].
- (35.2) (3) Periodic.
- (4) Derivative<sup>(10)</sup> functions:  $\varphi\alpha\varphi^{-1}$ , and
- (5) Zero-functions:  $\varphi^0$ ,  $\varphi^{\overline{0.0}}$ .

As far as functional equations are concerned we present the reader in 3.7 with De Morgan's own technique for the solution of equation

$$(35.3) \quad \alpha \phi x = \beta \phi x,$$

where  $\alpha, \beta$  known functions [art.50], with a special application to

$$(35.4) \quad \phi \alpha x = \phi x$$

[art. 102] which is an important particular case of (35.3). As we shall see, 'throughout the study of (35.1)–(35.4), De Morgan draws heavily on the work of his predecessors, most particularly on that of the members of the Analytical Society.

Like the Memoirs of the Society [2.3, (1),(3),(4)], De Morgan's obscurely written treatise is much neglected today. Moreover, this work was hardly read by his contemporaries. Some plausible reason for this may be the fact that the treatise was published as an encyclopedia article and not as a book, as his [1831] or [1842c] which were widely read. The confusing order of exposition of the material studied, as well as the ad hoc method suggested for the solution of (35.3)–(35.4), must have been also reasons for the absence of references to De Morgan's work by his contemporaries. Nowadays, apart from few hints [3.1, (1)], his [1836] remains virtually unknown. We would like to note its omission in [Dhombres 1986; see also 0.2].

In addition, there are several typographical errors, inconsistent notations, lack of parenthesis as well as many unrigorous – by our standards – procedures. But even for the standards of his own time, De Morgan's demonstrations lack the rigour of his contemporaries, such as Murphy who was occupied in his [1837] with a study of inverse and non-commutative operations<sup>(11)</sup>. One has to get used to De Morgan's idiomatic vocabulary and peculiar reasoning which accepts ambiguities and errors. As Richards pointed out in her [1987; see 3.4,(7)], all these features are part of De Morgan's historical and conceptual approach, a fact which forces us to study his work –as in 3.4– through his own eyes, as far as possible. In many instances, however, we will provide computations or elucidating explanations missing in his work, clarifying in so doing certain obscurities in notation. Any typographical errors detected will be pointed out in the endnotes.

### 3.6 De Morgan on the foundations of the calculus of functions [1836].

Babbage and Herschel had viewed the calculus of functions as a new doctrine capable to serve as a mode of discovery applicable to other branches of mathematics and science. In their published work emphasis lay in inventing notation and techniques for the solution of functional equations; an enquiry into the foundational issues of this calculus was largely passed over looking forward to applications and generalizations rather than backwards to the actual origins and foundations of the subject. Various analogies were recorded and exploited between this calculus and algebra, differential, and integral calculus, but these were more or less intuitive observations which lacked the appropriate systematic study -despite Babbage's isolated study of them in his [1817; see also 2.6-2.9].

Drawing on their notation, the new fundamental concepts introduced, the properties of these concepts (often implied in the course of a demonstration) and the diversity of the artefacts and methods used by these analysts, De Morgan set off to incorporate all these elements in a systematic and unified way. As far as the methodology for the solution of functional equations was concerned, little improvement could be effected -as we shall see- upon the methods of Laplace, Babbage and Herschel [3.7-3.8]. However, the study of foundational issues, such as the properties of inverse, convertible, periodic and other functions [see (35.2)], formed the most challenging ground for De Morgan to exert for the first time his critical reasoning and his tendency to delve into the "nature of things", deduce all possible relations between symbols, and vindicate the view -the germs of which were to be found in the work of his English (and French) predecessors [1.8,2.9]- that symbolic relations "are worth looking at as modes of invention" [art.25; see also (5),(9) below].

Somewhat<sup>1</sup> philosophically orientated, and, above all impressed by Peacock's algebra which was viewed as an extension of arithmetic -where the "anomalies" perceived in the latter were by now eliminated [art.8]-, De Morgan was initially concerned<sup>with</sup> laying the grounds for the study of the calculus of functions by con-



sidering this calculus as an extension of algebra. It was strictly and consciously through this point of view that he could delve into the nature of functional symbols and provide their crucial connections. This process occupied roughly art.2-49 of his treatise and it is our concern in this section to cite and discuss its most representative aspects.

Based upon Carnot's version of the issue of degrees of indeterminateness of mathematical symbolical language [1.8, 2.9], De Morgan illustrated the ascent from arithmetic to algebra, and from algebra to the calculus of functions. This process of gradual extension through abstraction, involved a detailed enquiry into the inverse of a direct operation. A most useful tool throughout his discursive study was his peculiar adoption of Peacock's principle (PEF) [3.4,(19)]. As we shall see, this ad hoc application of the PEF often led to fallacious reasoning, and occasional open questions<sup>(1)</sup>.

He began by defining the term "function" as "any mathematical expression considered with reference to its form, and not to the value which it derives from giving particular values to the letters contained in it" [art.2]. He called attention to the fact that "two functions which are algebraically identical are not therefore of the same form". For example, between  $(a+x)(a-x)$  and  $a^2-x^2$  an "algebraical equality exists" [meaning that the result after the computations amounts to the same function], but these two functions "are not of the same form" [art.3]<sup>(2)</sup>.

In art.5 he claimed that previously to entering upon the symbols peculiar to the calculus of functions, "it will be useful to look at the nature of the symbols which have preceded". Recalling the example mentioned above, he said that "It is usual to say that all symbols are symbols of quantity" but that is true "only in the sense analogous to that in which  $(a+x)(a-x)$  and  $a^2-x^2$  are regarded as identical in Algebra"; these two functions are not the same "as representative of operations" [art.5]. He hence delved into the notion of a symbol of quantity. In the arithmetic of "concrete quantities", 1 is the only symbol of quantity, while 2 is "the quantity resulting from the operations contained in the symbol  $1+1$ "; by contrast, in the arithmetic of "abstract numbers", 2 is "only the representation of the operation  $1+1$ ".

He concluded his enquiries in art.5 by saying that arith-

metic is "that art of reducing the result of every operation which can be performed upon different functions of 1" to the form  
(36.1)  $a+bx+cx^2+\dots$

where  $a,b,c,\dots$  are any of the "defined symbols"  $0,1,\dots,9$  and "x is the operation  $9+1$ ". As we shall see below, his mode of expressing natural numbers as functions of 1 would be slightly elaborated via the introduction of the inverse of addition which switched the emphasis from 1 to 0 [see (36.6)].

In art.6 he distinguished between arithmetic and algebra, using his so-called later "form-matter" issue of abstraction [see 3.9, (16)].

But in Algebra, considered as a consequence of abstract arithmetic, these  $\{a,b,c,\dots\}$  are not symbols of quantity, but of undefined, or, ad libitum, operations upon 1; having the same indeterminate character with respect to 1, which 1 has with respect to concrete quantity [...]. And strictly speaking, we have in the letters of Algebra our first arbitrary symbols of operation.

I take his underlined phrase to mean that as  $a,b,c$  in (36.1) stand for the defined symbols  $0,1,\dots,9$ , each standing for the result of the operations  $1+1$ ,  $1+1+1$  (0 not being defined), the same symbols  $a,b,c,\dots$  in algebra stand for the result of arbitrary operations exerted upon the natural numbers [see also 3.9].

Now, the question comes as to the link between algebra and the calculus of functions. Since the symbols of the latter are not yet introduced, an essential difference between these two branches is to be perceived by appealing to the kind of problems tackled in the realms of each one. Algebra deals primarily with reducing problems to equations in  $a,b,c,\dots$  and in the solution of such equations via the determination of  $a,b,c$ . It deals, moreover, with another type of problem described in various ways; we choose the formulation given in the footnote of art.6:

(36.2) "Given a function, and the general form of another, required (if possible) a specific form, such that it and the given function shall be reducible to identity by the same operations".

It is somehow bizarre that, despite his claim to present an introductory work of an elementary level [art.1; 3.5], De Morgan omitted any examples to illustrate (36.2). He might have had in

mind, among other cases, the process of solving a differential equation by means of variation of constants. In art.7 he stated the "general problem" in the calculus of functions as follows:

(36.3) "Given any number of quantities and any conditions which are to exist between functions of these quantities, required the forms of the functions which will satisfy these conditions".

A case of (36.3) is the following problem:

(36.4) "required that operation which being performed upon  $x$ , and again upon the result shall reproduce  $x$ ".

Evidently, a particular answer to (36.4) is the function  $a-x$ , a constant [art. 7].

Had he had included a wider variety of examples his conclusion, after a brief discussion of (36.3) in art.7, might have been based on more persuasive arguments. He claimed that "as Algebra gives results of greater generality than Arithmetic, so the consideration of the form which will satisfy certain conditions [i.e. problem (36.3)] gives results of greater generality than those of common Algebra" [art.7].

Despite the insufficiency of his preceding discussion, De Morgan felt confident enough to state his first general remark, which forms a sort of motto throughout the treatise:

Every hint, therefore, which can be gained from the connection of Arithmetic and Algebra, may teach us what to look for, and what to avoid, in the transition from Algebra to the Calculus of Functions. And, at first, we must observe, that as the indeterminate character of the algebraical symbols kept out of view the limitations necessary in Arithmetic, so the corresponding character of the symbols to which we shall come, as compared with the definite forms of Algebra, may equally make us lose sight of limitations necessary in the latter science. And as each neglect of the necessary conditions of Arithmetic made algebraical operations more extensive than those of the former science, the same may happen here<sup>(3)</sup>.

Thus, as Peacock regarded arithmetic as the "science of suggestion" in his algebra [see 3.4, (18)], De Morgan was to consider algebra as a science of suggestion in the calculus of functions.

De Morgan discussed the limitations of pure Arithmetic, as in the case of  $a > b$  where  $a$  must be greater than  $b$ , and further the necessity for a method of interpretation of  $\sqrt{-1}$ . He referred

to Peacock [1830, 1833] for an answer to such difficulties. He displayed the usual algebraic forms, such as  $(\pm x)^{\pm y}$  etc and the logarithms and sines of such forms pointing out that certain of them "may be found to satisfy conditions which common algebraical reasoning would lead us to conclude it impossible to satisfy" [art. 8]. Postponing concrete examples until many articles later, he claimed optimistically that "the expressions not yet considered, and discontinuous functions in particular, may satisfy an algebraically impossible condition [stressing above "in Algebra, as it now stands"], just as the negative quantity of Algebra satisfies an arithmetically impossible condition" [art. 8].

The notion of discontinuity was to be further discussed particularly in connection with problems, which, like Babbage's (27.41), were regarded as impossible in the realms of the functional calculus [see, for example, art.72 on (27.41)]. This matter will not be studied in this thesis. However, few instances are worthy to be recorded as representative samples of De Morgan's reasoning which often led to nonsensical conclusions. First of all he remarked that we cannot arrange algebraic forms forms, such as  $a$ ,  $a+bx$ ,  $\log x$  in order of magnitude or in any order "dictated by any other notion which appears necessary to them". Judging from the "so many and distinct varieties" of forms, "we should feel inclined to say that the different classes of functions are rather the objects of different sciences than of different branches of the same science; or, at least, are the objects of branches as different as those which treat of possible and impossible quantities in Algebra" [art.9].

He went to say in art.10 that despite the imperfect considerations connected with functions, we have the notion of "discontinuous functions" which were suggested in the realms of the integral calculus "so like what we may conceive to have preceded continuous variation in algebraical quantities, that we do not despair of seeing laws of arrangement established among forms". In art.73 he stated the following "theorem":

(36.5) "a discontinuous function of a discontinuous function may be a continuous function".

This statement, as involving the word "may" and as illustrated by means of an example, is a typical De Morgan-theorem. Aware, most probably, of its lack of clarity, he drew the following argument from algebra in order to establish (36.5):

If  $\neg(\neg a) = +a$ , because the negation of a negation is an affirmation, surely the discontinuance of discontinuity has a right to be considered as the commencement of continuity<sup>(4)</sup>.

or

By means of this analogy, which in fact is but an unthodox application of Peacock's PEF, De Morgan found a heuristic way so as not to exclude discontinuity from his system.

We proceed now to a discussion of his study of extensions, as necessary for the definition of inverse operations, and of the description of the formal transition from algebra to the calculi of functions and operations. "Every new step in the science of symbols", he claimed in art.11, "has been accompanied by a correlative [...] inverse step". This requires "new modifications of symbols", in other words, some "previous extension of the notions necessary to the performance of the direct operation". For example, in arithmetic 2 is reconverted into 1 only by the "invention of new symbols", such as  $\frac{1}{1+1}$ , and "an extension of the notion of whole numbers" [art.11; see also Woodhouse 1803, 58-63, 160-62]. In the next two articles he put the foundations for the new symbols and terms introduced further below in connection with the inverse of a function.

He gave in succession the following distinct "sets" or "series" of quantities 1, 2, 3, ... and their inverses according to the number 0, 1 or 2 respectively that all numbers can be considered as functions of, namely

$$(36.6) \quad \dots, -2, -1, 0, +1, +2, \dots$$

$$(36.7) \quad \dots, \frac{1}{3}, \frac{1}{2}, 1, 2, 3, \dots$$

and

$$(36.8) \quad \dots 2^{1/\lambda 4}, 2^{1/\lambda 3}, (2^{1/\lambda 2} = 2^{\lambda 2}), 2^{\lambda 3}, 2^{\lambda 4}, \dots$$

where " $\lambda x$ " stands for " $\log_2 x$ " [arts.12-13]. In the case of (36.6), "by extension of our language" we need say that the natural numbers are "functions of 0"; "for since it is the definition of an inverse function that the direct operation performed upon it produces the subject of the function" and since  $-a+a$  is always = 0. In the case (36.7) all numbers are considered as functions of 1. Improving over his earlier statements in art.5 [see (36.1)] he decided to consider 0 as the symbol first operated on, and +1 as the modus operandi by which x becomes

$x+1$ ". The series (36.8) was evidently the outcome of the question "according to what method of derivation may all numbers be considered as functions of 2". He concluded art. 13 by saying:

We might propose an infinite number of other ways in which the series of natural numbers might be considered as functions of any one among them; but the above will be sufficient to illustrate this point, that each set gives a distinct set of inverses, and that we have therefore an unlimited number of methods by which we may represent inverse functions of different species, choosing of course those methods which present any analogy to the manner of deriving the inverse in question.

De Morgan applied the results of his inquiries for a first definition of  $\varphi^{-1}x$  without yet any reference to the uniqueness of the inverse of a function. He first introduced the "functional symbols"  $F, f, \varphi, \alpha, \beta, \dots$ ;  $\varphi x$  or  $\varphi_x$  stands for "some operation to be performed on  $x$ " and  $\varphi\psi x$  means "that the operation  $\psi$  having been performed upon  $x$ , the operation  $\varphi$  is to be performed upon the result". By means of simple examples, such as  $\varphi x = a^x$  or  $\psi x = x^2$ , he determined the compounds  $\varphi\varphi x$ ,  $\varphi\psi x$  etc [art.14]. But prior to that he summarized in art.14 his earlier study in order to explain the nature of the functional symbols by means of the gradual process of abstraction from the algebraic ones.

As in Arithmetic the notion of concrete quantity was generalized into that of abstract number, and in Algebra definite abstract number was abandoned for symbols of general number, thus enabling us to express any definite operation performed with an indefinite number, so in the Calculus of Functions the definite character of the operation is dropped, and indefinite symbols of operation are used, which may be either unknown, to be found by means of conditions, or arbitrary, to be assigned at pleasure, or conventionally definite, (like  $\pi$  and  $e$  in Algebra) to denote particular operations which frequently occur.

In art.15 he abbreviated  $\varphi\varphi x$  into  $\varphi^2x$ , accordingly expressing the problem (36.4) <sup>by</sup> the functional equation

$$(36.9) \quad \varphi^2x = x.$$

In art.16 he treated the notion of the inverse of a function  $\varphi x$ . Letting  $\varphi x = y$ , he claimed that  $x$  is also a function of  $y$ , hence  $x = \psi y$ . Let  $y = (x-1)^2$ . Solving for  $x$  and changing  $y$  into  $x$  we have

that one form of  $\psi x$  is  $1+\sqrt{x}$ . He called this function "an algebraical" inverse of  $(x-1)^2$ . On similar lines, if  $\phi x=ax$ , then  $\phi^3 x=a^3 x$ , or, the algebraical inverse of  $\phi^3 x$  is  $x/a^3$  [art.16].

In order to introduce a suitable notation he drew on (36.6)-(36.7), reasoning as follows: In the process of deducing the inverse of  $\phi^3 x$  above, he observed "that there is no reference to the accumulated operations by which  $a^3 x$  was supposed to have been obtained; the inverse is the same, whether we consider  $a^3 x$  as our primitive function, or [...] as the third function of  $ax$  [that is, regarding  $ax$  as the primitive]". Reassuming the series (36.6), he noticed that, since "the modus operandi has no reference to the number of steps by which that predecessor was formed from 0" we can choose  $-1, -2, \dots$  "as convenient indices of the algebraical inverses of  $\phi x, \phi^2 x, \dots$ ". [art.16]. Thus,

$$(36.10) \quad \phi x=y \longrightarrow x=\phi^{-1}y \quad \text{and} \quad \phi\phi^{-1}x=\phi^0x=x$$

or, inductively,

$$(36.11) \quad \phi^n \phi^{-n} x = x.$$

But another question arose: if  $\phi^2 x = \psi x$ , "what is the connection of the operations  $\phi$  and  $\psi$ "? Now, by analogy with the series (36.7) he concluded that "since  $\psi$  is  $\phi^2$ ,  $\phi$  shall be  $\psi^{1/2}$ ", in other words, fractional indices serve to denote what De Morgan called as "functional inverses". In general " $\psi^{m/n}$  indicates that operation, which being performed  $n$  times, shall produce  $\psi^m$ ", or,  $\psi^{m/n}$  is the  $n^{\text{th}}$  functional inverse of  $\psi^m$  [art. 16].

De Morgan went on to compare the nature of symbols of the calculus of differences with those of the calculus of functions on lines analogous to those followed in his comparison of the latter calculus with algebra. Symbols  $\Delta, d, \Sigma, \dots$ , he claimed, "are certainly not symbols of quantity but of operation, and yet not strictly functional symbols, in the sense in which we have to consider functions" [art. 18]. Contrasts and similarities between these two calculi were observed through the peculiar function of  $\Delta$  when applied to a function  $\phi x$ , by means of its definition

$$(36.12) \quad \Delta \phi x = \phi(x+h) - \phi x.$$

He first regarded  $\Delta \phi x$  with respect to  $\phi x$  and consequently as a function of  $x$ .

In the first case we notice a substantial difference between the compounds  $\Delta \phi x$  and  $\psi \phi x$  as the result in the latter compound is independent of the form  $\phi$ . If  $\psi x = x + x^2$ , then  $\psi \phi x = \phi x + (\phi x)^2$ ,  $\psi e^x = e^x + (e^x)^2$  etc. On the contrary, due to (36.12), " $\Delta \sin x$  does

not perform the same operation on  $\sin x$  which  $\Delta x^2$  does on  $x$ ". Thus, despite the simplicity of the definition (36.12) "we have ascended one step higher than the calculus in the scale of generalization" by which arithmetic became -through gradual abstraction- the calculus of functions. More explicitly,  $\Delta$  in (36.12) stands to  $\phi$  "in the same relation as  $\phi$  to  $x$ , or as  $x$  to abstract, or as abstract to concrete quantity" [art. 18].

Other kind of analogies were observed in the second case where  $\Delta\phi x$ , or  $\Delta u_x$ , was considered with respect to  $x$ . De Morgan regarded  $+\Delta u_x$  in a way analogous to  $+1$  in (36.6) claiming that:

$u_x + \Delta u_x$  has a connection with  $(1+\Delta)u_x$  of the same kind as  $a+ba$  with  $(1+b)a$ ;  $a$  being considered as the result of an operation on 1, and  $(1+b)a$  being the same operation on  $a$  which  $1+b$  is on 1. Still more direct is the analogy of  $u_x + \Delta u_x$  and  $(1+\Delta)u_x$  with 7 pebbles + 3 pebbles and  $(7+3)$  pebbles.

Considered in this light, he added, the calculus of differences is a "refinement", an "extension" of the method of figures, that is, of "the only demonstrable root of human knowledge"<sup>(5)</sup>.

In art.19 he claimed to have introduced above in a simple manner the "common method" of separation of symbols, mentioning by this opportunity Lagrange's theorem. He commented briefly upon Laplace's method of generating functions<sup>(6)</sup> and in art.22 he remarked that the theorems of the calculus of operations do not require an extension of the sign  $+$ , as in the passage from  $u_x + \Delta u_x$  to  $(1+\Delta)u_x$ , whereas the case is not that simple if we consider  $(\phi+\psi)x$  to mean  $\phi x + \psi x$ . Thus, algebra gives place to a new inquiry, that is "how to invent notation, so that the rules which follow from it shall be simple and analogous to those which have preceded" [art. 22].

According to this point of view,

$$(36.13) \quad (\phi+\psi)x = \phi x + \psi x$$

is useless as a definition, unless

$$(36.14) \quad \phi\psi x = \psi\phi x$$

and

$$(36.15) \quad (\phi+\psi)^2 x = (\phi^2 + 2\phi\psi + \psi^2)x,$$

for then we have an analogy with the calculus of operations [art. 22]. De Morgan observed that in most cases "analysis does not yet furnish" the means of interpreting  $+$  and  $-$  so that (36.15) holds true. For example "we have not the most remote means" of inter-



preting + in  $(\log+\sin)x$  [art. 23]. However, he presented few specific cases in which both (36.14) and (36.15) are satisfied.

He let two functions  $\phi$  and  $\psi$  be of the form

$$(36.16) \quad a_i(x+m) = m \quad ,$$

for  $i=1,2$  respectively.  $a_i$  and  $m$  constants, and proved that both (36.14)-(36.15) are satisfied if we define

$$(36.17) \quad (\phi+\psi)x = \phi x + \psi x + m \quad (7).$$

If  $i=1,2,\dots,n$  in (36.16), then separation of symbols can be carried out if we assign to + the following meaning:

$$(36.18) \quad (p_1\phi_1 + \dots + p_n\phi_n)x = p_1\phi_1x + \dots + p_n\phi_nx + (p_1 + \dots + p_n - 1)m$$

where  $p_i$  negative or positive integral numbers [art.22] (8).

These last enquiries, held De Morgan, "open a wide field of speculation upon the possible limits of analysis". Again he looked back at algebra "to pick out the leading features of its notation" observing that the basic operations +, - and  $x$ ,  $\div$  are connected by the theory of logarithms [art.24]. From this angle he posed an analogous question in the calculus of functions, that is, granting any given forms of "operation"  $\phi x, x, \dots$  as "fundamental", what must these operations be so that certain modes of connection may exist between them [art.25]. His epistemological position is encapsulated in the following suggestion; that

as every extension of notation has, by laying down new operations as fundamental, assumed the possession of certain new powers, [...], the relations of symbols are worth looking at as modes of invention (9).

In art.26. he held that "The student who endeavours to apply principles must immediately become an inventor of notation, and much of his success must depend upon whether he does it well or not". A few warnings were included, one against an abuse of abbreviation and the other in connection with symmetry; the latter is "indispensable in all complicated processes". Moreover "an unsymmetrical result from symmetrical data is almost a proof of error, while the contrary is a presumption of truth" [art. 26] (10).

Analogy from algebra was to be repeatedly stressed with precaution. The case of zero as a functional index,  $\phi^0$ , was to have "effects analogous to those produced by making  $x=0$  in Algebra" [art.27]. While in art. 16 [see (35.12)]  $\phi^0x$  was defined in analogy with the series in (36.5), now we come across a more conceptual definition drawn in analogy from algebra: "As the zero

of quantity is that which is conceived neither to increase nor diminish quantity, so we shall term the zero of operation that which is conceived to make no alteration in the subject of a function, and which is denoted by  $\varphi^0$ " [art. 27].

Providing without explanation the solution of the equation

$$(36.19) \quad \varphi(ax) = a^2 \varphi x,$$

with ' $a$ ' <sup>$\alpha$</sup>  constant, in the general form

$$(36.20) \quad " \varphi x = x^2 \times \text{any } f \text{ of } \left( \cos 2\pi \frac{\log x}{\log a} \right) "$$

he added that certain limitations are to be posed to the word "any" as "derived from our future consideration of inverse functions" [art. 29] <sup>(11)</sup>.

In art. 32 De Morgan assumed the function

$$(36.21) \quad y = (x^2 - 1)^3 = \psi x. \quad \text{to}$$

If we solve (36.21) for  $x$  we have due<sub>A</sub> the square and cubic roots six different values, and hence six distinct algebraical inverses of  $\psi x$ . One of them is

$$(36.22) \quad (1 + \sqrt[3]{x})^{1/2},$$

which was called the "arithmetical" inverse of  $\psi x$  distinguished from the others, "non-arithmetical" ones, as it was the unique value in his example to satisfy both

$$(36.23) \quad \psi \psi^{-1} x = x$$

and

$$(36.24) \quad \psi^{-1} \psi x = x.$$

Thus,  $\psi^{-1} x$  denoted the unique inverse <sup>(12)</sup> of a function  $\psi x$  defined by the above two properties; the inverses which satisfied only (36.23) were denoted by  $\psi_{-1}$  [art. 32-33].

The conception of  $\psi^{-1} x$  by a simultaneous consideration of (36.23)-(36.24) gave rise to the notion of "convertible" functions  $\varphi, \psi$  according to the rule

$$(36.25) \quad \varphi \psi x = \psi \varphi x.$$

Thus  $\psi^{-1} x$  is convertible with  $\psi x$  while  $\psi_{-1} x$  are inconvertible with it. Combining further this new notion with his earlier enquiries he presented two "theorems". The first amounts to

$$(36.26) \quad \text{"Every inverse is convertible with one or other of the forms of the direct function."}$$

This obscure statement was illustrated solely by examples. For example,  $1 + \sqrt{x}$ ,  $1 - \sqrt{x}$  are not both convertible with the direct function  $(x-1)^2$ ; the first is convertible with  $(x-1)^2$  and the

second with  $(1-x)^2$  [art. 33]. The second theorem concerned another property peculiar to inconvertible inverses and was proved in a most confusing, unrigorous way [art. 34] <sup>(13)</sup>.

He went on to introduce a "periodic" function  $\phi$  of order  $m$  as one which satisfies the equation

$$(36.27) \quad \phi^m x = x$$

[art.35]. This concept divided all functions in two categories, periodic and non-periodic. He inquired next into the limit of  $\phi^n x$  in the case when  $\phi x$  is not periodic, proving that

$$(36.28) \quad \text{"The limit (if there be one) of } \phi^n x \text{ made by increasing } n \text{ without limit is a solution of the equation } \phi x = x\text{"}$$

Assuming that  $n$  is so great that both  $\phi^n x$  and  $\phi^{n+1} x$  differ from the limit  $L$  by the quantities  $l$ ,  $l_1$  respectively which "may be made as small as we please", he obtained  $\phi(L)=L$  [art.37]. In art.39-42 he illustrated applications of (36.28) in problems of approximation in the realms of algebra and of the differential calculus. In his [1838], written shortly after his [1836], he referred to this theorem offering the following example from the calculus of functions. Let

$$(36.29) \quad \phi x = a(x+m)-m,$$

then  $\phi x = a(x+m)-m$ , which equals  $-m$  when  $a < 1$ ; but  $-m$  is the root of  $\phi x = x$  [1838, 187] <sup>(14)</sup>.

Thus De Morgan laid the first fundamental concepts of the calculus of functions focusing in and around the intricate matter of the inverse function  $\phi^{-1}x$ . In art.44-49 he built the preliminary notions and theorems necessary for the introduction of his basic method given for the solution of functional equations in art.50. The two key features of this method concerned the notion of the successive function  $\phi^n x$  and the "derivative" of  $\phi x, \phi \phi^{-1}x$ .

On lines similar to those followed by Herschel in his early enquires [2.4], De Morgan assumed that

$$(36.30) \quad \phi^n x = \Phi(n, x)$$

[art.44]. Based upon two examples given by Herschel in [1813] he illustrated Laplace's method for the determination of successive functions by reduction to finite difference equations. The second example concerning the determination of  $\phi^n x$  when  $\phi x = 2x^2 - 1$  [art. 46] was identically provided in 2.4 [see (24.14)]. He consequently regarded  $n$  to be negative or fractional in the expression (24.14) which gives  $\phi^n x$  [art. 47-48] stressing that:

When we talk of the most general form of a function, we must be

considered as speaking conjecturally, for we have no test by which to try the generality of any solution whatsoever.

Finally in art.49 he studied in depth Babbage's transform (26.5), claiming that "Much of what has been done in the Calculus of Functions depends upon a class of forms of which we now proceed"; i.e. upon the form

$$(36.31) \quad \varphi\alpha\varphi^{-1}x,$$

which he called "a derivative"<sup>(15)</sup> of the function  $\alpha x$ . Such forms are common in algebra. For, let  $\varphi x = x^2$ ,  $\alpha x = x + c$ ; then  $\sqrt{x^2 + c} = \varphi^{-1}\alpha\varphi x$ . Among the theorems proved in art.49 we note the following:

(36.32) Derivatives of derivatives of  $\alpha x$  are derivatives of  $\alpha x$ .

(36.33) If  $\mu$  and  $\nu$  be corresponding derivative forms of  $\alpha$  and  $\beta$ , then  $\mu\nu$  is a derivative of  $\alpha\beta$ .

(36.34) If  $\sigma$  and  $\beta$  are inverse to one another, or convertible with each other, then their corresponding derivatives have the same properties.

(36.35) The successive functions of the derivatives of  $\alpha x$  are the corresponding derivatives of  $\alpha x$ .  
~~the successive functions of~~

(36.36) The derivatives of a periodic function are periodic functions of the same order.

All these theorems were proved directly from the definition (36.21); (36.35) and (36.36) were attributed to Babbage [art.49,fn; see 2.6 after (26.5)]. We will conclude De Morgan's foundational study with his proof of theorem (36.33). Let

$$(36.37) \quad \mu x = \varphi\alpha\varphi^{-1}x, \quad \nu x = \varphi\beta\varphi^{-1}x.$$

Then,  $\mu\nu x = \varphi\alpha\varphi^{-1}\varphi\beta\varphi^{-1}x$ . But, according to (36.24),  $\varphi^{-1}\varphi x = x$ , therefore it follows that  $\mu\nu x = \varphi\alpha\beta\varphi^{-1}x$ , or  $\mu\nu$  is a derivative of  $\alpha\beta$ . We notice that as in other cases in art.49, he simplified the procedure by taking  $\varphi$  to be the arbitrary function in both  $\mu$  and  $\nu$  as given by (36.37). The first basic problem to be studied was to see whether given two functions  $\alpha(x)$  and  $\beta(x)$  there exists an  $\varphi$  so that  $\beta(x)$  is derived from  $\alpha(x)$  by means of it [art.50; 3.7].

3.7 De Morgan on functional equations in one variable [1836]; connections between the calculus of functions and algebra.

On the whole, De Morgan's study of functional equations in one variable, including inquiries and comments on related topics of the integral calculus and on the history of the calculus of functions, covers art.50-261, in other words the largest part of

his treatise [see 3.5. (1)]. Focusing on functional equations only, we notice that what prevails is a rather confusing combination of Babbage's techniques as in [1815], and De Morgan's own method given below. A representative instance of this combination is the solution of equation (37.14). To a lesser extent, we notice an analogous blend of Laplace's original method of reduction to finite difference equations with De Morgan's suggested method -as in the case of equation (37.54) [see (37.70)-(37.79)].

Having dealt with Babbage's and Herschel's functional methods in 2.4-2.8 -including De Morgan's comments on them [see 3.5.(7)]- we focus here on De Morgan's peculiar adaptation of their techniques, providing an account of his method for the solution of the equations (35.3) and (35.4). Due to the length and complexity of De Morgan's exposition we will confine primarily to these two equations which form the core of his study. However, what mostly attracts our attention in studying this part of his work, is his constant tendency to draw potential links and analogies between algebra and his subject under study. We will thus first display some representative examples, outcomes of a combination of his earlier foundational study of periodic, convertible and other functions and of his new techniques proposed, concluding with the technicalities of his method.

This slightly unorthodox order of exposition aims in emphasizing his mode of reasoning rather, than the actual techniques involved. However, as we will briefly refer in so doing to his original method, we present first a brief sketch of the solution of equation (35.4) as given by De Morgan in his textbook on the calculus [1842c, 737].

The only mode of solving  $\xi Bx = \xi x$  which has yet been given, when  $Bx$  is not repeating ( $\therefore$  non-periodic), depends upon the expression of  $B^n x$  as a function of  $n$  and  $x$ . Let  $B^n x = \chi(n, x)$ . This function  $\chi$  is not altered by a simultaneous change of  $n$  into  $n-1$ , and  $x$  into  $Bx$ : if, then,  $n = Bx$  be the solution of  $\text{const.} = \chi(x, n)$ , the function  $Bx$  is a solution  $B Bx = Bx - 1$ . Consequently,  $\cos 2n Bx$  is one solution of  $\xi Bx = \xi x$ , and  $\theta \cos 2n Bx$  is a very general solution, where  $\theta$  is any function which does not contain inverse trigonometrical operations<sup>(1)</sup>.

Thus, if we have for solution equation (35.4), or,

$$(37.1) \quad \varphi \alpha x = \varphi x ,$$

where  $\alpha$  a known function, it suffices to take

$$(37.2) \quad \alpha^n x = \chi(n, x) = c,$$

where  $\alpha$  non-periodic and  $c$  an arbitrary constant, and determine via (37.2) the value of  $n$  as a function  $A$  of  $x^{(2)}$ . This function  $Ax$  (or  $Bx$  in the above quotation) is called by De Morgan the "exponential inverse" of  $\alpha x$  [art. 102, 104]. Equation (37.1) is in fact a particular case of a more general equation (35.3), or,

$$(37.3) \quad \varphi \alpha x = \beta \varphi x \quad \text{or} \quad \alpha x = \varphi^{-1} \beta \varphi x$$

tackled before (37.1) in art. 50. Equation (37.3) expresses the following problem which forms one of his main concerns:

"If functions  $\alpha x$  and  $\beta x$  be given, neither of which are  
(37.4) periodic, required to find whether  $\beta$  be a derivative of  $\alpha$ , and, if so, what derivative".

Postponing explanatory remarks for a while, let us see De Morgan's principal concerns in his study of various categories of functions. One of the subjects that deeply interested him was the investigation of "convertible" (i.e. commutative) functions, the definition of which came as an outcome of his study of inverse functions [see (36.23)–(36.25)]. If  $\alpha$  is a given function, then the solution of the equation

$$(47.5) \quad \varphi \alpha x = \alpha \varphi x,$$

according to the method of problem (37.4), will provide us with a "large number" of functions which are convertible with  $\alpha x$  [art. 114–116; see also art. 77 and (37.69) below].

Later he resumed his former enquiries, claiming in art. 144:

Unless we can combine our various cases by some method of connection, there is little use in considering them at present. The nature of compound functions is of first importance. What connection exists between  $\varphi \psi x$  and  $\psi \varphi x$ ? Till something more than we shall be able to exhibit is made known upon this point, the Calculus of Functions will be in much the same position as Arithmetic without the knowledge of the relation which exists between  $b$  times  $a$  and  $a$  times  $b^{(3)}$ .

Calling  $\psi \varphi$  the "converse" of the compound  $\varphi \psi$ , "a term which must be carefully distinguished from the inverse" [art. 144], De Morgan went on to investigate the following problem:

"Given the results of a compound operation of the  
(37.6) second order, and of its converse, required the operations themselves".

Problem (37.6) amounts, evidently, to the solution of the system of the equations

$$(37.8) \quad \phi\psi x = \alpha x \quad , \quad \psi\phi x = \beta x \quad ,$$

where  $\alpha$ ,  $\beta$  are given. If we replace  $x$  by  $\phi^{-1}x$  in the former, we have according to the latter

$$(37.9) \quad \alpha x = \phi\beta\phi^{-1}x,$$

and a symmetrical result for  $\beta$  in terms of  $\psi$ . Thus, the given problem (37.6) is once more reduced to his initial problem (37.4) [art. 145; see also art. 146-150].

Another type of problem tackled, closely related to the study of convertible functions, was the solution of (26.7), or,

$$(37.10) \quad \phi^n x = \alpha x.$$

First he distinguished sharply between (37.10) and

$$(37.11) \quad \phi^n x = x,$$

or (26.4), arguing that the range of the solutions of the former equation is "much less extensive" than those of the latter [art.152]. He further remarked that since

$$(37.12) \quad \phi^n(\phi x) = \phi(\phi^n x),$$

it follows that

$$(37.13) \quad \phi\alpha x = \alpha\phi x ;$$

that is,  $\phi$  and  $\alpha$  in (37.10) must be convertible. He added: "Now when  $\alpha x = x$ , it is convertible with every function of  $x$ ; but when  $\alpha x$  is any other function of  $x$ , however simple, it is convertible only with a very inconsiderable variety of forms, and those not all assignable at present" [art. 152].

He thus expressed, as in the case of convertible functions in art.144, his wish to have any existing gaps filled in the near future. De Morgan's open questions are interesting not so much as characteristic of his period but as characteristic samples of his own peculiar way of viewing mathematics. In art. 152 he stated:

Now though  $\phi^n x = x$  is a case of  $\phi^n x = \alpha x$ , we cannot for any useful purpose consider them together, until we have some idea how to fill up the void, if we may so speak, between  $\alpha x$  and  $x$ , so as to assign functions continually approaching nearer and nearer to  $x$  in the extensiveness of the forms with which they are convertible.

To solve (37.10) in the form

$$(37.14) \quad \psi^n x = \alpha x \quad ,$$

De Morgan put, after Babbage,

$$(37.15) \quad \psi x = \phi\phi^{-1}x \quad .$$

Thus, the solution of (37.14) was reduced to the determination of  $\varphi$  from

$$(37.16) \quad \varphi f^n x = \alpha \varphi x,$$

where  $\alpha$  and  $f$  are known, according to the problem (37.4) -or (37.3). As in problem (37.1), he let  $A$  and  $F$  be the exponential inverses of  $\alpha$  and  $f$ . Isolating his results we have that

$$(37.17) \quad \psi x = A^{-1}(Ax - 1/n)$$



and

$$(37.18) \quad \psi^n x = FfF^{-1}x = x-1$$

[art.156; see for details (37.58), (37.65)]. Thus, "The whole question relative to the solution of  $\psi^n x = \alpha x$  is thus reduced to the solution of  $\psi^n x = x-1$ " [art.157]. Further below in art.158 we read:

The solution of  $\psi^n x = \alpha x$ , in all its generality, is a fundamental inquiry in the subject of the present Treatise: nor can more essential progress be looked for without it, than in common Algebra without the complete solution of  $x^n=1$ . Our own suspicion is, that the solutions are infinite in number, but embraced under a finite number of forms; but we hope the subject will be further considered<sup>(4)</sup>.

With the exception of his method for the solution of (37.1) which does not depend upon the assumption of a particular solution, De Morgan's study of functional equations—drawing heavily on his English predecessors—achieves very little beyond elaborating Babbage's and Herschel's methods and concerns. Apparently, he felt that the value of his study lay principally in his illustration of the links between the calculus of algebra and functions and in his open questions which might found the basis for future researchers [see also (4) above].

Certain of De Morgan's interests, as in the case of convertible functions, coincide with Murphy's in [1837; (3) above]. But, the calculus of functions is far more extensive than that of operations, and, while the connection between  $\varphi\psi$  and  $\psi\varphi$  can be investigated for  $\varphi$ ,  $\psi$  specific distributive operations [as in (33.42)–(33.46)], in the calculus of functions the problem is more complicated. De Morgan was close to Murphy's law (33.37), or  $(\varphi\psi)^{-1} = \psi^{-1}\varphi^{-1}$ , when he called  $\psi\varphi$  the "converse" of  $\varphi\psi$  [as in art.144] but surprisingly he omitted to formulate it,  probably out of lack of necessity in his applications. His approach, as based upon the issues listed in (35.1), would prove much more  except for inconvertible inverses [see (69.27)],



fruitful in the realms of traditional logic [6.4-6.9].

Before we proceed to a discussion of his method as primarily based upon (37.2), let us see few more instances which encapsulate the spirit of his investigations. Having tackled the problem (37.5) in art. 115, followed by the solution of the equation

$$(37.19) \quad \varphi \alpha x = \varphi x - 1$$

in art.119, De Morgan, following roughly Babbage's order of exposition in [1815; see (25.5)-(25.29)], considers the next step which amounts to finding a function which shall not change when any of  $\alpha x$ ,  $\beta x$ ,  $\gamma x$ , is substituted for  $x$  [see (25.29)]. "We must content ourselves here", he claims, "with showing that we want a process analogous to one of ordinary Algebra, which, if it had not been found, would have left that science as full of difficulties as is the Calculus of Functions at present" [art.120].

Considering the problem

$$(37.20) \quad \varphi(x+1) = \varphi(x^2) = \varphi(x),$$

he deduced from it

$$(37.21) \quad \varphi((x+1)^2+1) = \varphi((x^2+1)^2+1) = \varphi x,$$

and so on, led, by substituting  $\alpha x$  and  $\beta x$  for  $x+1$  and  $x^2$  respectively to a more general equation, which, if it could be solved, "it would become the most unfailing instrument" for such problems [art.120]. In art.121 he considered it necessary to comment as follows:

In Algebra, the difficulty of resolving such an equation as  $\alpha\beta x = \gamma x$  is overcome by developing  $\alpha\beta$  and  $\gamma$  in powers of  $x$ , that is, changing all functions into functions of one general form. But in the Calculus of Functions there is yet no theorem by which any function may be expressed in terms of the successive functions of another function, or of functions in any other way related to it<sup>(5)</sup>.

In art.122 he claimed that "The only cases in which the preceding question can now be solved are where the given functions are among the successive functions of some other function". Postponing Babbage's method for the general problem mentioned in art.120 [see (25.29) a particular case of which is (37.20)] for art. 161-163, he discussed numerous problems, only few of them directly related to that. First of all, he commented upon transcendentals which arise from inverse operations -including historical references- in art.123-124. He claimed that the calculus of functions is essentially "an inverse calculus" [art.125]

and next he gave few instances from the direct part of this calculus, e.g. given  $\beta x$  and  $\alpha x$  how we can express  $\beta^n x$  in terms of  $\alpha^n x$ , through an example [art.126]. Another instance concerned the successive functions of periodic functions accompanied by the remark: "The periodic functions have hitherto appeared to stand in some sort of relation to others, similar to that of rational and irrational expressions in Algebra" [art.129].

In art.131 he remarked that though "There is no general method of finding a particular solution of a functional equation [...] there is an indefinite number of ways in which the solution of one equation may be made to depend upon that of another, either with or without a particular solution". Omitting to mention Babbage's name, he took the linear equation

$$(37.22) \quad a_x \phi \alpha x + b_x \phi \beta x + \dots + m_x = 0,$$

where  $a_x, b_x, \dots, m_x$  are given functions of  $x$ , noticing that by means of a particular solution, (37.22) can be reduced to an equation deprived of its last term [see (22.25)-(22.26); art.135]. He stressed that if  $\alpha$  <sup>and</sup>  $\beta$  are successive functions of a periodic function, a particular solution may be obtained [art.136; see claims in art.120-121 mentioned above].

With the instances recorded so far we have some evidence of De Morgan's fragmentary and discursive exposition, often interrupted by historical references, educational concerns, foundational problems and comments on the analogies he perceived between algebra and the calculus of functions. A typical case of the disorder of his presentation is his discussion of the problem put forward in art.120, a case of which is the solution of (37.20). Despite the cross-references -which are not always supplied- the reader is confronted between art.120 and art.162, where the general method (25.29) is displayed, with numerous irrelevant problems. Moreover, art.120 is not referred to in art.162 and the intermediate examples are far from lucidly exposed<sup>(6)</sup>.

Let us now focus on the technicalities involved in his method. In art.50 problem (37.4) is reduced to the solution of

$$(37.23) \quad \beta x = \phi \alpha \phi^{-1} x.$$

If we put  $\phi x$  instead of  $x$  in (37.23) we arrive at the equation

$$(37.24) \quad \beta \phi x = \phi \alpha x.$$

Equation (37.24) was tackled analytically by means of Taylor's theorem in [Collins 1831,345]. De Morgan had not seen this investigation but he <sup>was</sup> aware of the "theorem" (37.4) through

another reference of Collins [art.50,fn]('7). Motivated by Collins he set off to deal with (37.24). His procedure was based upon the assumption that if  $\alpha x$ ,  $\beta y$ , are non-periodic functions, we can always determine their successive values in the form

$$(37.25) \quad \alpha^n x = A(n, x) \quad , \quad \beta^n y = B(n, y).$$

The technique put forward consequently consisted of eliminating  $n$  [except where it is an index of a function] between the two equations in (37.25)('8). Let the result of this elimination be

$$(37.26) \quad F(x, y, \alpha^n x, \beta^n y) = 0.$$

He then assumed a relation  $y = \phi x$  to exist between  $x, y$  which results out of replacing  $\alpha^n x$  and  $\beta^n y$  in (37.26) by arbitrary constants  $C, C'$  respectively; that is,  $y = \phi x$  is defined by

$$(37.27) \quad F(x, y, C, C') = 0.$$

It remained to be proved that such an  $\phi$  satisfies (37.24), for then problem (37.4) is solved.

From (37.25) we have readily that

$$(37.28) \quad \alpha^{n+1} x = A(n, \alpha x), \quad \beta^{n+1} y = B(n, \beta y).$$

Once more, elimination of  $n$  [between the right-hand sides of formulae (37.28); see (2) above] leads to

$$(37.29) \quad F(\alpha x, \beta y, \alpha^{n+1} x, \beta^{n+1} y) = 0.$$

Thus, according to the definition of  $\phi x$  by (37.27), we have that  $\beta y = \phi \alpha x$  satisfies relation

$$(37.30) \quad F(\alpha x, \beta y, C, C') = 0.$$

Or, the system of equations

$$(37.31) \quad y = \phi x, \quad \beta y = \phi \alpha x$$

"are both true" when (37.27) is true. Elimination of  $y$  from (37.31) gives (37.23) as required. In other words, it was shown in art. 50 that the equation (37.24) has as solution any  $\phi x$  which satisfies the condition (37.27) where  $y = \phi x$ .

As an example of this "theorem", he undertook to find "what derivative is  $a' + b'x$  of  $a + bx$ " [art.51]. We can put a function  $a + bx$  in the form  $p(x+q) - q$ . Thus, if  $\alpha x = a + bx$ , we have that

$$(37.32) \quad \alpha x = b\left(x + \frac{a}{b-1}\right) - \frac{a}{b-1},$$

a symmetrical expression holding for  $\beta x = a' + b'x$ . By iteration it follows that

$$(37.33) \quad \alpha^n x = b^n\left(x + \frac{a}{b-1}\right) - \frac{a}{b-1},$$

and similarly for  $\beta^n x$ . He then let for simplicity

$$(37.34) \quad \alpha^n x = C - \frac{a}{b-1}, \quad \beta^n y = C' - \frac{a'}{b'-1}.$$

Thus, as in (37.2), he assumed for convenience the successive value of  $\alpha x$ ,  $\alpha^n x$ , to be equal to a constant<sup>(9)</sup>. Eliminating now  $n$  between the right-hand sides of (37.33) and (37.34) he obtained, by taking the logarithms,

$$(37.35) \quad \lambda \cdot \left[ y + \frac{a'}{b'-1} \right]^{\lambda b} - \lambda \cdot \left[ x + \frac{a}{b-1} \right]^{\lambda b'} = C' \lambda b - C \lambda b',$$

where  $\lambda x = \log x$  and  $C'$ ,  $C$  stand for  $\log C'$  and  $\log C$  respectively.

Regarding now

$$(37.36) \quad \log^{-1}(C' \lambda b - C \lambda b') = C,$$

equation (37.35) [solved algebraically for  $y = \phi x$ ] gives

$$(37.37) \quad \phi x = C^{\frac{1}{\lambda b}} \left[ x + \frac{a}{b-1} \right]^{\frac{\lambda b'}{\lambda b}} - \frac{a'}{b'-1}.$$

Apparently, solving (37.35) for  $x$  and changing the variable,  $\phi^{-1}x$  was determined in exactly the same form (37.37) only  $a, b$  were changed into  $a', b'$  and vice-versa and the exponent of  $C$  was now negative. Via (37.32), (37.37) and the corresponding formula for  $\phi^{-1}x$ , we can determine  $\alpha \phi^{-1}x$  and next  $\phi \alpha \phi^{-1}x$ , the latter proved easily to be equal to  $\beta x$ . Thus,  $a' + b'x$  is a derivative of  $a + bx$ , where the derivative function  $\phi x$  is given by (37.37) [art. 51].

With this example, De Morgan solved his first functional equation of the first order, in fact, equation

$$(37.38) \quad a' + b' \phi x = \phi(a + bx),$$

a case of (37.24) tackled above in art. 50<sup>(10)</sup>. Besides Collins, De Morgan was additionally influenced by Herschel's conception of  $\phi^n(x)$  as a function of  $n$  and  $x$  [see (24.14) and (36.30)]. Moreover, the use of logarithms and the elimination technique might had been the result of an indirect influence from Monge and Babbage [see (14.22)–(14.24); (25.38)–(25.39); art. 249–250].

An immediate concern was to examine the solution of problem (37.4), or (37.23)–(37.24), in the case where one or both of the functions  $\alpha x$  and  $\beta x$  are periodic. Focusing on periodic functions, the first move was to determine  $\phi^m x$  for any  $m$  when

$$(37.39) \quad \phi^n x = x,$$

for a specific  $n$  known. Drawing on Herschel's circulating functions [1818;2.8], he let  $r_1, \dots, r_n$  be the  $n^{\text{th}}$  roots of 1 and called

$$(37.40) \quad \frac{r_1^\omega + \dots + r_n^\omega}{n} = R_\omega,$$

where  $\omega$  any positive integer [see (28.19)]. If  $\phi x$  any periodic function of the  $n^{\text{th}}$  order, then we have (28.18) or

$$(37.41) \quad \phi^m x = x R_\omega + \phi x R_{\omega-1} + \dots + \phi^{n-1} x R_{\omega-n+1},$$

where  $\omega = (\text{mult. of } n) + m$ . For example, if  $\phi^2 x = x$ , then

$$(37.42) \quad \phi^m x = \phi x \frac{1 - (-1)^m}{2} + x \frac{1 + (-1)^m}{2}$$

[art. 66].

In art. 70 he let  $\phi x$  and  $\psi x$  be two periodic functions of the second order, the form of their successive values given by (37.42). Equating  $\phi^n x$  and  $\psi^n y$  to two constants  $c$  and  $c'$  respectively, he obtained after eliminating  $n$  (as in (37.35), (37.37))

$$(37.43) \quad y \phi x - x \psi y = c(y - \psi y) - c'(x - \phi x),$$

from which  $y$  is to be determined in terms of  $x$ . Thus  $\psi x$  can be made a derivative of  $\phi x$ .

Take now problem (37.4) and let only one of the functions  $\alpha x$  and  $\beta x$  be periodic. Evidently, (37.4) cannot be solved, for a derivative of a periodic function is also periodic [see (36.36)]. Aware, thus, that in this case (37.4) is impossible, De Morgan took the opportunity to delve into the notion of impossibility, a notion directly linked with that of discontinuity. Letting in (37.23),  $\beta x = x+1$  and  $\alpha x = 1-x$ , non-periodic and periodic functions of the second order respectively, he arrived at

$$(37.44) \quad x+1 = \theta(1-\theta^{-1})x.$$

Applying (37.42) to  $\alpha x$ , elimination of  $n$  between  $\alpha^n x$  and  $\beta^n y$  had as outcome an exponent of the form

$$(37.45) \quad 1/\log(-1)$$

which stood for him as a sign of impossibility, hence of discontinuity. Within this context he further contemplated on Babbage's impossible equation (27.41) [art. 71-74; see also 3.6]. Finally, he considered the initial problem (37.4), or equation (37.23) for  $\alpha x$  and  $\beta x$  periodic functions of any order. In this case, he proved in a theoretical way that indeed  $\beta x$  can be expressed as a derivative of  $\alpha x$  [art. 75].

In the course of these inquiries related to problem (37.4),

he investigated thoroughly a separate category of functions,

$$(37.46) \quad a+bx, \frac{a+bx}{a'+b'x}, ax^b,$$

whose successive functions are of the same form, which he called as "algebraical cognates" [art.55-65]. Among his concerns was the condition of convertibility of such functions [art.56-58] <sup>(11)</sup>.

He went on to study convertible functions. In art.76 he remarked that if  $\phi$  and  $\psi$  are convertible, so are their derivatives, and vice versa. He pointed out, however, that if

$$(37.47) \quad \partial\phi\partial^{-1}x = \partial\psi\partial^{-1}x,$$

we are not to assume that

$$(37.48) \quad \phi = \psi,$$

"though this [(37.48)] is evidently one of the values which satisfy that equation [(37.47)]" [art.76]. Thus, in many respects De Morgan was far more critical and careful than Babbage who had inferred from (26.3) or (37.14) that

$$(37.49) \quad \psi\alpha x = \alpha\psi x,$$

and hence  $\psi=\alpha$  [see 2.6, (3)].

His preliminary account included thus a complete study of problem (37.4), a first study of convertible functions in art.76-79 concluding with symmetrical functions in art.80-93. We quote from his [1842c, 737] for a summary of his inquiries into the latter species of functions;

But when  $\beta x$  is a repeating [periodic] function of the  $n^{\text{th}}$  order, any symmetrical function of  $x, \beta x, \beta^2 x, \dots, \beta^{n-1} x$  is a solution of  $\xi\beta x = \xi x$ . But so much is not necessary: for any symmetrical function of the set  $\chi(x, \beta x, \dots, \beta^{n-1} x), \chi(\beta x, \beta^2 x, \dots, x), \chi(\beta^2 x, \beta^3 x, \dots, \beta x)$ , will do; and the last is not necessarily symmetrical with respect to  $x, \beta x, \dots$ . Thus  $ab^2c^3 + bc^2a^3 + ca^2b^3$  is not symmetrical with respect to  $a, b, c$  <sup>(12)</sup>.

Thus, De Morgan dealt with the equation

$$(37.50) \quad \psi\phi x = \psi x, \text{ where } \phi^n x = x,$$

before examining the same equation when  $\phi x$  is non-periodic [see art. 88-89 and 102 respectively].

He then decided to introduce properly the "various classes of functional equations which it may be necessary to consider" [art.94]. Three cases were distinguished. In the first case,

given the relation

$$(37.51) \quad F(x, y, \phi y, \psi x, \dots) = 0$$

where  $F, \phi, \psi, \dots$  are all known forms, we seek the relation which exists between the variables  $y$  and  $x$ . In the second case, the functional forms are required to satisfy the equation, as for example

$$(37.52) \quad \phi x + \phi y = \phi(x+y),$$

independently of any relations between  $x, y, \dots$  [art.94-97] (13).

The third case, e.g. the study of the equation (26.53), or

$$(37.53) \quad F(x, \phi x, \phi \alpha x, \phi \beta x, \dots) = 0,$$

where  $F, \alpha, \beta$  are known functions and  $\phi$  the form sought, was the one to which the treatise was devoted [art.94,95,98].

Isolating (37.53) in art.98, he reproduced in brief Babbage's reasoning in [1815; see (25.33)-(25.35)]. He stressed that "In almost every case, it will be necessary to suppose a particular solution" of (37.53), and hastened to add that "precisely the same thing takes place in Algebra. For instance,  $x^2+ax=b$  is not solved, except by reduction to the more simple form  $y^2=b\dots$ " [art.98]. He introduced next Laplace's method for

$$(37.54) \quad F(x, \phi \alpha x, \phi \beta x) = 0,$$

on lines similar to those followed by Herschel in [1813b; see (24.19); 1836, art. 100].

The first basic concern was the solution of (37.1), or

$$(37.55) \quad \phi \alpha x = \phi x,$$

in other words, of the basic equation (25.5). Instead of relying upon Babbage's assumption of a particular solution -since, in this case "no general method can be given" [art.101]- De Morgan proceeded to apply his own method as put forward in art.50 for the solution of problem (37.4), or of equation (37.24). Now, the given equation (37.55) is evidently a case of (37.24) where in the latter  $\beta x = x$ . Hence, according to (37.25)-(37.27) [or, as in the example which followed, (37.33)-(37.34)], we are to eliminate  $n$  between

$$(37.56) \quad \alpha^n x [= \chi(n, x)] = c, \quad [\beta^n y =] y = c'.$$

But  $n$  does not appear in the second of the above two formulae. We are at liberty to write instead of  $c, c'$  "any functions of  $n$  which do not change on changing  $n$  into  $n+1$ ", thus, it suffices to eliminate  $n$  between

$$(37.57) \quad \alpha^n x [= \chi(n, x)] = \mu \cos 2\pi n, \quad [\beta^n y =] y = \nu \cos 2\pi n$$

where  $\mu, \nu$  are apparently arbitrary functional symbols ( $\chi$  known,

since  $\alpha$  is given )and  $v$  such that does not invert the cosine [art. 102; see also (2) above].

Let (37.57) give by solution

$$(37.58) \quad n = Ax, \quad n = By,$$

$A$  and  $B$  known functions [see, for example, (37.67)]. Let  $x$  be replaced by  $\alpha x$  in (37.57). We have

$$(37.59) \quad \alpha^n(\alpha x) = \mu \cos 2\pi(n+1) = \mu \cos 2\pi n, \quad y=c'$$

and hence

$$(37.60) \quad n = A\alpha x, \quad n = By;$$

thus, "the fundamental relation is preserved, namely, that a change of  $n$  into  $n+1$  is equivalent to a change of  $x$  into  $\alpha x$ ".

Elimination of  $n$  between relations (37.58) and (37.60) gives

$$(37.61) \quad y = B^{-1}Ax, \quad y = B^{-1}A\alpha x$$

or, if we call  $\xi\alpha x$  the solution of (37.55), we have that

$$(37.62) \quad \xi\alpha x = B^{-1}Ax [= v\cos 2\pi Ax].$$

Moreover,

$$(37.63) \quad \alpha^{n-1}(\alpha x) = \mu \cos 2\pi(n-1)$$

and thus, instead of (37.60) it would be more proper to say that

$$(37.64) \quad n-1 = A\alpha x, \quad n-1 = By.$$

In other words, as a result of this procedure we have the following "theorems"

$$(37.65) \quad \text{Let (37.57) with } \alpha^n x = c. \text{ Then, } n \text{ or } Ax \text{ satisfies the equation } A\alpha x = Ax-1 \text{ (i) and } \xi\alpha x = v\cos 2\pi Ax \text{ (ii)}$$

[art.102]<sup>(14)</sup>. This detailed account -considered with the process given for problem (37.4) and the example which followed it above- somehow clarifies De Morgan's condensed presentation given in [1842c] which we quoted at the beginning of this section.

For a direct application of (37.65) assume (37.55) with  $\alpha x = bx$ ,  $b$  constant. To define the exponential inverse of  $\alpha^n x$  we reason as follows:

$$(37.66) \quad \alpha^n x = b^n x = c \longrightarrow \log b^n c = \log c \quad \text{or}$$

$$(37.67) \quad n = Ax = \frac{\lambda c - \lambda x}{\lambda b},$$

thus, the solution of  $\phi bx = \phi x$  is given by

$$(37.68) \quad \phi x = v\cos 2\pi Ax,$$

where  $Ax$  is given by (37.67) and  $v$  an arbitrary function which does not invert the cosine [art.106]<sup>(15)</sup>. Another application of this method followed for the solution of equation (37.5). In this case elimination of  $n$  was carried out between



$$(37.69) \quad \alpha^n x = \mu \cos 2n\pi, \quad \alpha^n y = v \cos 2n\pi,$$

"in which, however, the present state of analysis will oblige us to assume either  $\mu$  or  $v$  constant form" [art. 115]. See also problems (37.6), (37.14) above.

On the whole, De Morgan's method was applied to several cases, the most important of them sketched so far in our study. In the rest of the part devoted to functional equations, it was mainly Babbage's manoeuvres that he used with few instances where he applied Laplace's method or Herschel's and Spence's techniques [on the latter two see (28.9)-(28.10); 2.8, (3)].

We conclude our study with a combination of De Morgan's and Laplace's method as applied to equations tackled by Herschel and Babbage in 2.4 and 2.6 respectively. Let equation (37.54) [or (24.19)]. Replacing  $x$  by  $\alpha^{-1}x$ , (37.54) is reduced to

$$(37.70) \quad F(\alpha^{-1}x, \varphi x, \varphi \beta \alpha^{-1}x) = 0.$$

Let  $Ex$  be the exponential inverse of  $\beta \alpha^{-1}x$  and assume

$$(37.71) \quad \varphi x = \psi Ex.$$

Then, it follows that

$$(37.72) \quad \varphi \beta \alpha^{-1}x = \psi E \beta \alpha^{-1}x = \psi (Ex-1).$$

Substitute (37.72) in (37.70) and write  $E^{-1}x$  for  $x$  which gives

$$(37.73) \quad F(\alpha^{-1}E^{-1}x, \psi x, \psi (x-1)) = 0,$$

or, if

$$(37.74) \quad u_x = \psi (x-1),$$

we have

$$(37.75) \quad F(\alpha^{-1}E^{-1}x, u_{x+1}, u_x) = 0,$$

"a common equation of differences" [art.175]. On similar lines equation (26.12), or

$$(37.76) \quad F(x, \varphi x, \varphi \alpha x, \dots, \varphi \alpha^n x) = 0$$

was reduced in art.175 to the finite difference equation

$$(37.77) \quad F(A^{-1}x, u_x, u_{x+1}, \dots, u_{x+n}) = 0 \text{ (16)}.$$

Summing up, we saw De Morgan's rather unsuccessful method proposed for the solution of the basic functional equations that appeared in Babbage's and Herschel's studies. Contrary to Herschel who had avoided transcendentals on the grounds that they were "useless" [see (24.32)], De Morgan was happy not only to make use of them, but also to seek for new transcendental forms [see art.123-128]. The confusion and ambiguity which characterized his exposition and procedures apparently left little ground for one to perceive easily his just critical remarks and foundational concerns [see also comments in (3), (4), (5), (6)]

above]. We conclude our study of De Morgan's treatise of functions with a brief review of his foundational inquiries in functions with two variables.

### 3.8 De Morgan on the calculus of functions with two variables [1836].

In the last part of his treatise, [1836, art.262-328], De Morgan extended his enquiries on functions with one variable to functions with two variables. This extension required the introduction of new concepts and notation, which accordingly gave rise to a more detailed classification and hence to a greater amount of assumptions and complications. Art.262-288 cover a foundational study of functions with two variables, the principal concept under consideration being the "zero-function"  $\varphi^{\overline{0},\overline{0}}(x,y)$  defined in analogy with  $\varphi^0(x)$ . After a detailed study of the zero-function and its properties, De Morgan applied his exponential-elimination technique in order to study his basic problem (37.4) on functions with two variables. In the rest of his essay, art.289-328, he focused on the solution of first order functional equations in two variables, such as,

$$(38.1) \quad \varphi(\alpha(x,y), \beta(x,y)) = \varphi(x,y),$$

$$(38.2) \quad \varphi(\psi(x,y), \psi(x,y)) = \psi(\varphi(x,y), \varphi(x,y))$$

and

$$(38.3) \quad \varphi(\alpha(x), \beta(y)) = \varphi(x,y),$$

concluding with few equations of order higher than one and some functional differential equations.

Due to an inconsistent notation and his classification of functions in various categories- according to the properties of their moduli [see (38.19)] and their zero-functions [(38.7)-(38.8)], his method of problem (37.4) becomes now more complicated. For example, the zero-function of  $\alpha(x,y)$ ,  $\alpha^{\overline{0},\overline{0}}(x,y)$ , is denoted occasionally as  $\alpha^{\overline{0},\overline{0}}$ ,  $\alpha^{\overline{0}}$ ,  $\alpha_0$  or as  $\alpha^0$  [see art.268-270; 289-290; 270,279; 313 respectively]. Due to numerous typographical ambiguities, it is impossible in certain cases to distinguish between the functional symbol  $\alpha$  and the constant  $a$ . Parenthesis are often omitted, and thus both (38.1) and (38.3) can be found written in the same form

$$(38.4) \quad \varphi(\alpha,\beta) = \varphi,$$

causing difficulty in discerning between  $\alpha(x,y)$  and  $\alpha(x)$ .

Clarifications on such matters will be provided in our study in due course.

Once more, De Morgan draws heavily on Babbage's study of functional equations in two variables, particularly as in his [1816; 2.6]. Omitting Babbage's generalities, he does little more than reproducing the former's techniques or combining them with his own method. Of most interest is his foundational study of functions with two variables, an extension of Babbage's few pertinent comments in [1816;1822] on lines similar to those followed in the early part of the essay<sup>(1)</sup>. We will focus on De Morgan's original research in functions with two variables concluding with a brief commentary on his treatment of (38.1)-(38.3).

De Morgan introduced simultaneous substitution via two equations:

$$(38.5) \quad \overline{\psi^{m,m}}(\psi(x,y), \psi(x,y)) = \overline{\psi^{m+1,m+1}}(x,y) \text{ (2)}$$

and

$$(38.6) \quad \psi(\overline{\psi^{m,m}}(x,y), \overline{\psi^{m,m}}(x,y)) = \overline{\psi^{m+1,m+1}}(x,y).$$

In order to determine  $\overline{\psi^{0,0}}$  he wrote that if we make  $m = 0$  in (38.5)-(38.6) "in like manner as we hitherto have applied extended notation only to cases which satisfy such conditions of convertibility as are satisfied by the general notation, we must make the following equations determine  $\overline{\psi^{0,0}}$ , namely":

$$(38.7) \quad \overline{\psi^{0,0}}(\psi(x,y), \psi(x,y)) = \psi(x,y)$$

and

$$(38.8) \quad \psi(\overline{\psi^{0,0}}(x,y), \overline{\psi^{0,0}}(x,y)) = \psi(x,y) \text{ (3)}$$

[art.267]. Thus, the condition of convertibility for one-variable functions (36.25), was now via (38.7) and (38.8) extended to the condition of "simultaneous convertibility"

$$(38.9) \quad \beta(\alpha \overline{x,y}, \alpha \overline{x,y}) = \alpha(\beta \overline{x,y}, \beta \overline{x,y}),$$

where the bar denoted a parenthesis [art.267]<sup>(4)</sup>.

Accordingly,  $\varphi^0$  was extended to  $\overline{\psi^{0,0}}$  as follows: "In analogy with  $\varphi^0\varphi x = \varphi\varphi^0 x = x$ , which do not involve a condition, because  $\varphi^0 x$  or  $x$  is convertible with all functions, we shall let  $\overline{\psi^{0,0}}$  signify those functions only in which both equations [(38.7)-(38.8)] are satisfied" [art.268]. He remarked that (38.7) is satisfied by an infinite number of functions "independent of the form of  $\psi(x,y)$ "<sup>(5)</sup>. Further, "whenever the second equation is satisfied, the first is satisfied by one or other [sic] of the values of  $\overline{\psi^{0,0}}$  derived from it" [art.269]. He based his latter observation reasoning as follows. If  $a$  is determined from the equation

$$(38.10) \quad \psi(a,a) = \psi(x,y),$$

then, making  $x=y$  "it is evident that one value of  $a$  reduces itself to  $x$ " [art.269]. This reasoning is fairly close to that followed in art.32 in connection with the definition of  $\phi^{-1}(x)$  [see (36.21)-(36.24)].

Going further than Babbage [1822] -who had defined  $\overline{\psi^{0.0}}(x,y)$  simply by means of (29.5)-(29.6)- he set off to inquire into the properties of the "zero-function of  $\psi$ ",  $\overline{\psi^{0.0}}$ , starting with an example. Let

$$(38.11) \quad \psi(x,y) = x^2+y.$$

According to (38.10) we have that

$$(38.12) \quad a^2+a = x^2+y \longrightarrow a = -\frac{1}{2} \pm \left(-\frac{1}{4} + x^2+y\right)^{1/2}.$$

Taking the positive sign, the zero-function of (38.11) was deduced from (38.12) to be

$$(38.13) \quad \overline{\psi^{0.0}}(x,y) = -\frac{1}{2} + \left(-\frac{1}{4} + x^2+y\right)^{1/2}.$$

Indeed, if we make  $x=y$ , it follows from (38.13) that  $\overline{\psi^{0.0}}(x,x)=x$ , or, in addition to (38.8), (38.7) is also satisfied [art.269].

In art.270 De Morgan introduced the name of "primitive zero-functions" as "all those which have no other zero-functions except themselves"; that is all functions  $\chi(x,y)$  which satisfy the equation

$$(38.14) \quad \chi(x,x) = x^{(6)}.$$

He then claimed that every function  $\phi(x,y)$  can be represented under the form

$$(38.15) \quad \Phi\phi_0(x,y)$$

"where  $\phi_0$  means the zero-function of  $\phi$ , and is one of the primitive zero-functions contained in the general solution of  $\phi(x,x)=x$ " [art.270]. Based upon the properties (38.7)-(38.8) which define  $\overline{\psi^{0.0}}$  he proved readily that

$$(38.16) \quad \text{"The zero-function of } \alpha(x,y) \text{ is also a zero-function of } \psi\alpha(x,y), \psi \text{ being any form whatever of a single subject"}$$

and

$$(38.17) \quad \text{"The zero-function of a zero-function is the zero-function itself"}$$

[art.270]. A third property followed in art.271, its proof based on the form (38.15) where  $\Phi$  a function of a single variable:

(38.18) "all zero-functions are primitives".

In art.275 he delved further into a study of the form (38.15), calling  $\bar{\alpha}$  the "modulus" of  $\alpha(x,y)$  which he properly defined as follows: by  $\bar{\alpha}$  or  $A$  he defined the "modulus" of  $\alpha(x,y)$  as the "function  $\alpha(x,x)$  considered as of a single subject", or

$$(38.19) \quad \bar{\alpha}(z) = \alpha(z,z) = A(z).$$

Thus, according to (38.16)-(38.17) it can be established that

$$(38.20) \quad \alpha(x,y) = \bar{\alpha} \overline{\alpha^{\circ \cdot \circ}}(x,y) = A \overline{\alpha^{\circ \cdot \circ}}(x,y)$$

for every function  $\alpha(x,y)$  [art.270-275] ('?'). Let, for example

$$(38.21) \quad \alpha(x,y) = \frac{x^3+y^3 \log(x/y)}{x^2+xy+y^2} \quad (e).$$

Then, by (38.10) we have  $\alpha(a,a)=\alpha(x,y) \longrightarrow a/3 = \alpha(x,y)$  or

$$(38.22) \quad \overline{\alpha^{\circ \cdot \circ}}(x,y) = 3\alpha(x,y)$$

and by (38.19) that

$$(38.23) \quad \bar{\alpha}(z) \text{ or } A(z) = \frac{z}{3}.$$

Combining (38.22) and (38.23), formula (38.20) is satisfied for  $\alpha(x,y)$  given by (38.21) [see art.281].

This novel conception of any  $\alpha(x,y)$  being expressed in the form (38.15), or (38.20), had a big advantage in formulating the successive functions  $\overline{\alpha^{m \cdot m}}(x,y)$  of  $\alpha(x,y)$ . As we saw in 3.7, the ability to express  $\varphi^m(x)$  was one of the key points of De Morgan's technique put forward for the solution of equations (37.1) and (37.3). Based upon (38.20) and (38.7) he proved inductively that

$$(38.24) \quad \overline{\alpha^{m \cdot m}}(x,y) = A^m \overline{\alpha^{\circ \cdot \circ}}(x,y)$$

[art.281]. The last step required in order to consider the basic problem (37.4) for functions with two variables, was to extend the notion of the derivative (36.31). Like Babbage, he let

$$(38.25) \quad \varphi\alpha(\varphi^{-1}x, \varphi^{-1}y),$$

to stand for the derivative of  $\alpha(x,y)$ , where  $\varphi$  an arbitrary function [art.278].

Without proof he stated in art.278 the theorems

$$(38.26) \quad \text{Derivatives of derivatives of } \alpha(x,y) \text{ are derivatives of } \alpha(x,y).$$

$$(38.27) \quad \text{If } \mu(x,y) \text{ and } \nu(x,y) \text{ are derived by means of the same function } \varphi \text{ from } \alpha(x,y) \text{ and } \beta(x,y), \text{ then } \mu(\nu(x,y), \nu(x,y)) \text{ is the corresponding derivative of } \alpha(\beta(x,y), \beta(x,y)).$$

(38.28) If  $\alpha$  and  $\beta$  have the corresponding derivatives  $\mu$  and  $\nu$ , and if  $\alpha(\beta, \beta) = \beta(\alpha, \alpha)$ , then  $\mu(\nu, \nu) = \nu(\mu, \mu)$ .

(38.29) The successive functions, however taken, of any derivative of  $\alpha(x, y)$ , are the corresponding derivatives of the same successive functions of  $\alpha$ .

The definition (38.25) and the property (38.29) were attributed to Babbage [1816; see (26.45)]. By means of (38.29) it was in fact shown that for any  $p$  positive integer

(38.30)  $[\varphi\alpha(\varphi^{-1}x, \varphi^{-1}y)]^p = \varphi\alpha^p(\varphi^{-1}x, \varphi^{-1}y)$   
[art.278] ('9).

Taking next under consideration his theory of zero-functions he proved based directly on definitions that

(38.31) "All the derivatives of zero-functions are zero-functions"

and

(38.32) "If any function be simultaneously periodic then all its derivatives are the same".

The definition of simultaneous periodicity was given in analogy with functions with one variable (36.27), by

(38.33)  $\alpha^{\overline{m.m}}(x, y) = \alpha^{\overline{o.o}}(x, y)$

[art.280]. He distinguished two types of simultaneously periodic functions, the "permanent" and the "not-permanent". The first category consists of all zero-functions which obviously satisfy (38.33) since by definition  $\alpha(x, x) = x$  [see (38.7)]. He then provided a rule for how to construct non-permanent functions of any degree  $n$  based on any periodic function of a single variable of degree  $n$  and a zero function.

To  $k \in$ , for example, the zero-function

(38.34)  $\alpha(x, y) = 1/2 (x+y)$ .

If we consider this <sup>function</sup> as of a single subject, we have that

(38.35)  $\frac{2}{x+y}$

is periodic of the second degree [in respect with (38.34)]. For, by (38.34) we have that  $\alpha(x, x) = x$  and thus (38.35) is but  $\varphi x = 1/x$ , with  $\varphi^2 x = x = \alpha(x, x)$ . It suffices to substitute  $\alpha(x, y)$  for  $x$  and  $y$  in (38.35). We then have that

(38.36)  $\varphi\alpha(x, y)$

is simultaneously periodic of the second degree. Indeed, we have

$$\begin{aligned}
 (\varphi\alpha)^{\overline{2,2}}(x,y) &= \varphi\alpha(\varphi\alpha(x,y), \varphi\alpha(x,y)) = \left[ \frac{2}{\frac{1}{2}(\varphi\alpha(x,y) + \varphi\alpha(x,y))} \right] = \\
 &= \left[ \frac{2}{\frac{1}{2}(\frac{2}{2 \cdot 1/2(x+y)} + \frac{2}{2 \cdot 1/2(x+y)})} \right] = \frac{2(x+y)}{2+2} = \frac{x+y}{2} = [\alpha(x,y)] = [\alpha^{\overline{0,0}}(x,y)] = \\
 &= [(\varphi\alpha)^{\overline{0,0}}(x,y)] = \varphi^2\alpha(x,y) \text{ [art.280] } (^{1\alpha}).
 \end{aligned}$$

Having proved the property (38.24) in art.281, he introduced example (38.21) deriving the zero-function (38.22), the modulus A (38.23) and finally the successive function of (38.21) which, according to (38.24) is

$$(38.37) \quad \alpha^{\overline{n,n}}(x,y) = \frac{1}{3^n} \cdot \frac{x^3 + y^3 \log(x/y)}{x^2 + xy + y^2}$$

[art.281]. Now the ground was ready for a consideration of problem (37.4), that is of

(38.38) How to make any function  $\beta$  a derivative of any other function  $\alpha$ .

[art.284].

It was first proved that

(38.39) "If  $\beta$  be a derivative of  $\alpha$ , then both the modulus and zero-function of  $\beta$  are the corresponding derivatives of those of  $\alpha$ ".

According to the formula (38.20) we have

$$(38.40) \quad \beta(x,y) = B\beta^{\overline{0,0}}(x,y), \quad \alpha(x,y) = A\alpha^{\overline{0,0}}(x,y).$$

By assumption

$$(38.41) \quad \beta = \varphi\alpha(\varphi^{-1}x, \varphi^{-1}y),$$

hence, (38.40) gives readily

$$(38.42) \quad B\beta^{\overline{0,0}}(x,y) = \varphi A\alpha^{\overline{0,0}}(\varphi^{-1}x, \varphi^{-1}y).$$

Putting  $y=x$  in (38.42) we have that

$$(38.43) \quad Bx = \varphi A\varphi^{-1}x,$$

and thus the first part of proposition (38.39) is proved [art.284].

Replacing next  $Bx$  by  $\varphi A\varphi^{-1}x$  in (38.42) we have

$$(38.44) \quad \beta^{\overline{0,0}}(x,y) = \varphi\alpha^{\overline{0,0}}(\varphi^{-1}x, \varphi^{-1}y),$$

hence (38.39) is proved. "It is evident", remarked De Morgan,

"that the problem [(38.38)] cannot be generally solved. First make the modulus of one a derivative of the modulus of the other; then see whether the deriving function so obtained will make the zero-function of one the derivative of that of the other" [art.284]. He thus went forward to extend his technique used in (37.4) to the solution of the more general problem (38.38). We will illustrate his method with an example.

Let  $\alpha$  and  $\beta$  be given by

$$(38.45) \quad \alpha(x,y) = ax + by, \quad \beta(x',y') = (ax'^2 + by'^2)^{1/2}.$$

The next step is to find the successive functions of  $\alpha$  and  $\beta$ . According to (38.24) this amounts to finding their moduli and zero-functions. By (38.19)

$$(38.46) \quad A(z) = (a + b)z$$

and by (38.10),  $\alpha(\kappa, \kappa) = \alpha(x, y)$  or

$$(38.47) \quad \kappa = \overline{\alpha^{0,0}}(x,y) = \frac{ax+by}{a+b}.$$

Hence, by (38.24) we have

$$(38.48) \quad \overline{\alpha^{n,n}}(x,y) = (a+b)^n \cdot \frac{ax+by}{a+b} = (a+b)^{n-1} \cdot (ax+by).$$

On similar lines  $B = (a+b)^{1/2}$  and

$\overline{\beta^{0,0}}(x',y') = (ax'^2+by'^2)^{1/2}/(a+b)^{1/2}$ , hence

$$(38.49) \quad \overline{\beta^{n,n}}(x',y') = (a+b)^{(n-1)/2} (ax'^2 + by'^2)^{1/2}.$$

Omitting any computational details, De Morgan stated simply (38.48) and (38.49) equating their right-hand sides to unity so as to eliminate  $n$ . The result is

$$(38.50) \quad ax'^2+by'^2 = ax+by.$$

Dividing both sides of (38.50) by  $(a+b)$  and taking the square roots, we have

$$(38.51) \quad \overline{\beta^{0,0}}(x',y') = \left[ \frac{ax+by}{a+b} \right]^{1/2} = (\overline{\alpha^{0,0}}(x,y))^{1/2},$$

or, the zero-function of  $\beta$  is derived from the zero-function of  $\alpha$  by  $\varphi(z) = \sqrt{z}$ . From (38.48)-(38.49) we notice that the modulus  $A$  of  $\alpha$  is also connected with  $B$  by means of the same relation  $B=\sqrt{A}$  [art.284] (11).

Still unsettled was the question "whether any given zero-function can be made a derivative of any other". He put forward a method which "will show that (continuously or discontinuously) a zero-function may, in some cases, be made a derivative of



another" [art.285]. We will not delve into his far from lucid method, but merely state that he illustrated it with the following example. He let

$$(38.52) \quad \alpha(x,y) = \frac{x+y}{2}, \quad \beta(x,y) = (xy)^{1/2}.$$

where  $\alpha$  and  $\beta$  are evidently zero-functions. His procedure gave as a result that  $\beta$  can be a derivative of  $\alpha$  by means of the function

$$(38.53) \quad \varphi(x) = e^{mx}$$

[art.285] <sup>(12)</sup>.

Let us now conclude this foundational study with a few words on his theory of the "simultaneous inverses". He claimed in art.286 that this theory can be reduced to that of inverses of functions of one subject only, by the equations

$$(38.54) \quad \varphi(x,y) = \Phi \overline{\varphi^{0,0}}(x,y), \quad \overline{\varphi^{n,n}}(x,y) = \Phi^n \overline{\varphi^{0,0}}(x,y).$$

Thus  $\Phi^{-1}$  and  $\Phi_{-1}$  are the moduli of the convertible and inconver-  
tible inverses respectively. In the former case we have:

$$(38.55) \quad \Phi \overline{\alpha^{0,0}}(\Phi^{-1} \overline{\alpha^{0,0}}, \Phi^{-1} \overline{\alpha^{0,0}}) = \Phi \Phi^{-1} \overline{\alpha^{0,0}} = \overline{\alpha^{0,0}}(x,y),$$

a similar relation holding for  $\Phi$ ,  $\Phi^{-1}$  interchanged. Let

$$(38.56) \quad \varphi(x,y) = x + y.$$

We readily have that

$$(38.57) \quad \overline{\varphi^{0,0}}(x,y) = \frac{x+y}{2}, \quad \Phi z = 2z, \quad \Phi^{-1} z = \frac{z}{2},$$

hence,

$$(38.58) \quad \overline{\varphi^{-1,-1}}(x,y) = \overline{\alpha^{0,0}}(\Phi^{-1} \overline{\alpha^{0,0}}, \Phi^{-1} \overline{\alpha^{0,0}}) = \frac{\frac{(x+y)}{2} + \frac{(x+y)}{2}}{2} = \frac{x+y}{4}.$$

The rest of his essay was devoted to functional equations. The first case to be considered was (38.1) in the form (38.4) or

$$(38.59) \quad \varphi(\alpha, \beta) = \varphi.$$

According to the method applied to equations in one variable, the general solution of (38.59) (an extension of equation (37.55)) depends upon the determination of

$$(38.60) \quad \alpha(x,y) \overline{\alpha^{n,n}}_{\alpha, \beta}, \quad \beta(x,y) \overline{\beta^{n,n}}_{\alpha, \beta} \quad (13)$$

as explicit functions of  $n, x, y$ . The symbols at the right-hand side denote "internal substitution", that is  $x$  is replaced by  $\alpha(x,y)$  and  $y$  by  $\beta(x,y)$  [art.288, 291].

Let

$$(38.61) \quad \alpha(x,y) \overline{\alpha^{n,n}}_{\alpha, \beta} = V(x,y,n), \quad \beta(x,y) \overline{\beta^{n,n}}_{\alpha, \beta} = W(x,y,n).$$

and  $f$  a convenient function, by means of which  $V$  and  $W$  are related. Assume that

$$(38.62) \quad f(V, W) = 0.$$

Then, from this equation we can deduce  $n$  in the form

$$(38.63) \quad n = F(x, y),$$

[the equivalent of the exponential function  $A$  of  $\alpha$  in (37.58)].

According to his theory  $F$  must satisfy equation

$$(38.64) \quad n-1 = F(\alpha, \beta) \text{ or } F(\alpha, \beta) = F(x, y) - 1.$$

From (38.64) we may "either derive transcendental solutions", such as

$$(38.65) \quad \varphi(x, y) = \theta \cos 2\pi F(x, y),$$

or, "by taking another form,  $f_1$  instead of  $f$ " derive a new form  $F_1$  obtaining

$$(38.66) \quad \varphi(x, y) = F(x, y) - F_1(x, y),$$

that is "any number of particular solutions, and may thus form a general solution" [art.291] (14).

For example, take the equation

$$(38.67) \quad \varphi(x + y, 2y) = \varphi(x, y).$$

We can easily find that

$$(38.68) \quad \alpha(x, y) \frac{\eta, \eta}{a, b} = x + (2^n - 1)y \text{ and } \beta(x, y) \frac{\eta, \eta}{a, b} = 2^{ny}.$$

It remains "to determine  $n$  by making  $x-y+2^{ny}$  any function whatsoever of  $2^{ny}$ ", or, make

$$(38.69) \quad 2^n = \frac{1}{y} \psi(x-y),$$

hence

$$(38.70) \quad n = \psi(x-y) - \frac{\lambda y}{\lambda 2}$$

where  $\lambda y = \log y$ . Now, taking "two such solutions, the difference of which merely gives an arbitrary function of  $x-y$ , we find  $\theta(x-y)$  as a general form of a solution" [art.292] (15).

One more example followed [art.293] and next De Morgan summarized his first conclusions drawing a comparison between functional equations in one and two subjects respectively, taking under consideration the degree of indeterminateness of the new problems that appeared. In art.294 we read:

Or rather, if we consider the greater indeterminateness of the problem as requiring greater generality in the terms employed to express the solution, we should say that, in functions of two subjects, a general algebraical form, containing an arbitrary func-

tion, stands in the same relation to the solution as the single algebraical function containing an arbitrary constant does to the solutions of equations of functions containing one subject only. If we consider the degree of indeterminateness of a problem as a conception of comparison which actually must be formed, though not capable of numerical appreciation, we are justified in saying that, within the limits of common algebra, the indeterminateness of  $\phi x$  is to that of  $\phi(x,y)$  in the same relation as an arbitrary constant to an arbitrary function of given forms<sup>(16)</sup>.

We thus see once more De Morgan's tendency to point out all possible relations between the concepts and symbols of the calculus of functions [see 3.6, 3.7]. In particular, the concept of the zero-function gave rise to new analogies between algebra and the calculus of functions [art. 272-274], as well as to a new ground for classification of functions [art. 274, 305]<sup>(17)</sup>.

In art. 296 he commented upon the distinction between internal and external substitution as in (38.60) and (38.5) respectively. He then went on to consider the equation

$$(38.71) \quad \phi(\alpha(x,y), \alpha(x,y)) = \phi(x,y),$$

distinguishing two cases; first when the modulus of  $\alpha$  is constant and next when this condition is dropped. In the first case he showed that (38.71) admits of no continuous solutions except  $\phi(x,y) = \text{const.}$  In the latter case we obtain a transcendental solution bearing "a great analogy with the case of subjects [functions] of a single function [subject]" [art. 297]<sup>(18)</sup>.

In art. 298 he introduced the inverse of  $\theta(x,y)$ , as "the function in which  $\alpha$  and  $\beta$  must be simultaneously substituted for  $x$  and  $y$  in order that  $\theta(x,y)$  may result". Let  $\alpha(x,y) = x'$ ,  $\beta(x,y) = y'$  from which  $x = \alpha_1(x',y')$  and  $y = \beta_1(x',y')$ . Then  $\theta(\alpha_1(x,y), \beta_1(x,y))$  is the function required<sup>(19)</sup>.

In art. 305 he investigated the "conditions of simultaneous convertibility", or the solution of the equation (38.2) or

$$(38.72) \quad \phi(\psi, \psi) = \psi(\phi, \phi)$$

in five distinct cases:

- 1) When  $\phi, \psi$  are both zero-functions.
- 2) When only  $\phi$  is a zero-function.
- 3) When both  $\phi$  and  $\psi$  have constant moduli.
- 4) When only  $\phi$  has constant modulus, and
- 5) When both  $\phi$  and  $\psi$  have variable moduli.

In the first case, (38.72) is reduced to  $\psi = \varphi$ , which is the only solution. In the second case we have equation (38.71). In the third case it is sufficient that the constant moduli are the same. If only  $\varphi$  has a constant modulus, then (38.72) is reduced to  $\psi(\varphi, \varphi) = \text{const}$ . In the last case it should hold that

$$(38.73) \quad \bar{\varphi} \bar{\psi} \bar{\psi} \bar{\varphi} = \bar{\psi} \bar{\varphi} \bar{\varphi} \bar{\psi},$$

which shows that the moduli of  $\varphi$  and  $\psi$  must be convertible and that  $\bar{\psi} \bar{\varphi}$ ,  $\bar{\varphi} \bar{\psi}$  must be the same. Thus, if we take the convertible functions

$$(38.74) \quad 2x^2-1 \text{ and } 4x^3-3x$$

and the zero-function  $\sqrt{xy}$ , we find the simultaneously convertible functions

$$(38.75) \quad 2xy-1 = \varphi \text{ and } \sqrt{xy} (4xy-3) = \psi$$

[art.305] (20).

Finally, equation (38.3) was treated in art.308. He distinguished between two classes of solutions, the "disjunctive" ones and the "simultaneous". He illustrated both cases with Babbage's examples (26.27) and (26.28) respectively. In the next article he reconsidered (38.3) for  $\beta(x) = \alpha(x)$ , applying his own method as based on the exponential inverse of  $\alpha$  [art.309]. He then considered equations of order higher than one, such as

$$(38.76) \quad \varphi^n(x, y) = \alpha(x, y)$$

[art.311], Babbage's equations (26.44) and (26.50) [art.319-325] and lastly functional differential equations, such as (26.54) and (25.49) [art.326-328].

3.9 De Morgan's mathematical work 1837-1865: connections between algebra and the calculi of functions and operations.

W.W.

According to <sup>^</sup>R. Ball, De Morgan's treatise on the calculus of functions [1836] <sup>^</sup>contains <sup>^</sup>"an investigation of the principles of symbolic reasoning, but the applications deal with the solution of functional equations rather than with the general theory of functions" [1889, 133]. Indeed, hardly 60 out of the 328 articles of his [1836] are devoted to a study of fundamental notions, their properties and their symbolical connections. Contrary to his statements in art.1 [see 3.5.(2)-(4)], De Morgan's treatise was mainly concerned with technical aspects of the solution of functional equations. Moreover, these aspects were treated in a most obscure way for reasons discussed above [3.5; 3.6.(1);

3.7.(6); 3.8].

It is thus hardly surprising that De Morgan's treatise passed virtually unnoticed by his contemporaries [3.5]. Only J.Graves had apparently been through it in 1836 in connection with Babbage's paradox (27.41) [2.7,(12)]. Functional equations exerted very little interest among English analysts of mid 19<sup>th</sup> century and none of De Morgan's open questions formed the basis for "future" researchers of his time, contrary to his wishes. In fact, up to Boole's textbook [1860] it was mainly Laplace's method which survived, together with Babbage's elimination technique [see (24.19), (25.2); 2.5,(13)]. The only novelty to be noticed during that time, was Gregory's application of symbolic methods to linear functional equations [see 4.4].

It remains to see which were the consequences of De Morgan's treatise in his own work from 1837 onwards. As we shall see, functional equations as such featured in very few cases; that is mainly in connection with the calculus [1842c] and the foundations of algebra [1842a; 1849a]<sup>(1)</sup>. However, we will have the opportunity to record several instances where his early study of analogies and links between arithmetic, algebra, finite-difference operators and the calculus of functions had numerous effects in his later research in algebra and the calculus of operations. It is through this angle that we will present an overview of De Morgan's mathematical work up to 1865.

First of all we will comment upon his textbook on The differential and integral calculus [1842c], used on numerous occasions so far in the thesis as a source of reference [2.2,(13); 2.3,(21); 3.1,(2); 3.3,(7)]. In his study of the solution of partial differential equations under certain conditions, De Morgan came across an equation of the form

$$(39.1) \quad \phi x = \mu x + vx\phi ax,$$

where  $\mu, v, a$  are known functions [1842c,228]. This equation, initially solved by Babbage as (25.21), and consequently treated by De Morgan in [1836, art.133,176], was resumed in [1842c,727] followed by the remark that

though our present means of expression hardly enable us to lay the merest rudiments of what will one day be the calculus of functions, yet as much as this is known, that many such equations can be solved, and that there is an infinite number of solutions in most cases.

A brief theoretical study of the equation

$$(39.2) \quad \xi \delta x = \xi x ,$$

where  $\delta x$  a known function, was displayed at [1842c,737] for  $\delta x$  non-periodic and periodic respectively [see 3.7.(1),(12)]. This study was preceded by the distinction between "convertible" and "inconvertible" inverses and between "repeating" and "not repeating" functions [1842c,736]. The change of terminology in the latter case was explained in a footnote "I use the word repeating, and not periodic, because I consider that the latter term is wanting to express the difference of character between algebraic and trigonometrical quantities" [1842c,736].

As we saw in 3.6-3.8, De Morgan believed firmly that a mathematician should be capable of inventing new terms and symbols. This freedom he had exerted in several cases but he was often inconsistent rendering the study of his work difficult. By 1840 he was aware of the terminology of both Servois and Murphy, but in many cases he preferred his own flexible vocabulary. In his article on "Operations", published in Penny Encyclopedia in 1840, he distinguished three "characters of fundamental symbolical relations of algebra", namely, the "distributive" [absent in 1836], the "commutative or convertible", and the "depressible"; by the latter he referred to the laws

$$(39.3) \quad a^m a^n = a^{m+n}, \quad (a^m)^n = a^{mn}$$

[1840,444].

De Morgan included in his [1842c] a summary of Gregory's recent work in linear differential equations and an account of the symbolic solution of the earth-figure equation. Moreover, he included the basic theorems of the calculus of operations and related topics, such as Arbogast's derivations and Laplace's generating functions<sup>(2)</sup>. Thus, his book followed Lagrange's course of algebraic calculus. However, while avoiding Cauchy's name and notation, he gave an elementary exposition of Cauchy's theory of limits [3.1, (2); Richards 1987, 24-25].

J.P.Higman was particularly struck by De Morgan's elementary illustrations of the doctrine of limits, holding that the best treatise on the subject written prior to [1842c] was Wallace's "Fluxions" in 1815 [see 2.2, (5), and (4) below]. Higman, a member of the Analytical Society [Enros 1983, 33], had produced himself a textbook on the calculus, including limits [see Higman

1825; 3.2]. In a letter of his to De Morgan in 5.5.1847 we read that the latter was his "favourite pupil in the lecture room at Trinity"<sup>(3)</sup>. A year later Higman wrote to thank De Morgan for having sent him Formal logic [1847b]. With this opportunity he praised the latter's textbook by saying:

But although I have not had time to devote that attention to your work on Logic which the profound nature of the subject [....] requires, yet I have been reading again and again your work on the Diff<sup>1</sup> Calculus [1842c], which appears to me to be beyond all comparison the most profound and philosophical treatise that has appeared upon the subject. [....]. I got over from Cambridge, the works of Cournot, Navier, .... Moigno, and with the exception of the last which appeared to me very valuable as containing the lectures of Cauchy, they would not bear to be placed in comparison with yours. As for the Cambridge works on the subject which are very numerous, they appear to me utterly worthless<sup>(4)</sup>.

Higman would have also admired De Morgan's [1836], but there is no evidence in his letters that he had read it. In fact, De Morgan regarded limits as part of algebra. While discussing an impossible case in [1836, art.53], he claimed that it sufficed to find a limit of an undefined function "upon principles adopted in Algebra", adding in a footnote:

By Algebra, we mean also the theory of limits: we consider that common Arithmetic introduces the theory of limits in such expressions as  $\sqrt{2}$ ; nor can we find a meaning for the equation  $\sqrt{8}=2\sqrt{2}$ , without either the theory of limits or geometry. We assume, therefore, the former as a part of Algebra<sup>(5)</sup>.

Algebra was the ground for many of De Morgan's inquiries. We noticed in 3.6-3.7 various connections perceived by him in [1836] between algebra and the calculi of operations and functions. Let us see now what else he was to say on this matter. His first important statements, echoing strongly of his [1836], are to be found in his article on "Operations" [1840] cited above. In order to establish in elementary terms the method of separation of symbols, he presented Taylor's theorem in the form

$$(39.4) \quad \Delta\phi x = D\phi x + \frac{1}{2}D^2\phi x + \frac{1}{2.3}D^3\phi x + \dots$$

remarking that if we consider  $\Delta$ ,  $D$  as symbols of quantity, we could divide by  $\phi x$  and obtain

$$(39.5) \quad \Delta = D + \frac{1}{2}D^2 + \frac{1}{2.3}D^3 + \dots = e^{D-1}$$

[1840,492].

He then drew a bizarre analogy between the arithmetic of negative numbers and the calculus <sup>of</sup> operations. "Let two persons be required", he wrote, "the one to take four pebbles out of three, and the other to subtract a unit from (not the differential coefficient of  $\phi x$ , but) the direction to take the differential coefficient of  $\phi x$  [ $e^{D-1}$  or  $d/dx -1$ ], and it could hardly be said that the first had a more hopeless task than the second" [1840, 442]. This example was given so as to justify his claim that if mathematicians had given to operations a sort of existence as quantities, then the complications involved in the early conception of a negative quantity would have been resolved.

To illustrate further the view that arithmetic can be considered "as a science of operations upon one single magnitude, the unit", he reasoned as follows: "If we always express the unit by  $I$ , we may, if we please, consider 2 not as  $I+I$ , but as the direction to perform upon  $I$  the operation  $I+I$ ; so that 2 being merely a direction what to do,  $2I$  may represent the result of so doing". Arriving next at the equation

$$(39.6) \quad 3x(2I) = 6I$$

he claimed that "if we throw away  $I$ " we have

$$(39.7) \quad 3x2 = 6,$$

"an expression of equivalence of operations" deduced from (39.6) on lines analogous to the deduction of (39.5) from (39.4) by throwing away  $\phi x$  [1840,443].

This mode of reasoning is but a more sophisticated reproduction of that displayed in [1836, art.5, 12; (36.1), (36.6)] where he had called the symbols of algebra as symbols of operation. Moreover, the heuristic example with the pebbles, served once more as a justification for the claim that arithmetic is the only "true demonstrable root of human knowledge" [see 3.6,(5)]. Before we switch from the notion of arithmetic as a calculus of operations, we cite a relevant passage from some manuscripts on the calculus of operations written in 1854:

The calculus of operations is a system of significant algebra, in



which the letters signify, not quantities but operations performed on quantity: and not any operations, but certain operations proper for the purpose [...]. The idea of this calculus is not so far removed from ordinary notions as a beginner might imagine. Even common arithmetic may easily be made a calculus of operations. If I be the actual objective unit, then 2 signifies the operation of joining I and I, and thinking of them as joined, 5 signifies the operation of joining I,I,... and 1 signifies the no-operation of letting what there is alone<sup>(6)</sup>.

We thus have a case where De Morgan's introduction of a certain mode of reasoning in 1836 remained unchanged twenty years later. In [1840,442-3], he commented upon the insufficient demonstrations of the theorems of the calculus of operations by French analysts, holding that it was Servois who made a "separate species of calculus of functions" out of the properties of "separable" operations. The last important step was attributed to Peacock [1830] despite his *failure* to mention the calculus of operations. Peacock, he claimed, laid down the principles according to which "the theory of separation" is not a calculus of functions founded on algebra, but "algebra itself" [1840,443].

Instances from De Morgan's enquiries and views on the calculi of operations and functions are also found in his papers on the foundations of algebra contributed to the Cambridge Philosophical Transactions from 1839 up to 1844, cited according to their date of publication as [1842a,b; 1849a,b]. These four papers, studied cursorily by recent historians, deal mainly with the laws according to which the operations  $A+B$ ,  $AB$  and  $A^B$  are defined; while [1849a] deals with a consideration of  $A^B$  for complex numbers  $A,B$ , [1849b] is devoted to "Triple algebra" on lines similar to Hamilton's quaternions. Taking for granted all the recent studies on these papers<sup>(7)</sup>, we focus on the first and third of them so as to pick up a few scattered instances which show explicit influences from his early work.

De Morgan's distinction between the "art" and "science" of algebra dates <sup>from</sup> 1835 [see 3.4.(13)]. In fact, the same distinction appeared in his [1830,13; see 3.5 before (8)] in connection with arithmetic. He distinguished between "technical" and "logical" algebra; the former as concerned with "the art of using symbols under regulation", the latter with the interpretation of the

"symbolic results" in [1842a,173]. Attributing this distinction to Peacock, he preferred the term "technical" to Peacock's "symbolical" on the grounds that the latter "does not distinguish the use of symbols from the explanation of symbols" [1842a,177,fn]. We thus discern between a symbol being "defined" and a symbol being "explained" [1842a,174]. This dual character of algebraic symbols corresponds to what gradually came to be called by De Morgan as the "form-matter" distinction [see (35.1); and (6) below].

The content of [1842a] is particularly ill-arranged. Drawing first from his [1836] and some queer instances from the calculus of operations, De Morgan commented upon addition, the symbol  $\sqrt{-1}$  and inverse operations [1842a,177-182]. The rest of the paper deals once more with the same issues, this time within the context of Peacock's [1830]. Assuming a symbol  $\Omega$  such that

$$(39.8) \quad a + b\Omega = a_1 + b_1\Omega \longrightarrow a = a_1, \quad b = b_1,$$

he claimed that "no definite symbol of ordinary algebra will fulfil this condition". He remarked that in passing from  $x$  to  $-x$  by two operations, we make use in ordinary algebra of one particular solution of

$$(39.9) \quad \phi^2 x = -x,$$

namely of

$$(39.10) \quad \phi x = \sqrt{-1} x.$$

He further showed that if  $nx$  is a particular solution of

$$(39.11) \quad \phi^2 x = \alpha x,$$

$\alpha$  a known function, then a general solution of (39.11) is

$$(39.12) \quad f n f^{-1} x,$$

where  $f$  is arbitrary but convertible with  $\alpha$  [1842a, 177].

Thus, if  $f$  an arbitrary function which obeys the property

$$(39.13) \quad f(-x) = -fx,$$

a general solution of (39.9) is

$$(39.14) \quad f((-1)^{1/2} f^{-1} x).$$

Without referring to Babbage's transform (39.12), or to his [1836], he added: "with our very limited knowledge of the laws of inversion, no solution which we can now express in finite terms will afford any help. Our means of expression must be augmented before we can hope to overcome this difficulty" [1842a, 177].

He next let 0 stand for the state where "we have no object under our perception" and 1 for the first magnitude under consideration; thus, the "operation from one state to the other"

is denoted by  $0+1$ . He thus viewed addition as "connected with the symbol 0", calling it and subtraction respectively the "direct" and "inverse zero process", while, multiplication and division "might be called the direct and inverse unit processes" [1842a, 178-180]. At a footnote at page 179, he referred to [1836, art.12,13, 17] where one can find "some analogies connecting simple addition with zero, and multiplication with unity" [see (36.6)-(36.7) ]. Further, if we have a right "to assume a clear conception" of the direct-inverse distinction, "it is in the comparison of addition with subtraction, and of multiplication with division". But, he remarked that  $a+x$ ,  $a-x$  are inverse functions only with respect to  $a$ , and similarly  $ax$  and  $a/x$ . "When we come to the symbol  $x^n$  [1849a], then, and then only do we begin to describe inversion correctly: for we usually consider this as a function of  $x$  and not of  $n$ , when we assert  $x^{1/n}$  to be the inverse" [1842a, 180].

All these instances, explicitly drawn from his study of the calculus of functions, are not given attention by recent historians. On similar lines, the examples drawn below from the calculus of operations are also omitted. In a far from lucid way, De Morgan tried to explain technical algebra via the method of separation of symbols drawing an analogy between  $0, +, -$  and  $\phi(x \rightarrow h)$ ,  $\Sigma$  and  $\Delta$  respectively [1842a, 180-1]. Another analogy was displayed between  $0, 0+1, 0+1+1$ , and "a new progression of operations",  $\nabla^0, \nabla^1, \nabla^2, \dots$ , "in which the zero and its processes remain subject to the usual definitions"; in this case "nothing prevents us from supposing that the prescribed definitions of the unit process may remain true if  $\nabla^0$  be made the unit,  $\nabla^2$  being derived from  $\nabla^1$  by the same train of operations as  $\nabla^1$  from  $\nabla^0$ , and so on" [1842a, 181].

It is above all peculiar to De Morgan, among English algebraists of that time, to correlate in all possible ways fundamental notions of different branches of mathematics and to use any analogies perceived so as to delve into the nature of the specific notion under consideration. In his calculus of functions [1836] it was algebra which served more or less as the science of suggestion. Now, in his study of the foundations of algebra he draws on functional and operator notions and properties. The symbol  $\nabla$  above was apparently borrowed from Laplace or Herschel [1.6, (2); (23.14)]. In fact, he had introduced it as an operator in

[1836, art. 21] in connection with the so-called "Calculus of Ratios" (8).

The climax of his tendency to link functional with algebraic notions is to be found in the end of his third paper [1849a]. Having illustrated the general meaning of  $A^B$ , when  $A$ ,  $B$  were complex numbers, he commented upon the process of inversion of an algebraic operation. Drawing on the form-matter distinction, as put forward in [1836], he wrote [1849a, 142]:

But though we are not to look for any new inexplicables from  $A+B$ ,  $AB$ , or  $A^B$ , it should be remembered that there is a scale of ascent in the fundamental mode of deriving them from one another which does stop any-where. Addition being obtained, and the general notion of operation, the solution of  $\phi(x+1)=\phi x+c$  gives  $\phi x=cx$ , and introduces multiplication. Next  $\phi(x+1)=c\phi x$  gives  $\phi x=c^x$ , and introduces involution. But  $\phi(x+1)=c^{\phi x}$ , the solution of which gives the next step, gives for  $\phi x$  a function which has not been considered; though its particular cases  $\phi 1 = a$ ,  $\phi^2=c^a$ , [...] are known. If  $\phi x$  could be completely inverted, new inexplicables might, and perhaps would arise, either from this or some succeeding cases (9).

Besides the fact that he drew heavily on his [1836] in order to found his new arguments in the realms of the foundations of algebra, we also notice that De Morgan's mode of reasoning is virtually the same. Hypothesis after hypothesis, and words such as "may" and "probably" are to be found frequently in his writings together with hints for future research. Any mysteries perceived he noted optimistically, expecting their resolution; the thread of research is to be that of historical continuity. This tendency to bring to the surface any obscure points is rather part of his role as a teacher, than as a researcher. His effort, based on the certainty of the results produced so far, lies more in presenting a unified presentation of what has been achieved in various branches of mathematics through all possible links and analogies, than in fully studying a single topic and develop it further [see 3.7, (3)-(5)].

When a student, De Morgan was impressed by Woodhouse's dictum "Since it leads to truth, it must have a logic", applied then for the justification of imaginary quantities [1865, 179]. This dictum, together with Whewell's faith in historical progress [see 3.2, (11); 3.4, (7)] led De Morgan not to dismiss any impossible or

inexplicable entities, as for example inconvertible inverses, or discontinuous functions [3.6,(4),(13);3.7]. In [1836, art.184] he wrote: "Some writers have rejected divergent series, on account of the difficulties to which they are subject. But we shall endeavour to make it appear likely that series which are never divergent should be much more cautiously used". Thus, he did not dismiss any ambiguous notions<sup>(10)</sup>, but tried to delve into their "logic" and to embrace them all in a unique system whose basis was algebra.

As we had mentioned above, the very theory of limits was to be viewed from an algebraic angle [see (5) above]. In his [1865] he studied the concepts of 0 and  $0^{-1}$  or  $\infty$  in a similar manner as he had studied inverse operations in his [1836]. He is once more concerned now with a suitable extension of the sign of equality in algebra, so as the new system could embrace the notion of  $0^{-1}$  as an outcome of the solution of the equation  $x+2=x+1$ . All these enquiries, he wrote, are "set out, as wanted, in algebraical reasoning, which is often an attempt to snatch a fearful explanation, as if the writer were afraid of a reproving voice in the wind" [1865,188].

The crown of De Morgan's inquiries in algebra was his Trigonometry and double algebra [1849c]. The terms "single" and "double" algebra applied respectively to a study of algebra without the use of angles, and to a study which involved this notion [1849b,254]. In his book [1849c] "double algebra" stood as an equivalent to symbolic or technical algebra [1849c,89]. Some passages echo Condillac's epistemology, while others give evidence of his outmost concern with the intricate matter of inversion.

Language itself is a science of symbols (namely, words) having meanings (which are described in the dictionary, by words of the same or other language) and rules of combination (laid down in its grammar)<sup>(11)</sup>.

Further below we read [1849c,93,fn]:

Most inverse questions lead to multiplicity of answers. But the student does not fully expect this when he asks an inverse question, unless he be familiar with the logical character of the predicate of a proposition. A always gives B: what gives B? answer, A always, and, for aught that appears, many other things.

This instance reinforces his earlier arguments on that logic

should be taught to students before they are acquainted with advanced mathematics [1835a,293; 3.4,(14)].

In his [1849c, 101-5] he encapsulated all his previous inquiries in symbolic algebra, laying down 14 laws which establish the operations  $A+B$ ,  $AB$ ,  $A^B$ . In so doing, De Morgan was very close to formally defining the notion of field in abstract modern algebra<sup>(12)</sup>. Earlier in the book he had claimed that symbolic algebra is an "apparently useless art, except as it may afterwards furnish the grammar of science" [1849c, 92-3]. He added that by the 14 laws of combination of algebraic symbols we are provided with a "symbolic algebra which may here-after become the grammar of a hundred distinct significant algebras" [1849c, 101]. Interpretation of symbolical forms was a matter that concerned him more than the mere establishment of relations between such forms. In the 1860's he held that algebra was not to be considered as "satisfactorily established until every symbolic change has its interpretation" [1865, 171].

His interest in assigning suitable interpretations to mathematical symbols is particularly apparent in his brief inquiries into the nature of the calculus of operations in the 1850's. In the manuscripts quoted above in (6), he discussed the symbol  $E$ , where  $E$  stood for the forward difference operator. He held that "our view of the calculus of operations can only be compared to such a view of algebra" as we take in arithmetic before the acceptance of negative numbers. "We can hardly pretend, with safety to use any fractional indices of operation, though we may put them on trial in a case or two". He next referred to his [1849c, 101-3] for the rules of common algebra, remarking that  $E$  is distributive, and commutative with constants, adding, however, that some "compound symbols", such as  $\log E$  or  $e^E$ , do not admit of immediate interpretation. He next appealed to his functional inquiries reasoning as follows:

In order to understand the possibility of introducing such symbols, we must remember that all the leading combinations of algebra have what we may call their functional definition, that is, each combination may be defined by a functional property which belongs to that combination and to 40 others. Thus we know that  $\phi(x+y) = \phi x \cdot \phi y$  cannot be true independent of  $x$  and  $y$  unless  $\phi(x) = a^x[\dots]$ . Now, if instead of indicating by  $\phi$  a process of creating new value by operating upon value, we could indicate a process of creating new

modus of operation by operating upon operations, and if  $A, B$ , being any operations and  $AB$ : compound,  $\phi(AB)$ : new operation equivalent to  $\phi A + \phi B$ , then, if  $\phi e = 1$  we might [have]  $\phi A = \log A$ . We cannot however, up to this time, arrive at any notions of operation sufficiently general to make the proceeding of any use in demonstration<sup>(13)</sup>.

The same mode of reasoning, as based on functional definitions and properties, was to appear in his papers on the syllogism in the 1850's. All the issues listed in (35.1) were to be applied in his gradual study of the abstract copula and of the calculus of relations; among them the form-matter issue studied in 3.6. Another instance of it appeared in his [1849a, 142; see (9)] but as a term it is found as such in his philosophical paper on infinity [1865]. This paper draws heavily on De Morgan's logical studies and at certain instances reminds us of Boole's study of infinity in his Laws of thought [1854]<sup>(14)</sup>.

We conclude our overview of De Morgan's late mathematical work with few passages from his paper on infinity where we see his more mature views on the topics discussed so far in 3.4-3.9. Actually his views remain the same, only now they are clothed in a more philosophical language, an outcome of his study of logic in the 1850's. In [1865, 169] we read:

Those who have seized the spirit of the relation between the different forms of algebra, the ascent from arithmetic to single and thence to double algebra, and to such triple algebra as has been given, the divergence to the calculus of operations, the algebra divested of some of its laws which has been made an extension of the calculus of operations, and the method of quaternions, seek for illustration of difficulties by allowing the formal science to remain untouched, and looking for other matter of meaning to the symbols, under which all the relations of form shall be preserved. Algebra is a deduction of complex relations from simple relations, in which the meaning of the complex is derived from that of the simple, but the meaning of the simple requires material introduction, and cannot be deduced from the form itself. Set a person to interpret the hieroglyphic  $A+B=B+A$ , telling him no more than that = is a sign of sameness, and he may come out successful by attributing to + representation of any convertible relation whatever; as that A cannot walk side by side with B unless B walk side by side with A. The fundamental forms of arithmetic and algebra are so few,

that no doubt many systems of meaning are capable of giving life to them<sup>(15)</sup>.

A parallelism between algebra and logic, followed by certain pedagogical views on algebra, form the concluding section of his paper on infinity. Raising the subject of the neglect of pure logic by mathematicians, he wrote:

I know of no mathematical writing which more strongly illustrates my opinions on this point than the first edition of Dr Peacock's Algebra. It is founded on the basis of the permanence of equivalent forms. This is a very near approach to the assertion that algebra is, like logic, a formal science: nothing was wanted but an introduction and incorporation of that distinction between form and matter which now rules in the definition of pure logic. My mode of statement would be that algebra ought to be a formal science: I do not maintain that it is. It will become a formal science when all its forms, without exception, shall be true of every material instance, equally without exception<sup>(16)</sup>.

He continued his arguments giving two examples of identities which are not "purely formal". Algebra, he held, is not a formal science until "the symbols are so understood that  $2x = x$  gives  $2 = 1$ ". Thus the "form of algebra "

(39.15) "ab = b necessitates a = 1"

"is not universal; it fails when  $b = 0$  " [1865, 181; see also Richards 1987, 29-30].

With these last citations from De Morgan's late mathematical work [1865] we have a complete - more or less- view of his whole work, from early in 1830's up to early in the 1860's, strictly through those of his concerns that found the basis of his essay on functions [1836]. As the time passed by, he looked upon the theories of empirical philosophers critically [1865, 160]. However, his mathematical point of view, reinforced by his mature work on logic, remained deep down the same. Stressing in his own peculiar way the operational character of mathematical symbols, he never ceased to seek for analogies between algebra and the calculi of operations of functions. In different pieces of his work he tried to unite these branches of mathematics, limits and divergent series included. His interest in the proper study and instruction of the foundation of both algebra and functions



remained unchanged together with his wider educational concerns [on the latter see some interesting comments in 1865, 146 fn 1]. In practice, algebra became more of an art than a science [Richards 1987, 30]. However, throughout his life De Morgan persisted in discovering the universal interpretation of algebraic forms. This tendency was in full accordance with the Victorian unitary view of truth. All the issues discussed so far in 3.4-3.9 will be seen applied implicitly or explicitly in his work in logic produced on parallel lines from 1831 up to the 1860's [6.4-6.9].

## Chapter 4

The development of the calculus of operations from Murphy to Boole: 1837-1845.

### 4.1 Introduction.

Physical applications had motivated Murphy to delve into a study of inverse distributive operators [1833b]. As a result he composed his [1837] in which he included a first study of non-commutative operations. Poor in applications, Murphy's abstract and general work gained no impact up to the early 1840's [3.3]. Independently from Murphy, Gregory justified the method of separation of symbols in 1839, providing a symbolical method for the solution of linear differential equations with constant coefficients on lines similar to those followed by Brisson and Français earlier in the century [1.6]. The inquiries of Gregory and Murphy formulated the main background for Boole's paper "On a general method in analysis" [1844]. The symbolical method invented by Boole could be applied now to equations with variable coefficients, marking the beginning of a new era within English mathematics.

This brief historical review is a more or less <sup>well</sup> known . In particular, Boole's significant contributions in the establishment of the operator calculus within differential equations are acknowledged in our days<sup>(1)</sup>. However, certain important issues have been overlooked and very little is known in connection with the actual techniques used, or with the evaluation of the results of symbolical methods in general. Focusing here on the period between Murphy's paper [1837] and Boole's method [1844] we notice first that the core of the latter's method is totally unfamiliar to us<sup>(2)</sup>. Moreover, hardly any attention has been paid to the applications of symbolical methods to differential equations, independently from Murphy's and Gregory's theory, from 1837 up to 1843. These applications form also part of Boole's background and can not be ignored.

In general we distinguish between two parallel and independent lines of research which brought as a result Boole's sophisticated symbolical approach. On one hand we have the theoretical

study of the calculus of operations from Brinkley and Herschel up to Murphy and Gregory together with the latter's method for equations with linear coefficients. This theoretical approach can be seen briefly as an attempt to improve over the work of French algebraists and to render its foundations rigorous.

The other line of research concerned strictly the study of differential equations which stemmed from Whewell's updated curriculum in mechanics. The study of electricity and physical astronomy gave rise to a consideration of the Riccati (33.2), the Legendre (33.5) [see 3.3, (8)] and the earth-figure (32.11) equations. Definite integrals, variation of constants, series, Taylor's theorem and instances of the method of separation of symbols were gradually put in action from 1830 onwards from Cambridge graduates such as Murphy, A.J. Ellis, Greatheed, Gaskin and R.L.Ellis, including an outsider, Brownwin. What is indeed interesting to notice is that, from Gaskin's solution of the earth-figure equation in 1839 well up to the 1850's, most of the papers published in the realms of symbolical methods focused on the earth-figure equation as their model<sup>(3)</sup>. This factor is very important, missed hitherto in a history of the calculus of operations.

With these views in mind, the contents of this chapter are as follows: Section 4.2 covers Greatheed's application of Taylor's theorem to partial differential equations in 1837 and Gaskin's treatment of the earth-figure equation in the form (13.33) in 1839 - as it appeared in Hymers [1839]. Gaskin's procedure apparently challenged R.L.Ellis and Brownwin to delve into a more general approach in 1841-43. The significant contributions of these two analysts are rescued from oblivion in 4.3. In 4.4 we cover Gregory's contributions between 1839-1843, concluding with Boole's work in the next two sections. In 4.5 we give a brief review of his work between 1841-1843, introducing next his obscure, lengthy paper [1844]. In 4.6 we study in depth the most important aspects of [1844] illustrating Boole's general method via his solution of the earth-figure equation. We will close in 4.8 with a commentary on the results obtained so far, including important instances from Boole's sequel paper to [1844], [1845d], in 4.7.

#### 4.2 The first applications of the calculus of operations to differential equations by Greatheed and Gaskin: 1837-1839.

The method of the calculus of operations was not promoted as a mathematical technique within Whewell's liberal education. Still, our study in 3.2 of the questions posed in the Tripos

during the period 1820-1837 showed that the advanced students were acquainted with Herschel's Examples [1820] and his Notes in Lacroix [1816]. However, well up to Murphy [1837], the method of separation of symbols was in no way connected with the solution of differential equations<sup>(1)</sup>. The first noticeable attempt taken in that direction was by the Cambridge graduate S.S.Greatheed in his paper on "A new method of solving equations of partial differentials" published in the Philosophical Magazine in 1837.

Greatheed, fourth wrangler [Trinity College] in 1835, was a composer of church music. He also contributed various papers in the first volume of the Cambridge Mathematical Journal assisting Gregory in the editorship of the journal. We have no evidence whatsoever on Greatheed's background when he composed his [1837] mentioned above; a most plausible hypothesis is that he drew on Herschel's work on the calculus of operations. However, we might add that in a later paper "On general differentiation" [1839] we have evidence that he was well acquainted with Laplace's generating functions, Murphy's definite integral methods and with Fourier's Théorie de la chaleur [1822]<sup>(2)</sup>.

Greatheed's opening remarks to his [1837] foreshadow the extraordinary interest English analysts were soon to show in the method of the calculus of operations:

Separation of the symbols of operation from those of quantity, has, as far as I know, been hitherto applied only to the calculus of finite differences, and to the differential calculus where both are involved. It appears to me that if any much greater eminence than that to which analysis has already been brought, remains to be obtained by it, that process is the most obvious and likely path<sup>(3)</sup>.

In brief, separation of symbols and Taylor's theorem

$$(42.1) \quad f(x+h) = e^{h \frac{d}{dx}} f(x),$$

served to reduce a partial differential equation to an ordinary one and to effect the latter's integration respectively. In great

detail he applied the combination of these two techniques to the equation

$$(42.2) \quad a \frac{dz}{dx} + b \frac{dz}{dy} = c,$$

where  $a, b, c$  constants, justifying thus the validity of his symbolical method by relying on its efficacy [1837, 239-241] <sup>(4)</sup>.

Greatheed's paper involved the study of <sup>the</sup> equation

$$(42.3) \quad \frac{dz}{dx} + XY \frac{dz}{dy} = Pz + Q,$$

where  $X = X(x)$ ,  $Y = Y(y)$  and  $P, Q$  functions of both  $x$  and  $y$ . Letting

$$(42.4) \quad y' = \int \frac{dy}{Y}, \quad \text{hence} \quad Y \frac{dz}{dy} = \frac{dz}{dy'},$$

and separating the symbols of operation from those of quantity, he reduced the "most general class" of equations treated by his method, (42.3), to the form of the linear differential equation

$$(42.5) \quad \frac{dz}{dx} + \left[ X \frac{d}{dy} - P \right] z = Q$$

[1837, 241-2].

The integrating factor of (42.5) is

$$(42.6) \quad e^{\int (X \frac{d}{dy} - P) dx} \quad \text{or} \quad e^{X' \frac{d}{dy}} e^{-\int P dx}$$

where  $X' = X dx$ . By multiplication with (42.6) the left-hand side of (42.5) becomes a total differential with respect to  $x$ . It remains to integrate both sides with respect to  $x$ , add an arbitrary function of  $y$  and finally divide both sides by (42.6). The left-hand side gives  $z$ , while the right-hand side involves multiplication by an operator which changes  $y$  into  $y - X'$  according to (42.1) [1837, 242].

In many cases this final step involves multiplication of the right-hand side by factors of the form  $(a \pm d/d \log y)^{-1}$ . For such cases the following "curious theorem" was used:

$$(42.7) \quad \frac{1}{a \pm \frac{d}{d \log y}} \cdot y^m = \frac{y^m}{a \pm m}$$

Greatheed proved (42.7) based on the simple transform

$$(42.8) \quad y = e^t \longrightarrow d\log y = dt = dy/y.$$

He next stated (42.7) for any number of independent variables [1837, 243]. Notice that theorem (42.7) is a case of theorem (45.19), first given by Murphy as (33.54) or (33.55).

We will illustrate his method by solving the equation

$$(42.9) \quad x \frac{dz}{dx} + y \frac{dz}{dy} = nz.$$

We first divide both sides of (42.9) by  $x$  and next eliminate the coefficient of  $dz/dy$ ,  $y$ , by means of (42.8). Hence, (42.9) is reduced to the form (42.5), or

$$(42.10) \quad \frac{dz}{dx} + \left[ \frac{1}{x} \frac{d}{d\log y} - \frac{n}{x} \right] z = 0.$$

The integrating factor of (42.10) is

$$(42.11) \quad e^{\log x \cdot \frac{d}{d\log y} - n \log x} = x^{-n} \cdot e^{\log x \cdot \frac{d}{d\log y}}$$

Multiplication of (42.10) by (42.11) and integration relative to  $x$  gives:

$$x^{-n} e^{\log x \cdot \frac{d}{d\log y}} z = \varphi(y) = \varphi(e^{\log y}) \longrightarrow z = x^n e^{-\log x \cdot \frac{d}{d\log y}} \varphi(e^{\log y}) = x^n \varphi(e^{\log y - \log x}), \text{ or,}$$

$$(42.12) \quad z = x^n \varphi \left[ \frac{y}{x} \right]$$

[1837, 243].

Greatheed claimed that his method was superior to that of Lagrange. He also held that in cases to which Lagrange's method did not apply, his own gives, "very readily, an abbreviated expression for the series, in which the solutions may be exhibited, but which could often be hard to determine by indirect methods" [1837, 246-7].

From this last remark we have two basic reasons for Greatheed's motivation to apply instances of the calculus of operations in the realms of differential equations:

- (i) Insufficiency of known methods, and  
(42.13) (ii) Preference to direct methods which provide elegant, abbreviated forms of solution.

To these we might add a noticeable tendency towards abstraction

and generalization. All these elements, including the proposal of new theorems, such as (42.7), would also characterize the pertinent, independent attempts of his contemporaries. We have no evidence of the impact of Greatheed's method other than its reconsideration by Gregory in the wider frame of the latter's method in 1841 [see (44.36)]. We have evidence, nevertheless, of some of the factors that led to the rapid growth of symbolical methods in the 1840's, other than a concern for the justification of the calculus of operations, in (42.13).

Greatheed had meant to contribute further in this field [1837,246] but no more was published by him on differential equations. Together with Gaskin they are the initiators of the "practical" line of development of symbolical methods, as distinguished in [4.1.(3)] from the "theoretical" one. With Gaskin the actual motivation stemmed principally from educational concerns and an ardent interest in problem-constructing and solving. However, the results of his original solution of the earth-figure equation in 1839 contributed implicitly on the lines engraved by Greatheed in the exploitation of the method of the calculus of operations to a degree greater than expected by Greatheed (see (3) above) (s).

With Th.Gaskin we introduce also J.Hymers with whom he collaborated. Both were graduates of St. John's College as second wranglers in 1831 and 1826 respectively. Both were moderators; Gaskin in 1835, 1839, 1840, 1842, 1848 and 1851, and Hymers in 1833-1834. These two figures marked an era in the history of the college through their lectures and overall concern for the state of studies. Hymers was a specialist in textbook writing. The value of his books did not lay so much in "their presenting the result of Dr. Hymers' own researches as in their bringing into the reading of the university the methods and discoveries of continental mathematics" [Mullinger 1901, 279]. On the other hand, Gaskin was known "for his unrivalled skill in the construction and solution of problems [....]. It seems to have been his custom to put any new theorem that he discovered in the form of a problem, rather than in that of a paper in a mathematical journal" [Routh 1889, iiii].

In the 19<sup>th</sup> century, Gaskin's name was directly linked with the earth-figure equation. His biographer was well aware of his relevant contributions when he wrote:

In Dr. Hymer's treatise on "Differential Equations" [1839], we find the solution of one of problems proposed by him in the Senate House, when Moderator in 1839, given as the best and simplest method of solving an important differential equation. This is the equation, a particular case of which occurs in the theory of the figure of the earth<sup>(6)</sup>.

In what follows below we give Gaskin's treatment of this equation as in Hymer [1839] concluding with a commentary on the actual question as posed in the Tripos

Gaskin considered the earth-figure equation - hereafter cited as EFE - in the general form (13.33), or

$$(42.13) \quad \frac{d^2y}{dx^2} + n^2y = \frac{m(m+1)}{x^2} \cdot y$$

where  $m$  is an integer. He let

$$(42.14) \quad y = \frac{u}{x^m},$$

hence, (42.13) was reduced to the form

$$(42.15) \quad \frac{d^2u}{dx^2} + n^2u = \frac{2m}{x} \frac{du}{dx},$$

written more conveniently as

$$(42.16) \quad x[d_x^2u + n^2u] = 2md_xu.$$

If we differentiate (42.16) and add  $n^2$  times (42.16) to the result, we have, after separation of symbols

$$(42.17) \quad x[d_x^2 + n^2]^2u = 2(m-1)d_x(d_x^2 + n^2)u.$$

Continuing this process  $m$  times, we arrive at equation

$$(42.18) \quad [d_x^2 + n^2]^{m+1}u = 0.$$

The general solution of (42.18) is

$$(42.19) \quad u = (a + a_1x + \dots + a_mx^m)\cos nx + (b + \dots + b_mx^m)\sin nx.$$

Gaskin was interested in the case of  $m=2$ , for then (42.13) gives the original form of the EFE (13.32) or (32.11), in other words equation

$$(42.20) \quad \frac{d^2y}{dx^2} + n^2y = \frac{6y}{x^2}.$$

But the same process, as that given below, can be used for any chosen  $m$ . Letting  $m=2$  in (42.19), we differentiate the expression twice, substitute the result in (42.16) and determine accordingly



$a_1, a_2, b_1, b_2$  relative to  $a$  and  $b$ . As a result we obtain the value of  $u$  in two arbitrary constants:

$$(42.21) \quad u = a \left[ \left( 1 - \frac{n^2 x^2}{3} \right) \cos nx + nx \sin x \right] + b \left[ \left( 1 - \frac{n^2 x^2}{3} \right) \sin x - nx \cos nx \right].$$

Or, by means of (42.14) for  $m=2$ , (42.21) gives the value of  $y$  as

$$(42.22) \quad y = a \left[ \left( \frac{1}{x^2} - \frac{n^2}{3} \right) \cos nx + \frac{n}{x} \sin x \right] + b \left[ \left( \frac{1}{x^2} - \frac{n^2}{3} \right) \sin x - \frac{n}{x} \cos nx \right]$$

[Hymers 1839, 83-4].

The logic underlying Gaskin's procedure is very similar to that of A.J. Ellis [see (32.11)-(32.20)]. It consists in fact of a modification of the given equation so as to apply the method of variation of constants. The main difference lies in the generality of Gaskin's procedure which affords the integration of a wider class of equations. In this he was aided by the method of separation of symbols. The solutions of the EFE, "in the "standard" form (32.11) or (42.20), (32.12) and (42.22) respectively are different in form. Their equivalence is proved if we change the arbitrary constants  $a, b$  in (42.22) by

$$(42.23) \quad a = C \sin a, \quad b = C \cos a.$$

We then have the solution (42.22) in two constants  $a, C$  as

$$(42.24) \quad y = C \left[ \left( 1 - \frac{3}{n^2 x^2} \right) \sin(nx + a) + \frac{3}{nx} \cos(nx + a) \right].$$

If we make  $y=u$ ,  $n=q$ ,  $x=c$  and  $a=c'$ , (42.24) becomes (32.12) (7).

Gaskin considered further the case of (42.13) for any rational  $m$ . He let

$$(42.25) \quad y = x^{m+1} \cdot u.$$

Thus (42.13) was reduced to the form

$$(42.26) \quad x[d_x^2 u + n^2 u] + 2(m+1)d_x u = 0.$$

He claimed then that  $u$  is of the definite integral form

$$(42.27) \quad u = b^{-n} \int_{-n}^n (r^2 - n^2)^m \cdot \cos(xr + a) dr,$$

$a, b$  constants. The result was verified solely by differentiation and substitution in (42.26) [Hymers 1839, 84-5; 1858, 125].

These results first appeared in a question posed by him at the Tripos in January 1839 as follows:

If  $m$  be the greatest root of the equation  $m^2 + m = p$ , [0]

$$C d_{r=\eta^2}^m \left[ \frac{\cos(x\sqrt{r+a})}{x^m \sqrt{r}} \right] \quad (1)$$

$$\text{or } C x^{m+1} \left( \int_{r=n}^{\cdot} - \int_{r=n}^{\cdot} \right) (r^2 - n^2)^m \cdot \cos(rx+a) \quad (2)$$

are general values of  $y$  in the equation

$$d_x^2 y + \left[ n^2 - \frac{p}{x^2} \right] y = 0 \quad (3)$$

according as  $m$  is an integral or fraction and in the first case

$$(d_x^2 + n^2)^{m+1} u = 0 \quad (4) \quad \text{where } u = yx^m \quad (5);$$

apply the first or third result to solve the equation

$$d_x^2 y + \left[ n^2 - \frac{6}{x^2} \right] y = 0 \quad (6) \text{ (e).}$$

Now, since  $p=m(m+1)$ , equation [3] is no other than (42.13) and [6] is (42.20). Also [5] and [4] are (42.14) and (42.18) respectively. Apparently Gaskin expected the students either to apply [1] for  $m=2$  and prove that it satisfies the EFE [6], or to proceed from [4] by variation of constants and deduce for  $m=2$  the result (42.22) by [5] and (42.21).

Let us focus on expression [1] or

$$(42.28) \quad C d_{r=\eta^2}^m \left[ \frac{\cos(x\sqrt{r+a})}{x^m \sqrt{r}} \right]$$

where  $C$  and  $a$  stand for two arbitrary constants and  $r$  is replaced by  $n^2$  after the differentiation<sup>(9)</sup>. For  $m=2$  (42.28) gives

$$(42.29) \quad C d_{r=\eta^2}^2 \left[ \frac{\cos(x\sqrt{r+a})}{x^2 \sqrt{r}} \right]$$

If we effect the differentiation we notice that the result is the same as (42.22) if instead of  $a$  and  $b$  we put

$$(42.30) \quad a = C \cos a, \quad b = -C \sin a.$$

Finally, formula [2] stands for

$$(42.30) \quad C x^{m+1} \int_{-n}^n (r^2 - n^2)^m \cdot \cos(rx+a) = y.$$

If we make  $C=b^{-n}$ , we obtain the solution  $y$  of [3], or (42.13), given above via (42.25) and (42.27). We thus have exactly the same results as consequently given in [Hymers 1839] only under a

slightly different form.

Gaskin and Hymers spent the best period of their life devoted to <sup>the</sup> benefit of the students. Hymers was particularly prolific as a textbook writer and Gaskin was the first to be appointed as a moderator so many times. Hymers included in his A treatise on differential equations and the calculus of finite differences [1839] also other results attributed to Gaskin, such as the solution of the equation

$$(42.31) \quad dx^n y + p_1 dx^{n-1} y + \dots + p_n y = X,$$

where  $p_1, \dots, p_n, X$  are functions of  $x$  [Hymers 1839, 66-8]. Through his procedure Gaskin anticipated Frobenius in the solution of (42.31) in 1873. On the latter's process see [Ince 1927, 119; Vessiot 1910, 117].

Thanks to Hymers, Gaskin's work on the EFE did not pass unnoticed. R.L. Ellis was the first to perceive the importance of it and to contribute further in the direction hinted at by Gaskin [Ellis 1841a, 169; 4.3]. He was mentioned afterwards by De Morgan [1842c, 705]. As we shall see in 4.3, Gaskin must have been additionally an important influence for Bronwin, the most prolific writer in the realms of the calculus of operators in the 1840's and 1850's. Moreover, Gaskin implicitly influenced Boole in his own symbolical method [see 4.6]. But Boole's general method in 1844 overshadowed the results of his predecessors and Gaskin's name was hardly mentioned ever since.

But while Gaskin's followers were busy with establishing a high status for the method of the calculus of operations, their basic model for so doing the EFE, Gaskin's name shone brightly again several years later in Glaisher's elaborate paper on the Riccati equation [1881; see 1.4, (4)]. Glaisher not only incorporated Gaskin's results in his huge paper but <sup>also</sup> improved on them, linking them also with those provided by Boole and others on the EFE. Certain of Glaisher's comments will render our study more lucid and complete. But, à propos, we will introduce this eminent mathematician, who, like Gaskin, is totally ignored.

J.W.L. Glaisher, a second wrangler in 1871, was like Gaskin very concerned with the status of mathematical studies at Cambridge and also keen in constructing exam problems. A distinguished mathematician and astronomer, Glaisher contributed in almost every domain of mathematics, the total amount of his papers

being nearly four hundred. A defender of pure mathematics, he spent the most fruitful years of his life in the integration of ordinary linear equations in series<sup>(10)</sup>. An expert in distinguishing quality (he was an obsessive collector of pottery), Glaisher, like De Morgan, had an "unfailing interest in the history of mathematics, almost in an antiquarian spirit, from time to time" [Forsyth 1930, viii]. Working on the Riccati equation since his graduation -see his [1871;1872]- he combined his own original contributions in this domain with his parallel historical researches in his lengthy paper [1881]<sup>(11)</sup>.

Glaisher showed that (42.30) is not, as Gaskin had claimed, the general integral of the EFE (42.13) by pointing out that it contains one arbitrary constant only [1881, 815]. Instead, the complete integral solution of (42.13) was deduced in the form

$$(42.32) \quad y = x^{m+1} \left[ c_1 \int_{-\infty}^n (r^2 - n^2)^m e^{xr} dr + c_2 \int_{+\infty}^n (r^2 - n^2)^m e^{-xr} dr \right]$$

[1881, 816].

Omitting his procedures at the moment, we would like to state one more important result, that is the equivalence between the symbolical form (42.28), or

$$(42.33) \quad y = Cx^{-m} \left[ \frac{d}{dr} \right]^m \frac{\cos(x\sqrt{r+a})}{\sqrt{r}},$$

and

$$(42.34) \quad y = Cx^{m+1} \left[ \frac{1}{x} \frac{d}{dx} \right]^m \frac{\cos(nx + a)}{x}.$$

As we shall see, symbolical forms such as (42.33) and (42.34) were to reappear in the realms of research in the EFE from 1839 onwards. Glaisher attributed both forms to Gaskin [1881, 810-11; for details on (42.33)-(42.34) see 4.6].

#### 4.3 Ellis and Bronwin on the earth-figure equation: 1841-1843.

R.L.Ellis graduated from Trinity College as a senior wrangler in 1840. In his teens he had one tutor in classics and another in mathematics and would read Cuvier's Theory of the earth with pleasure. He soon began to read mechanics and at the age of thirteen he commenced reading differential and integral

calculus. Having poor eyesight he once mentioned to his colleague, H. Goodwin, that the theory of the earth's shape given in Pratt [1836; see 3.2] was read to him [Ellis 1863, xiii-xv]. By 1841 Ellis was acquainted both with Laplace's Mécanique Céleste and with Gaskin's researches on the EFE which motivated his own work on that equation as in his [1841a, b]<sup>(1)</sup>.

Ellis did not make wide use of symbolic procedures like Greatheed and Gaskin. Instead he assumed the solution of the equation to be in the form of a power series and by substitution and certain transformations he would achieve the reduction of his initial equation to one of the form

$$(43.1) \quad \frac{d^m y}{dx^m} + q^m y = 0.$$

In both papers the final solution was provided in a finite form; but in the second paper he showed a particular concern for symbolical solutions. The very core of Ellis's inquiries were conveniently summarized by De Morgan in his [1842c]. First we will discuss Ellis's method as in [1841a] for the EFE in the "standard" form, and next we will give De Morgan's version of Ellis's method of [1841b] for the EFE in the "general" form [see 4.2,(7) on this terminology].

Let the EFE in its initial standard form

$$(43.2) \quad \frac{d^2 y}{dx^2} + n^2 y = \frac{6y}{x^2}.$$

Ellis set  $y$  be in the series form

$$(43.3) \quad y = \sum a_m x^m.$$

By differentiation of (43.3) and substitution in (43.2) he obtained the relation

$$(43.4) \quad (m-3)(m+2)a_m + n^2 a_{m-2} = 0.$$

To simplify (43.4) he got rid of the factor  $(m-3)$  by defining a new series

$$(43.5) \quad z = \sum b_m x^m$$

depending on the first, (43.3), through the relation

$$(43.6) \quad (m+2)a_m = (m-1)b_m.$$

By substitution, (43.4) becomes

$$(43.7) \quad m(m-1)b_m + n^2 b_{m-2} = 0.$$

This form implies that the series (43.5) satisfies the equation

$$(43.8) \quad \frac{d^2z}{dx^2} + n^2z = 0,$$

or that

$$(43.9) \quad z = C \sin(nx+a),$$

where C and a are arbitrary constants<sup>(2)</sup>.

From the relations (43.6) and (43.7) between the coefficients of the two series (43.3), (43.5), we can deduce easily

$$(43.10) \quad \sum a_m x^m = \sum b_m x^m + \frac{3}{n^2} \sum (m-1) b_m x^{m-2}.$$

The trick is to write (m-1) as [(m+2)-3] on the right-hand side of (43.6), so that to divide by m+2 and then replace one of the  $b_m$  by  $b_{m+2}$  via (43.7). Thus, from the series we arrive at a relation between their coefficients and conversely back at a relation between the two series. In fact, (43.10) can be written in the form

$$(43.11) \quad y = z + \frac{3}{n^2} \left[ \frac{z}{x} \right]'$$

Substituting now the known value of z from (43.9), (43.11) gives readily the value of y in the known form (32.12) or (42.24):

$$(43.12) \quad y = C \left[ \left( 1 - \frac{3}{n^2 x^2} \right) \sin(nx+a) + \frac{3}{nx} \cos(nx+a) \right]$$

[1841a, 169-170].

Ellis applied consequently the same process for the general case where the coefficient of  $y/x^2$  in (43.2) is of the form  $m(m-1)$  (see (42.13)) but the operations involved are too complicated and the result was merely hinted at and so we omit it [1841a, 170-1]. In the rest of his paper he dealt with equations of the form

$$(43.13) \quad \frac{d^m y}{dx^m} + q^m y = p(p-1) \frac{1}{x^2} \frac{d^{m-2} y}{dx^{m-2}}$$

showing that when either p or p-1 is divisible by m, the solution of (43.13) "may be made to depend on that of" (43.1) [1841a, 172-5].

Ellis concluded his first paper with the following remarks: "The equations which we have solved are not a very numerous nor perhaps an important class. But one of them, at least [(43.2)] is

susceptible of a physical application of great interest; and so few equations of the higher orders are integrable in finite terms, that the discussion of those which are, has always some degree of value" [1841a, 177].

While the importance paid by Gaskin to the EFE motivated Ellis to put forward his so-called method of "successive reductions" and integrate a wider class of equations than (42.13) in his [1841a], Gaskin's symbolical formulation of the solution of (42.13) challenged further Ellis to modify both his method and the finite form of the solutions obtained so far in his [1841a]. In his next paper [1841b] Ellis dealt with equations of the form

$$(43.14) \quad \frac{d^1 u}{dx^1} + n^2 y = \frac{pm}{x} \frac{d^{1-1} u}{dx^{1-1}}.$$

Interested mainly in second-order equations, we will confine to the case of  $l=p=2$ , that is

$$(43.15) \quad \frac{d^2 u}{dx^2} + n^2 y = \frac{2m}{x} \frac{du}{dx},$$

or (42.15), which is a transformed form of the EFE in its general form (42.13) [see also (42.14)].

We will present Ellis's solution of (43.15) as given by De Morgan in simple terms in [1842c, 702-3]. Let  $u$  in (43.15) be, as in (43.3), of the form

$$(43.16) \quad u = \sum a_1 x^1.$$

By differentiation and substitution in (43.15) we obtain

$$(43.17) \quad (1-2)(1-2m+1)a_{1+2} + n^2 a_1 = 0.$$

Assume now,

$$(43.18) \quad a_1 = (1-1)(1-3) \dots (1-2m+1)b_1$$

which reduces (43.17) to the simpler form

$$(43.19) \quad (1+2)(1+1)b_{1+2} + n^2 b_1 = 0.$$

Equation (43.19) is equivalent to (43.7) which implies that the new series with  $b_1$  as coefficients satisfies equation (43.8). Thus

$$(43.20) \quad \sum b_1 x^1 = C \sin(nx+a).$$

It now suffices, as in the case of (43.2), to define the operational relation between the two series. From (43.18), by multiplication with  $x^1$  it follows that

$$(43.21) \quad a_1 x^1 = x^{2m} \left[ \frac{d}{dx} \frac{1}{x} \right] (b_1 x^1),$$

the step which differentiates the previous procedure as applied in [1841a]. From (43.21) we can readily deduce the relation between (43.16) and (43.20) by taking the series on both sides, or

$$(43.22) \quad u = Cx^{2m} \left[ \frac{d}{dx} \frac{1}{x} \right]^m \sin(nx+a).$$

De Morgan showed that (43.22) can easily be reduced to

$$(43.23) \quad u = C \left[ \frac{d}{dr} \right]^m \frac{\sin(\sqrt{r}.x+a)}{\sqrt{r}},$$

where  $r=n^2$  and  $C, a$  constants. Taking under consideration the link between (43.15) or (42.15) and the EFE (42.13) we see that (43.23) is no other but Gaskin's symbolical solution of (42.15) which gives consequently that of (42.13) [see (42.28)]<sup>(3)</sup>.

Attributing the result (43.23) to Gaskin [1842c, 705], De Morgan enriched his account of the EFE by incorporating Poisson's method for the solution of the Riccati equation and by pointing out the close link between these two equations [1842c, 703-4; see also 1.4]. Apparently De Morgan was the first English mathematicians to notice this connection. Through his textbook, he must have contributed towards the diffusion of the recent work produced in the domain of these two equations enriching apparently Boole's own background for his [1844, 4.5]<sup>(4)</sup>.

Ellis concluded his second paper by remarking that:

Fourier's expression, by means of definite integrals for the  $i^{\text{th}}$  differential coefficient of any function, would enable us to extend our solutions to the cases in which  $p$  [in (43.14)] is fractional. But merely analytical transformations of the results at which we have arrived are not of much interest, and the methods of effecting them are direct and obvious [....]. It is not difficult to multiply artifices, by means of which particular equations may be solved, but the results will, generally speaking, be of little value<sup>(5)</sup>.

The last sentence of this quotation, together with Greatheed's opening remarks in his [1837] -see [4.2,(3)]- are more than simply prophetic. Greatheed had realised that the calculus of operations would soon become a most useful tool in analysis. Ellis was far more critical seeing that by means by such devices, an analyst might indulge in over-using symbolical techniques the



results of which would only give alternative forms of solution which, interesting in theory, would be of little use in practice.

Ellis contributed several papers on distinct topics such as optics, logic [see 8.4,(8)], linguistics, geometry and definite integrals. I would like to refer briefly to his paper on "Analytical demonstrations of Dr. Matthew Stewart's theorems" [1841c] in which he applied symbolical methods for an analytical expression at page 271, and to his [1845] on finite differences, where he discussed Fourier's solution of partial differential equations. Finally, it is worth mentioning his two papers on functional equations [1842a,b]. In the first he delved briefly into an etymological study of the differences between algebraic and functional equations; Murphy is mentioned in connection with his subject-operation-result distinction [1843a,92; see also 3.3, (10)]. In his [1843b] he drew on Lagrange, Clairaut and Babbage in connection with functional differential equations. His observations ended by the remark that the value of the calculus of functions "arises chiefly from the wide views it gives of the science of the combination of symbols" [1843b, 138]. Ellis was a moderator at Tripos in 1844-1845. All his papers were collected in his Mathematical writings [1863] by W. Walton, including a biographical memoir by H. Goodwin<sup>(6)</sup>.

The next to contribute in the "practical" aspect of the development of symbolical methods was B. Bronwin. Bronwin was a Cambridge outsider, clergyman by profession of Denby, Yorkshire. Apparently his main link with Cambridge was the astronomer J.W. Lubbock, wrangler of Trinity College in 1825, with whom he held a correspondence on the theory of planetary disturbance from 1831 well up to 1850<sup>(7)</sup>. An ardent reader of French works in physical astronomy, Bronwin must have realized the absence of efficient methods for the integration of equations which featured prominently in the realms of physics. Bronwin contributed around 40 papers during the period 1841-1852. Among the subjects he worked on we notice elliptic functions, integral transformations, definite multiple integrals, the theory of tides and the problem of three bodies.

Bronwin's first paper [1841] was devoted to partial differential equations. Far from original, this paper was followed by a sequence of three papers [1843a,b,c] on differential and finite difference equations in which he put forward new techniques in-

cluding definite integrals. As soon as Boole published his general method [1844], Bronwin set off applying symbolical methods, contributing altogether over 14 papers. Not only the most prolific of the writers on the calculus of operations, Bronwin was to supersede Boole in abstraction and generality [see 5.2.5.4]. We will confine here to a brief study of his [1843a,b] which give evidence of the influence of Gaskin and Ellis, and form the basis for his important mature work in the late 1840's.

All Bronwin's papers under study in this section, together with those by Ellis, were published in the Cambridge Mathematical Journal. They also bear the same title "On the integration of certain differential equations". This title most often implied that the equations under study were principally second order equations with variable coefficients including the EFE.

Bronwin considered in his [1843a] the equation

$$(43.24) \quad \frac{d^2y}{dx^2} - q^2y = \frac{m}{x^2} y, \quad m = p(p-1).$$

Notice that, if we put

$$(43.25) \quad q = n\sqrt{-1},$$

and change  $m$  and  $p$ , the resulting equation is the EFE in the general form (42.13). Bronwin made use of the transform

$$(43.26) \quad y = ze^{-qx}$$

reducing thus (43.24) to the form

$$(43.27) \quad \frac{d^2z}{dx^2} - 2q \frac{dz}{dx} = \frac{m}{x^2} z.$$

Probably after Ellis, Bronwin let

$$(43.28) \quad z = \sum a_n x^n,$$

obtaining, after differentiation and substitution in (43.27), the relation

$$(43.29) \quad [n(n-1) - m]a_n = (n-1)2qa_{n-1},$$

which he called for convenience "the scale" of the equation [1843a, 29-37].

Putting  $n=0, -1, -2, \dots$  in (43.29), he obtained a particular integral of (43.27) in the form of the descending series in  $x$

$$(43.30) \quad z = a_0 \left[ 1 + \frac{m}{2qx} + \frac{m(m-1.2)}{2(2qx)^2} + \dots \right].$$

Notice that since  $m=p(p-1)$ , the terms of this series from the term concerning  $x^{-p}$  onwards vanish, in other words the series

(43.30) is a terminating one. Changing  $q$  in to  $-q$  he obtained another particular integral. By addition and consideration of (43.25) we have the complete integral of the given equation (43.24) in the series form

$$(43.31) \quad y = Ce^{-qx} \left[ 1 + \frac{m}{2qx} + \frac{m(m-1.2)}{2(2qx)^2} + \dots \right] + Ce^{qx} \left[ 1 - \frac{m}{2qx} + \frac{m(m-1.2)}{2(2qx)^2} - \dots \right]$$

where  $C, C'$  arbitrary constants. Changing  $q$  in to  $q\sqrt{-1}$ , suitably changing  $C, C'$  and arranging the odd and even terms, we have the solution of the EFE (42.13) in the form of a terminating series [1843a, 37]. As we mentioned above, Ellis had tried to provide the solution of (42.13) in a series form but his method was not as successful as Bronwin's. Despite any lack of reference to Ellis or the EFE, it is rather obvious that Bronwin was stimulated by Ellis's series method as in the latter's paper [1841a].

In his [1843a], Bronwin introduced briefly, via some examples, another technique, that of reducing an equation to an integrable one by successive term by term differentiation or integration [1843a, 31-2]. This method was further developed in his next paper leading to some interesting observations. He let the equation

$$(43.32) \quad A_0 \frac{d^2y}{dx^2} + B_0 \frac{dy}{dx} + C_0 y = 0$$

where  $A_0, B_0, C_0$  are functions of  $x$ , explaining theoretically the method of successive differentiation and consequent elimination of  $y, dy/dx$  etc. until we arrive at an equation with two terms which is easily integrated [1843b, 175-6]. By means of examples he further examined the different cases to which successive differentiation may lead [1843b, 176-9]. He next studied the same equation (43.22) by employing successive integration [1843b, 179-180], concluding that through these two procedures he approached "certain transformations" which he briefly noticed. Among his applications we trace an instance of the EFE with which we will conclude our study in this section.

Bronwin took the EFE (42.13) in the form

$$(43.33) \quad x^2 \left[ \frac{d^2y}{dx^2} + n^2 y \right] = my, \quad m = r(r+1).$$

He let consequently the transformation

$$(43.34) \quad \frac{dy}{dx} + \frac{r}{x} y = y_1.$$

By differentiation and substitution in (43.33), transform (43.34) reduces the given equation to one in  $y_1$  of the same form, only  $m=r(r+1)$  is now changed to  $m_1=(r-1)r$ . By repetition of this process  $m$  may vanish [1843b, 181].

He went on to mention that "Many of the equations treated of in these papers seem particularly adapted for the application of a definite integral. There is, however, great difficulty in that application..." [1843b,181]. Considering equation (42.26), the reduced form of the EFE in its general form (42.13) when  $m$  is rational, he accused Hymers [1839,84] in claiming that (42.27) is a complete integral since it reduces to two integrals one of which vanishes [1843b,182]. Bronwin suggested another form for the complete integral of (42.26) but he soon discovered that he was also mistaken [1843c, 263-4].

Comparing Gaskin's and Bronwin's procedures in connection with (43.33), we notice that the idea lying behind is the same, that is elimination of the last term of the EFE. Together with Ellis's method, all depend upon the solution (42.18). Though not all of them <sup>explicitly</sup> symbolical, these methods employ techniques which either suggested the final solution to be put in symbolical form or influenced implicitly the later use of operator methods. The fact is that all the figures discussed so far focused on finite solutions. With the exception of Greatheed, all the others used the EFE as the basic equation under consideration. In its general form, the EFE motivated in the early 1840's the search for general methods for the solution of a wider class of second order symbolical equations. We now turn to the other line of development, the "theoretical" one as developed by Gregory at that time.

#### 4.4 Gregory's justification and application of the method of separation of symbols: 1839-1943.

D.F. Gregory was born in Edinburgh in 1813. One of Wallace's favourite pupils, Gregory entered Trinity College Cambridge in 1833 graduating as a fifth wrangler in 1837. <sup>late</sup> in 1837, together with his friend R.L. Ellis, he founded the Cambridge Mathemati-

cal Journal. While being chiefly responsible for the editing of the first volumes of this journal, Gregory contributed to it several papers on the calculus of operations— particularly in its first volume. Shortly before he died in 1844, he was succeeded by Ellis in the editorship of this journal. The core of his theory in the realms of symbolical methods was incorporated in his Examples [1841] together with an abundance of examples which covered a wide range of problems from the differential and integral calculus including results from recent research in definite integrals by Cauchy, Fourier, Poisson and others. This book formed a most important sequel to Peacock's and Herschel's collection of Examples [1820]. Most of Gregory's mathematical papers were edited by W. Walton as Mathematical writings [1865] (1).

Like De Morgan, Gregory was initially influenced by Peacock's work in symbolic algebra and by Herschel's work on the calculus of operations. However, acquainted additionally with the work of Français, Servois and Brisson of the 1810's [see 1.6–1.7], Gregory was motivated to improve their foundational study of the method of separation of symbols and to extend their applications. Though aware of Cauchy's own inquiries in the solution of linear differential equations with constant coefficients by symbolical methods [1.7], Gregory expressed his desire to offer a method, less complicated than Cauchy's and nearer to that of Brisson [Gregory 1839a, 14]. This method was soon to be applied to various kind of equations with constant coefficients, including functional equations.

Gregory introduced two distinct ways for the solution of linear differential equations. The first method, improved by Boole in 1841, led to what is nowadays usually called as "Heaveside expansion theorem" which is none other than Cauchy's theorem (17.51). The second method consisted in developing binomials of the form  $(a - d/dx)^{-n}$  in powers of  $d/dx$ . This method was often preferred to the former as more convenient in applications. Postponing Boole's own early inquiries to 4.5, we will focus here on Gregory's own important results in the field of symbolical methods, as independent from Boole's work, between 1839 and 1843. In so doing we will draw both on his papers, providing pagination from his [1865], and on his Examples [1841]. At some occasions we will also draw from the second edition of the latter book [1846].

But before we proceed to Gregory's symbolical methods, let us first discuss his foundational study of the method of 'separation of symbols'.

Gregory wrote "On the real nature of symbolical algebra" and hence treated the nature of the calculus of operations, in his first paper contributed in the Transactions of the Royal Society of Edinburgh in 1838 and published as [1840]. As he mentioned in the opening paragraph, he was led to this investigation "in following out the principle of the separation of symbols of operation from those of quantity" [1840, 1]. He thus considered symbolical algebra as "the science which treats of the combination of operations defined not by their nature, [...], but by the laws of combination to which they are subject" [1840, 2].

Gregory discussed first the operations  $+$ ,  $-$  in arithmetic and geometry [1840, 3-4] and then made some interesting comments on the exponentiation operation, its basic rules being

$$(44.1) \quad a^m \cdot a^n = a^{m+n}, \quad (a^m)^n = a^{mn}.$$

When  $m$  is no longer integer or fractional, we know, claimed Gregory, only one interpretation of  $a^m$  where  $m$  indicates an operation, namely the result of Taylor's theorem where  $m=d/dx$  and  $h=\log a$ . That is, since

$$(44.2) \quad e^{h \frac{d}{dx}} f(x) = f(x + h)$$

we have that

$$(44.3) \quad a^{\frac{d}{dx}} f(x) = f(x + \log a)$$

[1840, 4-5].

But what about expressions  $(\pm)^m$  for  $m=d/dx$  or  $\log x$ ? Interpretations can not be provided, observed Gregory [1840, 5-6] and went on to introduce Servois's paper [1814] - "which does not seem to have received the attention it deserves" - where the properties of distributive and commutative functions are presented for the first time [1840, 6-7]. He mentioned Lagrange's theorem (15.3), acknowledged Herschel as "the person in this country who made the freest use" of the calculus of operations, "the principles of which did not appear to be very sound", remarking next that "In France, Servois was, I believe, the only mathematician who attempted to explain its principles, though Brisson and Cauchy sometimes employed and extended its application". Servois perceived, claimed Gregory, that the properties of distributivity

and commutativity "were the foundation of the method of separation of the symbols [...]. This view, which so far as it goes, coincides with that which it is the object of this paper to develop, at once fixes the principles of the method on a firm and secure basis" [1840, 7].

Gregory studied next the binomial theorem, "the most important in symbolical algebra" as "expressing a relation between distributive and commutative operations" [1840,9]. He observed that it applies to

$$(44.4) \quad (1+a)^{d/dx},$$

since  $a$  and  $d/dx$  are distributive and commutative operations, but not to expressions such as

$$(44.5) \quad (1+a)^{\log}, \quad (1+f(x))^{d/dx},$$

since  $\log$  is not distributive and  $f(x)$ ,  $d/dx$  are not relatively commutative [1840, 10].

Ignorant at this stage of Murphy's relevant work, Gregory wrote that "Closely connected with the binomial theorem is the exponential theorem, and the same remarks will apply equally to both". He mentioned the formula (33.35) in  $x$ , holding that " $x$  should be a distributive and commutative function". It is upon the formula which gives the development of  $e^x$  that Taylor's theorem in its symbolic form (44.2) depends upon, he noticed [1840, 10]. However, neither in this paper nor in any consequent one did Gregory delve at all in the establishment of  $e^x$  [ $e^a$  in (33.35)] as an operator. When he discovered Murphy's work in 1841 he confined to the statement: "Some very valuable researches on [The principle of the method of separation of symbols] by Mr. Murphy will be found in the Philosophical Transactions for 1837" [1841, 235] (2).

For Gregory it sufficed to establish the application of the binomial theorem in the case of

$$(44.6) \quad \left( \frac{d}{dx} \pm a \right)^n f(x).$$

Such operations were involved in his next paper "On the solution of linear differential equations with constant coefficients" [1839a] in which he put forward the two distinct methods for the symbolic solution of such equations. Since the binomial theorem holds true for symbols of quantity  $a, b$  which follow the three

laws of combination

$$a^m \cdot a^n \times = a^{m+n} \times \quad (1)$$

$$(44.7) \quad a[b(x)] = b[a(x)] \quad (2)$$

$$a(x)+a(y) = a(x+y), \quad (3)$$

it should also hold true for any symbols of operation which have these three properties. Gregory repeated (44.7) for  $f_1$ ,  $f$  general symbols of operation and observed that the three laws laid above are indeed satisfied if instead of  $f$ ,  $f_1$  we put the specific symbols  $d/dx$ ,  $d/dy$  or  $\Delta$  [1839a, 25].

In a later paper "Demonstrations of theorems in the differential calculus and calculus of finite differences" [1839c] he resumed the three laws (44.7) calling (1), (2), (3) as the index, the commutative and the distributive laws respectively. Since  $d/dx$  and  $\Delta$  obey these laws, the binomial theorem "may be at once assumed as true with respect to them, so that it is not necessary to repeat the demonstration of it for each case" [1839e, 109]. He wrote in a footnote "it is scarcely necessary to add, that those theorems which depend on the binomial, as the polynomial and exponential, are equally extensive, so that they too may be applied to the Differential Calculus and Calculus of Finite Differences" (3).

Gregory's proof of the basic theorems of the calculus of finite differences involved a far more considerate use of the method of separation of symbols than that used by Lagrange, Arbogast or Herschel. However, it lacked the detailed rigour of Murphy's demonstrations in [1837]. For example, defining  $D$  by

$$(44.8) \quad Df(x) = f(x+1),$$

it was readily deduced from Taylor's theorem (44.2) -which he took for granted- that

$$(44.9) \quad D = e^{d/dx}.$$

It was consequently derived by means of the definition of  $\Delta$  that

$$(44.10) \quad \Delta = D-1 \quad \text{or} \quad D = 1+\Delta. \quad [(33.18)]$$

Thus, "D being a linear compound of commutative and distributive operations, is also a commutative and distributive operation" [1839e, 113]. By means of the index law he would deduce from (44.10) Lagrange's theorem (15.3) or (22.12) [1839e, 120]. But no study was included in connection with the properties of distributive operations such as (33.21) or of inverse operations such as (33.37) and (33.38) which were implied in his work (4).

Putting the known formula



$$(44.11) \quad \frac{d}{dx} (uv) = u \frac{du}{dx} + v \frac{du}{dx}$$

under the form

$$(44.12) \quad \frac{d}{dx} (uv) = \left( \frac{d'}{dx} + \frac{d}{dx} \right) uv,$$

where  $d'/dx$  <sup>and</sup>  $d/dx$  operate only on  $u$  and  $u$  respectively. Gregory took the  $n^{\text{th}}$  differential of both sides of (44.12) expanding the right-hand side according to the binomial. He justified this by observing that  $d/dx$ ,  $d'/dx$ , are distributive, and since they are independent operations, they are also commutative. As a result he arrived at Leibniz's theorem (33.48) claiming that it holds true for  $n$  negative or fractional [1839e, 110-111] (5).

Leibniz's theorem proved useful in many ways. Among its applications we have for

$$(44.13) \quad u = e^{ax} \longrightarrow d'/dx = a,$$

or, from (44.12) it follows readily that

$$(44.14) \quad \left( a \pm \frac{d}{dx} \right)^n u = e^{\pm ax} \left( \frac{d}{dx} \right)^n e^{\mp ax} u$$

[1839e, 111-112]. The theorem (44.14) was initially introduced in [1839a, 18] in a slightly more complicated way (see also Koppelman 1971, 193]. Notice that theorem (44.14) was proved earlier by Murphy, as (32.54), through his complicated theory of transmutations. By means of (44.14) Gregory proved in [1841, 31-32] the formula (33.57) which gives  $y^{nd}nu/dy^n$  relative to  $x$  [see 3.3, (14), formula (i)]. We would like to add that a similar theorem to (44.14) was proved by Gregory in the finite difference calculus, namely

$$(44.15) \quad (D - a)^n X = a^{n+x} \Delta^n (X a^{-x}),$$

where  $D$  is given by (44.10), in [1839b, 34]. Theorems (44.14) and (44.15) were to play an important role in the symbolical integration of differential and finite difference equations respectively.

Having laid down Gregory's theoretical foundation we now proceed to his actual mode of solution of various kinds of equations with constant coefficients. He assumed any linear differential equation in the form

$$(44.16) \quad f\left(\frac{d}{dx}\right)y = X,$$

symbolically solved as

$$(44.17) \quad y = \left[f\left(\frac{d}{dx}\right)\right]^{-1}X,$$

where

$$(44.18) \quad f\left(\frac{d}{dx}\right) = \frac{d^n}{dx^n} + A \frac{d^{n-1}}{dx^{n-1}} + \dots + R \frac{d}{dx} + S,$$

A, ..., S constants and  $X=X(x)$ . Resolving (44.18) into factors -implicitly assumed as different- (44.16) is reduced to

$$(44.19) \quad \left[\frac{d}{dx} - a_1\right] \dots \left[\frac{d}{dx} - a_n\right]y = X$$

[1839a, 15, 19]. On similar lines an arbitrary finite difference equations was put in the form

$$(44.20) \quad F(D)u_x = (D - a_1)\dots(D - a_n)u_x = X,$$

where  $a_1, \dots, a_n$  constants,  $X=X(x)$  and  $D$  the forward difference operator given by (44.10) [1839b, 33-34].

According to the first method suggested, (44.19) gives, by successively performing the operations  $(d/dx - a_1)^{-1}$  on  $X$  via the theorem (44.14), the solution of (44.16) in the form

$$(44.21) \quad y = \sum_{i=1}^n \frac{e^{a_i x} \int e^{-a_i x} X dx}{(a_1 - a_i) \dots (a_{i-1} - a_i) (a_{i+1} - a_i) \dots (a_n - a_i)}$$

[1839a, 19-21]. The corresponding solution for the case of equal and imaginary roots was additionally provided in his Examples<sup>(6)</sup>. This method corresponds to Cauchy's theorem (17.51) [see also Petrova 1987, 8-10; 4.5]. On similar lines the finite difference equation (44.15) was solved. For example, for two distinct roots  $a_1, a_2$ , we have

$$(44.22) \quad u_x = \frac{a_1^{x-1}}{a_1 - a_2} \Sigma(X a_1^{-x}) + \frac{a_2^{x-1}}{a_2 - a_1} \Sigma(X a_2^{-x})$$

and in the case of  $n$  equal roots,  $a_1 = a_2 = \dots = a_n = a$ , the solution of (44.15) is given by

$$(44.23) \quad u_x = (D - a)^{-n} X + (D - a)^{-n} 0 = a^{x-n} \Sigma^n(X a^{-x}) + a^{x-n} (c + c_1 x + \dots + c_n x^n)$$

[1839b, 33-35].

An alternative method to (44.21) was additionally given in [1839a] for the solution of (44.16). Since  $f(d/dx)$  was written as

a product of terms of the form

$$(44.24) \quad \left[ \frac{d}{dx} - a \right]^n,$$

Gregory would expand the inverse of (44.24), either in an ascending series of powers of  $d/dx$ , as

$$(44.25) \quad \left[ \frac{d}{dx} \right]^{-n} \left[ 1 - \frac{a}{d/dx} \right]^{-n}$$

or, in a descending series via

$$(44.26) \quad (-1)^n \left[ a - \frac{d}{dx} \right]^{-n},$$

by means of the binomial theorem which formed the core of his methodology. These expressions were not given by him in this general manner but were implicitly applied in specific examples as below.

Let the equation

$$(44.27) \quad \frac{d^2y}{dx^2} - 4 \frac{dy}{dx} + 4y = x^2.$$

By separation of symbols and factorization of the form (44.18) we have instead equation

$$(44.28) \quad \left[ \frac{d}{dx} - 2 \right]^2 y = x^2$$

which, symbolically solved, gives readily

$$(44.29) \quad y = \left[ \frac{d}{dx} - 2 \right]^{-2} x^2 + \left[ \frac{d}{dx} - 2 \right]^{-2} 0.$$

Now, by (44.26) we have:

$$\left[ \frac{d}{dx} - 2 \right]^{-2} x^2 = \left[ 2 - \frac{d}{dx} \right]^{-2} x^2 = \left[ 2^{-2} + 2 \cdot 2^{-3} \frac{d}{dx} + 3 \cdot 2^{-4} \frac{d^2}{dx^2} + \dots \right] x^2 \text{ or,}$$

$$(44.30) \quad \left[ \frac{d}{dx} - 2 \right]^{-2} x^2 = \frac{x^2}{2^2} + \frac{4x}{2^3} + \frac{6}{2^4} + 0,$$

since from  $d^3/dx^3$  onwards the result is zero.

By (44.25) the second term in (44.29) is evaluated as follows:

$$\left[\frac{d}{dx} - 2\right]^{-2} 0 = \frac{d^{-2}}{dx^{-2}} \left[1 - 2 \frac{d^{-1}}{dx^{-1}}\right] 0 = \left[1 - 2 \frac{d^{-1}}{dx^{-1}}\right]^{-2} (cx + c_1) =$$

$$= c \left[ x + 2 \frac{2x^2}{1.2} + 3 \frac{2^2 x^3}{1.2.3} + \dots \right] + c_1 \left[ 1 + 2.2x + 3.2^2 \frac{2^2 x^2}{1.2} + \dots \right], \text{ or}$$

$$(44.31) \quad \left[\frac{d}{dx} - 2\right]^{-2} 0 = (c_1 + c_2 x) e^{2x},$$

if we put  $c + 2c_1 = c_2$ . Combining (44.30) with (44.31) the solution of the given equation (44.27) is by (44.29)

$$(44.32) \quad y = \frac{x^2}{2^2} + \frac{4x}{2^3} + \frac{6}{2^4} + (c_1 + c_2 x) e^{2x}$$

[1839a, 22; 1846, 293-294] (7).

We will now illustrate Gregory's first method by applying it to a mixed equation, namely to

$$(44.33) \quad \frac{d}{dx} \Delta y + a \frac{dy}{dx} + b \Delta y + aby = X.$$

Equation (44.33) is put in the form

$$(44.34) \quad \left[ \frac{d}{dx} + b \right] (\Delta + a) y = X.$$

We thus have by (44.14) that

$$(\Delta + a)y = e^{-bx} \int X e^{bx} dx. \text{ Now, since } \Delta + a = D - (1 - a), \text{ we finally have by}$$

$$(44.22) \text{ for } n=1 \text{ that}$$

$$(44.35) \quad y = (1-a)^{x-1} \sum \{ (1-a)^{-x} e^{-bx} \int X e^{bx} dx \} +$$

$$+ c_1 e^{-bx} + (1-a)^x \varphi(\sin 2\pi x, \cos 2\pi x).$$

The result will be of a different form evidently if we operate first by  $(\Delta + a)$  [1839b, 37].

Mentioning Greatheed briefly in [1839b, 38] for his use of the "characteristic"  $d/dy$  as "an independent quantity", Gregory acknowledged him as "the first to call the attention of mathematicians to the utility" of symbolical methods in the case of partial differential equations in [1839c, 62-63]. He incorporated a sketch of Greatheed's treatment of equation (42.3) [1839c, 69-70] but mentioned Fourier as his main inspiration for his own

work on partial differential equations [1839c,62; see Fourier's symbolic solutions (17.27)-(17.31)].

Let us illustrate Gregory's own technique with equation (42.9) or

$$(44.36) \quad x \frac{dz}{dx} + y \frac{dz}{dy} = nz.$$

In [1839c, 70] he applied Greatheed's method as followed from the general case (42.3). In his Examples [1841], instead of regarding  $y$  as a constant -and thus reduce (44.36) to an ordinary linear equation in  $x$ , as in (42.10)- he assumed

$$(44.37) \quad du = \frac{dx}{x}, \quad dv = \frac{dy}{y}$$

reducing thus (44.36) to an equation in two variables without coefficients

$$(44.38) \quad \left[ \frac{d}{du} + \frac{d}{dv} - n \right] z = 0.$$

Integrating (44.38) in respect to  $u$  we have

$$(44.39) \quad z = e^{-v \left[ \frac{d}{dv} - n \right]} \varphi(u) = e^{nv} \varphi(u - v).$$

Solving (44.37) for  $u$  and  $v$  respectively and substituting their values in (44.39), we have finally

$$(44.40) \quad z = x^n f\left(\frac{y}{x}\right),$$

where  $f$  an arbitrary function [1846, 361]. The result (44.40) is identical to Greatheed's (42.12), and the method is very similar, only far simpler.

Our final illustration will be from Gregory's paper "On the solution of certain functional equations" [1843b]. Opening his paper with Ellis's important remarks on the difference between functional and algebraic equations -as in the latter's [1843a]- Gregory went on to "enforce and illustrate" Ellis's views. Writing a linear functional equation in the form

$$(44.41) \quad \varphi(\omega^n x) + a_1 \varphi(\omega^{n-1} x) + \dots + a_n \varphi(x) = X,$$

where  $\omega^1(x)$ ,  $X$  given functions of  $x$ , and  $\varphi$  the unknown function, he claimed that (44.41) bears a close analogy with the general linear equation in finite differences, and hence could be solved by a similar process [1843b, 247, 250].

Letting  $\pi$  be an operator defined by

$$(44.42) \quad \pi\phi(x) = \phi(\omega x),$$

he showed that  $\pi$  is distributive, commutative with constants and obeys the index law [see (44.7)]. Hence, via (44.42), (44.41) becomes

$$(44.43) \quad \pi^n\phi(x) + a_1\pi^{n-1}\phi(x) + \dots + a_n\phi(x) = X$$

"which is no longer in a functional form, since the unknown operation  $\phi$  is the subject of known operations" [1843b, 250-251]. By separation of symbols and factorization (44.43) is reduced to

$$(44.44) \quad (\pi - r_1)\dots(\pi - r_n)\phi(x) = X,$$

where  $r_i$  the roots of the equation

$$(44.45) \quad z^n + a_1z^{n-1} + \dots + a_n = 0,$$

exactly as in the case of the linear differential equation (44.16) [1843b, 251].

If  $N_1, N_2, \dots, N_n$  are the coefficients of the partial fractions arising from the decomposition of

$$(44.46) \quad \frac{1}{(z-r_1)\dots(z-r_n)}$$

then, effecting the inverse operations on (44.44), we have

(44.47)  $\phi(x) = N_1(\pi-r_1)^{-1}X + \dots + N_n(\pi-r_n)^{-1}X + (\pi-r_1)^{-1}0 + \dots + (\pi-r_n)^{-1}0$  [1843b, 251]. This analysis shows that the solution of a certain class of functional equations may be reduced to the determination of "certain inverse operations". These inverse operations were illustrated by few examples where Gregory combined Laplace's known method and his own expansion of  $(\pi-r)^{-1}$  by means of the binomial theorem [1843b, 252-256].

Omitting altogether to mention Babbage, Gregory referred instead to Herschel drawing on his Examples [1820] where functional equations were treated by Laplace's method [for such instances see (24.19), (26.20) and (32.4)]. De Morgan was also omitted in Gregory's brief account. As we had mentioned in 3.9, English analysts of the 1840's and 1850's did not get involved with the solution of functional equations. An identical presentation of Gregory's account, as briefly sketched above, was incorporated by Boole in his textbook on finite differences [1860, 222-223] (e).

Gregory applied further his symbolical method, as in the case of (44.38), for the solution of simultaneous differential equations in his [1839d; see details in Koppelman 1971, 192]. We would like to add a brief note in connection with another paper

"On a difficulty in the theory of algebra" [1843a] in which Gregory came close to defining the associative law—pointed<sup>out</sup> by Hamilton a year later [see 1843a, 238-9; Clock 1964, 46-7].

Legendre was the first to discover this law in 1798.

We conclude our account with few comments on Gregory's Examples. In the realms of the chapter "Integration of differential equations by series", he reproduced several examples from Ellis's papers [1841a,b] such as the latter's treatment of (42.13) [or (43.15)] and (43.13); the former being the general form of the EFE [1846, 345-350]. In connection with partial differential equations he also provided symbolical solutions of the vibrating-string equation (14.13) [1846, 356], as well as of the heat-diffusion equation (17.16) [1846, 354-5]. In the end of his chapter on the "Evaluation of definite integrals", he referred to Laplace's method of expressing solutions of partial differential equations in definite integral form including Laplace's solution of the heat-diffusion equation (17.9), (17.12), and Poisson's treatment of the wave-equation (17.32) [1846, 502-504].

Gregory's work was criticised by J. Young in 1848; it was defended the following year by C. Graves and since then it was not any further doubted [Koppelman 1971, 193-4]. As we shall see in 4.5 it formed a most valuable background for Boole's early work in 1841 and a further motivation for his general method in [1844]. From the mid 1840's Gregory's work, surpassed by Boole's own, was not to be often referred to otherwise but as a rich source of collection of examples. I would like, however, to add Russell's acknowledgement of Gregory's contributions in 1857. In a brief historical review of the calculus of operations Russell wrote after commenting upon Lagrange's and Laplace's initial contributions:

Hence the principle of the separation of symbols of operation from those of quantity, first, according to Gregory, correctly enunciated by M. Servois. At this point Mr. Gregory's own investigations commence, which include the application of this principle to linear differential equations with constant coefficients, and some of the simpler differential equations with variable coefficients, and also the remarkable method of replacing constants contained in definite integrals by symbols of operation<sup>(9)</sup>.

By the last comment, Russell apparently referred to Gregory's rich collection of definite integrals in his Examples, where he had included their application in the symbolic solution of important equations in physics as mentioned above. In that chapter Gregory cited results from the work of Euler, Legendre, Liouville, Laplace, Fourier, Cauchy and Poisson, among others, on the evaluation of definite integrals. This subject is only tangentially touched upon in our thesis. However, it should be mentioned that Gregory's collection was very valuable, including a study of important integrals, such as

$$(44.48) \quad \int_0^{\infty} \frac{\cos ax}{1+x^2} dx \quad \text{and} \quad \int_0^{\infty} e^{-[x^2 + \frac{a^2}{x^2}]} dx$$

introduced by Laplace in 1772 and 1810 respectively [1846, 483-486]. These integrals were to play a vital role in the study of the connection between the Riccati and EFE and in the production of further symbolic solutions of these equations [see Glaisher 1872].

4.5 Boole on the calculus of operations: 1841-1845; an outline of his paper "On a general method in analysis" [1844].

G.Boole, a self-taught school master from Lincoln of Ireland, was like Bronwin a Cambridge outsider. Born in 1815, he had read Lacroix, Laplace and Lagrange before he was 20 years old. Comparing Lagrange's and Newton's work in mechanics he was led to a contemplation of the issues of abstraction and induction in mathematical discovery by 1835. He was soon initiated by Bromhead in the spirit of the Analytical Society and in 1839 he made Gregory's acquaintance. Gregory encouraged Boole to contribute his first papers in the Cambridge Mathematical Journal being a very helpful friend up to 1843. By that time Boole made another important acquaintance with De Morgan with whom he shared many common interests including logic<sup>(1)</sup>.

For Boole the first field of research was Lagrange's calculus of variations. In his first paper, and in fact the second to be published in Gregory's journal as [1841b], based upon Lagrange's general motion equation (12.6), he proved the principle of conservation of living forces (12.8) and that of least action (12.9) [1841b, 101-2]. Prior to these demonstrations he



laid down the laws according to which the symbols of the calculus of variations operate, reproducing certain of Lagrange's theorems in a concise form [1841b, 97-101]. Boole's first paper is particularly obscurely written. However, it is highly important not only in giving evidence of Boole's ingenuity and originality by improving over Lagrange's work [see 1.2], but also in incorporating in full Boole's remarkable tendency for abstraction and generalization, together with his skillful use<sup>of</sup> the principles of the calculus of operations apparently employed before his acquaintance with Gregory's work [see Laita 1977, 167-8; MacHale 1985, 49-50].

During the period 1841-1843 Boole contributed papers on analytical and linear transformations [1841a; 1842a,b], as well as on analytical geometry and definite integrals [1841b; 1843a,b]. The former papers contain the germs of what was later to be called 'invariant theory' [MacHale 1985, 54-5]. In the first he hinted at the dual role of interpretation of purely symbolical equations [1841a, 65; Laita 1977, 166]. The subject of definite integrals and linear transformations was to be reconsidered in his [1845a,b] and [1845c] respectively.

By 1845 Boole had gained a high estimation by his Cambridge contemporary intellectuals. His masterpiece was his paper "On a general method of analysis" [1844]; nearly rejected by the Royal Society, this lengthy paper was finally published in the Transactions of the Society [7.1, text and (9)]. This paper will form the core of our study in this and the next section. But before we proceed to its introduction and general outline, let us first comment upon Boole's paper "On the integration of linear differential equations with constant coefficients" [1841c] written after Gregory's pertinent paper [1839a].

Opening his paper with a reference to Gregory [1839a], Boole let a differential equation to be of the form (44.16), its solution given symbolically by (44.17) or

$$(45.1) \quad y = [f(\frac{d}{dx})]^{-1}X.$$

Letting next  $d/dx=z$ , Boole solved the equation

$$(45.2) \quad f(z) = 0$$

resolving  $[f(d/dx)]^{-1}$ , in the case of  $n$  distinct roots of (45.2), in  $n$  fractions

$$(45.3) \quad \left[ f\left(\frac{d}{dx}\right) \right]^{-1} = \frac{N_1}{\left(\frac{d}{dx} - a_1\right)} + \dots + \frac{N_n}{\left(\frac{d}{dx} - a_n\right)},$$

where

$$(45.4) \quad N_1 = \frac{1}{(a_1 - a_2) \dots (a_1 - a_n)},$$

$N_2, \dots, N_n$  being of similar form [1841c, 114-116].

Next, based upon Gregory's theorem (44.14), he deduced in a simple manner the latter's solution (44.21) in the form

$$(45.5) \quad y = N_1 e^{a_1 x} \int e^{-a_1 x} X dx + \dots$$

calling (45.5) "the simplest and most symmetrical form into which the solution of the equation [(44.16)] can be brought" [1841c, 116]. Boole justified his procedure claiming that his method of resolution "is independent of any properties of the variable [z in (45.2)] except the three laws [(44.7)] which have been shown by Mr. Gregory [...] to be common to the symbol  $d/dx$  and to the algebraical symbols generally supposed to represent numbers" [1841c, 115] (2).

There followed a detailed application of that method, first for the case of  $r$  equal roots of (45.2), and finally for the case of complex roots. In the former case we have

$$(45.6) \quad f(z) = (x - a)^r \varphi(z).$$

By application of the same theorem of resolution in partial fractions, the solution of (44.16) will be of the form

$$(45.7) \quad y = M_0 \left[ \frac{d}{dx} - a \right]^{-r} + \dots + M_{r-1} \left[ \frac{d}{dx} - a \right]^{-1} X + N_1 \left[ \frac{d}{dx} - a_1 \right]^{-1} X + \dots$$

where

$$(45.8) \quad M_\rho = \frac{1}{\rho!} \left[ \frac{d}{dz} \right]^\rho \left[ \varphi(z) \right]^{-1}, \quad \rho = 0, \dots, r-1$$

where  $z=a$  after the differentiation [1841c, 116-7] (3).

Boole illustrated his method by applying it to the equation

$$(45.9) \quad \frac{d^n y}{dx^n} - y = X,$$

concluding his brief study by showing that in combination with Gregory's theorem (44.15) this method is also valid for the solution of linear equations in differences with constant coefficients [1841c, 117-119]. Summing up, he wrote:

It is thus seen that every step in the solution of Differential Equations and equations of Finite Differences is reduced to the known theorems of ordinary Algebra, with the exception of the two theorems [(44.14)], and [(44.15)], which are necessary for passing to the interpretation of the expressions at which we arrive. This seems to be as great a simplification of the problem as the present state of mathematics admits of, for any further improvement must involve the invention of new processes for the treatment of ordinary algebraical expressions. With such we are not at present concerned; our object being to reduce the more complicated processes of the higher analysis to the simpler results which have been already obtained, and which may be looked on in the military phrase as bases for our further operations<sup>(4)</sup>.

Boole's method of resolution in partial fractions and his further study of equal and complex roots of (45.2) was incorporated in Gregory's Examples [see 4.4,(6)]. It was also mentioned briefly in Boole [1844,226] as a method "limited in its applications". The object of Boole's new paper was to supersede the stage of using the calculus of operations merely in order to "simplify the processes of analysis" [as in (4) above] and develop a method free from the restriction of commutativity. All the equations to be studied in that paper would be assumed in the form

$$(45.10) \quad f_0(n)u + f_1(n)pu + \dots = u$$

where  $u$  and  $v$  functions of  $x$ , and  $n$ ,  $p$  operative symbols subject to the laws

$$(45.11) \quad f(n)p^m u = p^m f(n+m)u, \quad f(n)p^m = f(m)p^m$$

[184, 226].

In his introduction Boole acknowledged Gregory's foundational study of the method of separation of symbols and his numerous illustrations of its utility. He went on to mention Servois [1814], Murphy [1837] and De Morgan "in connection with the history of this branch of analysis". He assumed for granted both the principles of the calculus of operations and certain theorems established by these mathematicians, focusing next on Gregory's three laws (44.7). It is as a result of these laws, he claimed, that we can cast Taylor's theorem in the symbolical form (44.2) [1844, 225-6]. However, he omitted to make any specific reference whatsoever from the work of analysts mentioned above taking, ap-

parently, for granted Murphy's detailed study of inverse and non-commutative operations<sup>(5)</sup>.

Interested in providing a method for the solution of equations with variable coefficients, Boole mentioned that the method of series and generating functions, as introduced by Euler and Laplace [1.4,1.5,1.7] was the only general theory existing so far for this purpose. He displayed the advantages that his own method would have over the series method; the main one was that this new method was based upon certain general theorems which are independent of the forms of  $f_1(n)$  in (45.10) [1844, 227-8].

The core of Boole's method consists in brief of three components. First, his "general theorem" of development of  $f(n+p)$  and its numerous consequences for specific values of  $n$  and  $p$ ; next, his symbolical method for the solution of differential equations with variable coefficients -which depends upon some of the lemmas that follow the "general theorem". Finally, due to his "fundamental theorem of development" any problems concerning finite difference equations (and, hence, instances from the theory of series, generating functions or definite integrals) are always reduced to the solution of a differential equation according to the symbolical method put forward.

A detailed illustration of Boole's method for the solution of differential equations with variable coefficients will be given in 4.6 through the EFE. In what follows below we will give an outline of this little known paper<sup>(6)</sup> omitting details in demonstrations. For convenience we have divided our study in 8 stages following Boole's order of exposition:

Stage 1 : The general theorem of development of  $f(n+p)$ .

Stage 2 : The fundamental theorem.

Stage 3 : Solution of differential equations by series.

Stage 4 : Solution of differential equations in finite terms:  
 $u + \varphi(D)e^{\theta u} = V \quad (i).$

Stage 5 : A generalization of the study of (i).

Stage 6 : Summation of series - generating functions.

Stage 7 : Evaluation of definite integrals.

Stage 8 : The theory of finite difference equations<sup>(7)</sup>.

Stages 1, 2 and 4 are those which deserve the outmost notice; particular stage 4 will be carefully examined in 4.6.

Stage 1 : [1844, 228-232]. Boole's "general theorem" is as follows:

The development of  $f(\pi+\rho)$  in ascending powers of  $\rho$  is  $\sum f_m(\pi)\rho^m$  (i), where

$$(45.12) \quad f_m(\pi) = \frac{(\lambda-1)f_{m-1}(\pi)}{(\lambda^m-1)\pi}, \quad f_0(\pi) = f(\pi),$$

$\pi, \rho$  distributive operations subject to

$\rho f(\pi)u = \lambda f((\pi)\rho u)$  (ii),  $\lambda f(\pi) = f(\varphi(\pi))$  (iii),

$\lambda$  a functional symbol.

If  $\lambda f(\pi)$  in (ii)-(iii) stands for  $f(\pi+\Delta\pi)$  (iv), then (i) in (45.12) will accordingly give the formula

$$(45.13) \quad f(\pi + \rho) = f(\pi) + \frac{\Delta}{\Delta\pi} f(\pi)\rho + \frac{\Delta^2}{\Delta\pi^2} f(\pi) \frac{\rho^2}{1.2} + \dots$$

where

$$(45.14) \quad \frac{\Delta}{\Delta\pi} f(\pi) = \frac{f(\pi+\Delta\pi)-f(\pi)}{\Delta\pi}.$$

If  $\Delta\pi$  vanishes,  $\pi$  and  $\rho$  are commutative [see (ii) and (iv)] and (45.13) is reduced to Taylor's theorem. [Next followed three propositions by means of which it was readily proved that if  $\pi, \rho$  are given by

$$(45.15) \quad \pi = \frac{x e^{r \frac{d}{dx}} - x}{r}, \quad \rho = x e^{r \frac{d}{dx}}$$

then, these two operators combine according to the rules (45.11). The demonstration of (45.11) was based upon Taylor's theorem. The last general formula derived was

$$(45.16) \quad \pi(\pi-1)\dots(\pi-n+1)u = x(x+r)\dots(x+(n-1)r \left(\frac{\Delta}{\Delta x}\right)^n u$$

where  $\Delta x=r$  and  $\pi, \rho$  given by (45.15). In fact from (45.15) it is seen that

$$(45.17) \quad \pi = \frac{\rho-x}{r}$$

which served as the basis for (45.16).

Now, if we put

$$(45.18) \quad r = 0, \quad \pi = x \frac{d}{dx}, \quad \rho = x, \quad x = e^\theta, \quad D = \frac{d}{d\theta}$$

we obtain readily from (45.11) and (45.16) respectively the "known" formulae

$$(45.19) \quad f(D)e^{m\theta}u = e^{m\theta}f(D+m)u, \quad f(D)e^{m\theta} = f(m)e^{m\theta},$$

$$(45.20) \quad D(D-1) \dots (D-n+1)u = x^n \left( \frac{d}{dx} \right)^n u.$$

Boole's theorem (45.12) is a remarkable generalization and abstraction over the restricted inquiries of Cauchy, Murphy and Gregory in the realms of the calculus of operations. Through one single theorem he deduced first a generalized version of Taylor's theorem (45.13) useful in his theory of stages 5 and 8, and the known theorems (45.19)-(45.20) [see (33.54), (33.57), (44.14)] useful for the rest of his methodology. This general theorem was reproduced ONLY in Boole [1845d, 215-217; 1859 or 1877, 447-9] (e). In connection with (45.19)-(45.20) Boole wrote: "With a view to the maintenance of an unbroken analogy, it has, however, been thought better to deduce them from the properties of the more general system in  $\pi$  and  $p$ , than to assume them as already proved" [1844, 232].

Stage 2: [1844, 232-233]. Boole assumed a linear differential equation with variable coefficients in the form

$$(45.21) \quad (a + bx + cx^2 + \dots) \frac{d^n u}{dx^n} + (a' + b'x + \dots) \frac{d^{n-1} u}{dx^{n-1}} + \dots = X,$$

where  $a, b, \dots$  constants and  $X=X(x)$ . If we let

$$(45.22) \quad x = e^\theta, \quad D = d/d\theta,$$

then, by multiplication with  $x^n$ , (45.21) can easily be reduced to the "symbolical" form

$$(45.23) \quad f_0(D)u + f_1(D)e^\theta u + f_2(D)e^{2\theta} u + \dots = U,$$

where  $f_i(D)$  are polynomials of  $D$  and  $U=U(e^\theta)$ . If we consider further that

$$(45.24) \quad u = \sum u_m e^{m\theta},$$

then, observing that

$$(45.25) \quad f_i(D)e^{m\theta} = f_i(m)e^{m\theta} \quad i = 0, 1, \dots$$

according to (45.19), we have by substitution in the left hand side of (45.23) the relation

$$(45.26) \quad f_0(D)u + f_1(D)e^\theta u + \dots = \sum [(f_0(m)u_m + f_1(m)u_{m-1} + \dots)e^{m\theta}].$$

We thus see that the relation between the first member of (45.26) and the differential equation (45.23), "is the same as is borne by the coefficient of  $e^{m\theta}$  in the second member [of (45.26)] to

the linear equation of finite differences". Theorem (45.26) was called by Boole "a particular form of the fundamental theorem of development", cited hereafter as FTD<sup>(9)</sup>.

Stage 3: [1844, 234-244]. Boole claimed that every linear differential equation of the form (45.23), where  $U=0$ , is satisfied by the series (45.24) provided that

$$(45.27) \quad f_0(m)u_m + f_1(m)u_{m-1} + \dots = 0.$$

This is an evident consequence of the FTD (45.26). If  $p$  be the lowest value of  $m$ , then from (45.27) we have that  $f_0(p)=0$ , hence the values of  $p$  are determined. If  $p$  have  $n$  real values, then there will be  $n$  ascending developments of the form

$$(45.28) \quad u = \sum_{\mathbb{P}} u_m e^{m\theta}, \quad e^\theta = x,$$

$u_p$  being arbitrary. He included a rule for the case of equal or imaginary values of  $p$  [1844, 234-6]<sup>(10)</sup>. Followed an illustration of each case via examples and a brief account of this application of this theory to partial differential equations.

Let us illustrate Boole's series method by applying it to the differential equation

$$(45.29) \quad \frac{d^2u}{dx^2} - \frac{a-1}{x} \frac{du}{dx} - n^2u = 0.$$

Then, putting  $x=e^\theta$  and letting  $D=d/d\theta$ , the symbolical form of (45.29) is

$$(45.30) \quad D(D-a)u - n^2e^{a\theta}u = 0.$$

Now  $f_0(m)=m(m-a)=0 \rightarrow m=0$  or  $m=a$ . The relation between the coefficients  $u_m$  of (45.28) is given accordingly by

$$(45.31) \quad u_m = \frac{n^2}{m(m-a)} u_{m-2}.$$

We have thus two particular solutions of (45.29) which combined give the general solution as a sum of two ascending series of  $x$

$$(45.32) \quad u = C + \frac{Cn^2}{2(2-a)}x^2 + \frac{Cn^4}{2 \cdot 4(2-a)(4-a)}x^4 + \dots + C'x^a + \frac{C'n^2x^{a+2}}{2(a+2)} + \dots,$$

the first series starting from  $p=0$ , the second from  $p=a$  [ $e^\theta$  in (42.28) being replaced in the final result by  $x$ ; 1877, 438]<sup>(11)</sup>.

Stage 4: [1844, 244-257]. Under the title "On the integration of linear differential equations in finite terms" what Boole does in fact is a thorough study of the solution of equations which can be cast to the binomial symbolic form

$$(45.33) \quad u + \varphi(D)e^{\theta}u = U.$$

Despite the apparent restriction of the class of equations solvable by this method, still a wide range of second order differential equations important for their role in physics are included in the class under consideration.

First he shows that by division with  $f_0(D)$ , every equation of the form (45.23) can be reduced to one of similar form without coefficient in its first terms, that is to

$$(45.34) \quad u + \varphi_1(D)e^{\theta}u + \dots + \varphi_n(D)e^{n\theta}u = U$$

[1844, 244-5]. He regards next the class

$$(45.35) \quad u + a_1\varphi(D)e^{\theta}u + \dots + a_n\varphi(D)\dots\varphi(D-n+1)e^{n\theta}u = U.$$

Observing that

$$(45.36) \quad \varphi(D)\dots\varphi(D-n+1)e^{n\theta}u = [\varphi(D)e^{\theta}]^nu$$

according to (45.19) and putting

$$(45.37) \quad \varphi(D)e^{\theta} = \rho,$$

by substitution, (45.27) gives by solution

$$(45.38) \quad u = (1 + a_1\rho + \dots + a_n\rho^n)^{-1}U.$$

By resolution of the second side of (45.38) in partial fractions -as in the case of (45.3)-(45.4)- the value of  $u$  is to be readily deduced by

$$(45.39) \quad u = \sum_{i=1}^h N_i u_i$$

where  $u_i$  to be determined by the equation

$$(45.40) \quad u_i - q_i\varphi(D)e^{\theta}u_i = U$$

where  $q_i$  the roots of (45.2) [1844,246-7; 1877,415-416].

Which are the forms of  $\varphi(D)$  that render (45.40) or (45.35) integrable? The simplest case is

$$(45.41) \quad \varphi(D) = \frac{a_1D+b_1}{aD+b}.$$

Boole illustrated his method of reduction of (45.35) to (45.40) by an equation which gives  $\varphi(D)=(D+a)/(D+b)$ . In this case [(45.41)] equations (45.40) are readily reduced to first-order ones. For the particular form of  $\varphi(D)=D^{-1}$ , we have under consideration a linear equation with constant coefficients [1844, 246-7; 1877,416-418]. Postponing Boole's study of equation (45.40) for 4.6, we would like to stress that he searched for those equations whose solution depends upon that of

$$(45.42) \quad \frac{d^nu}{dx^n} \pm q^nu = X.$$



Evidently equations such as the EFE in either forms (13.32) and (13.33) belong to this latter category [1844, 247-57; 1877, 418-429].

Stage 5: [1844, 257-261]. Here Boole investigates a more general class than (45.35), namely those equations whose symbolical form is

$$(45.43) \quad f_0(D)u + f_1(D)\varphi(D)e^{\theta}u + f_2(D)\varphi(D)\varphi(D-1)e^{2\theta}u + \dots = U.$$

By putting

$$(45.44) \quad D - n\varphi(D)e^{\theta} = \pi \quad \text{and} \quad \varphi(D)e^{\theta} = \rho,$$

we have that

$$(45.45) \quad \rho f(\pi)u = f(\pi-1)\rho u, \quad D = \pi + n\rho.$$

Equation (45.43) is then successively reduced first to

$$(45.46) \quad f_0(D)u + f_1(D)\rho u + \dots = U,$$

and next, by means of theorem (45.13) to an equation of the form

$$(45.47) \quad \varphi_0(\pi)u + \varphi_1(\pi)\rho u + \dots = U.$$

He investigated the principal integral cases of the class (45.43) by means of a specific second order equation. The theory involved is far too abstract and general and apparently of no further applications<sup>(12)</sup>.

Stage 6: [1844, 261-170] is devoted to the "Theory of Series and Generating Functions". Assume first that we are given a series of the form (45.28), and let the law of the generation of its coefficients be

$$(45.48) \quad u_m + \varphi_1(m)u_{m-1} + \dots + \varphi_n(m)u_{m-n} = 0.$$

Following exactly the converse procedure of that followed in stage 3, the problem of summation is reduced to the solution of the differential equation which corresponds to (45.48) according to FTD (45.26). Let for example the series

$$(45.49) \quad u = 1 - \frac{n^2}{1.2}x^2 + \frac{n^2(n^2-2^2)}{1.2.3.4} - \dots$$

where

$$(45.50) \quad u_m = - \frac{m^2 - (m-2)^2}{m(m-1)} u_{m-1}.$$

Formula (45.50) corresponds to (45.48) or (45.27). The related differential equation will hence be

$$(45.51) \quad D(D-1)u - [(D-2)^2 - n^2]e^{2\theta}u = 0,$$

a form that corresponds to (45.23) for  $U=0$  by the FTD (45.26). The solution of (45.51), or of

$$(45.52) \quad (1 - x^2) \frac{d^2u}{dx^2} - x \frac{du}{dx} + n^2u = 0$$

will give the required form of series  $u$  [1844, 262; 1877, 442]<sup>(13)</sup>.

On fairly similar lines, given the relation between the coefficients  $u_m$ , the determination of the generating function of  $u_m$ ,  $\sum u_m x^m = u$ , is reduced, via the FTD, to the solution of the corresponding differential equation. Two examples served so as to illustrate this method [1844, 266-8] and next followed a brief study of the theory of generating functions as connected with equations of partial differences [1844, 268-270].

Stage 7: [1844, 270-275] is devoted to an application of the theory of series to the evaluation of definite integrals. The first example concerned the evaluation of

$$(45.53) \quad \int_0^n \frac{dx \cos rx}{(1-2v \cos x + v^2)^n}.$$

The first step amounts to the expansion of  $(1-2v \cos x + v^2)^{-n}$  in a series of the form

$$(45.54) \quad A_0 + 2(A_1 \cos \omega + A_2 \cos 2\omega + \dots),$$

$A_r$  required. By analytical computations, if we let  $v^2 = t$ , we find

$$(45.55) \quad A_r = t^{\frac{r}{2}} \sum_{m=0}^{\infty} (u_m t^m),$$

where

$$(45.56) \quad u_m - \frac{(n+m-1)(n+r+m-1)}{m(m+r)} u_{m-1} = 0.$$

Thus, if

$$(45.57) \quad \sum u_m t^m = u, \quad t = e^\theta,$$

we arrive by means of (45.56) to a differential equation in  $u$  as in the case of (45.50)-(45.52). Solved by the theory introduced at stage 4, this equation gives the solution

$$(45.58) \quad u = t^{-r} \left( \frac{d}{dt} \right)^{n-1} \frac{t^{n+r-1}}{\Gamma(n)(1-t)^n}$$

[1844, 270-2; 1877, 444-5]<sup>(14)</sup>.

Combining next this result with (45.55), we have the coefficient  $A_r$  of (45.54) determined by

$$(45.59) \quad A_r = \frac{1}{\Gamma(n)t^{r/2}} \left( \frac{d}{dt} \right)^{n-1} \frac{t^{r+n-1}}{(1-t)^n}$$

and hence the value of (45.53) was found to be  $nA_r$ .

Boole mentioned that Legendre "I believe, considered the above definite integral, but I am not acquainted with the result of his analysis" [1844, 272]. In the next example he considered a multiple integral connected with the theory of the attractions of ellipsoids, mentioning Green as the one that had considered a particular case of it [1844, 273]. The last example concerned another known multiple integral, its general theory being fully developed by Dirichlet and Cayley. Ellis was also mentioned in connection with a theorem he had proved by means of Fourier's transforms [1844, 274-5].

Stage 8: [1844, 276-282] involved the theory of finite difference equations, and hence the results (45.13), (45.19)-(45.20) of the "general theorem" (45.12). Boole showed how every linear equation

$$(45.60) \quad X_0 u_x + \dots + X_n u_{x-n} = X$$

may be cast to the form

$$(45.61) \quad f_0(n)u_x + f_1(n)\rho u_x + \dots = U_x,$$

where  $n$  and  $\rho$  are given by (45.15) and which satisfy the laws (45.11) [1844, 276-7]. He then studied first the solution of (45.61) in  $r+1$  terms, for  $U_x=0$ , as integrated by the series method given at stage 3 via one example. On this subject he referred to Bronwin [1843c; 1844, 278; see also 4.3].

Boole's final step in this last stage of his inquiries was the solution of (45.61) in finite terms. In so doing he made use once more of the formula (45.15) in a more general form given in [1844, 230]. Now  $\rho$  is given by

$$(45.62) \quad \rho = \varphi(x) e^{r \frac{d}{dx}} \quad \text{and } n = \frac{np-x}{r}$$

for  $r=-1$ . He was thus led to a study of (45.61) in  $\varphi(n)$  [1844, 279]. He distinguished several cases in the course of which he made use of the method exhibited in stage 4 [see 4.6]. Omitting the details we would like to conclude with Boole's remarks on Gregory:

Late fellow of Trinite College, Cambridge, and author of the well-known "Examples". Few in so short a life have done so much for science. The high sense which I entertain of his merits as a mathematician, is mingled with feelings of gratitude for much valuable assistance rendered to me in my earlier essays<sup>(15)</sup>.

#### 4.6 Boole's symbolical solution of binomial equations in finite form [1844]; the earth-figure equation.

In 4.5 we gave a general outline of Boole's lengthy paper "On a general method in analysis" [1844] dividing its study for convenience in 8 distinct stages. The most essential components of his methodology consist of his general theorem (45.12), the fundamental theorem (45.26) -cited as FTD- and lastly his treatment of binomial equations (45.40), or,

$$(46.1) \quad u + \varphi(D)e^{x\theta}u = U.$$

In this section we focus on (46.1), the theorems involved in its study and their application to the EFE in both the "standard" and "general" form [4.2.(7)], concluding with a connection between Boole's inquiries on the EFE and those of his predecessors. As in 4.5, references are provided from Boole [1844] as well as from Boole [1859] cited as [1877; 4.5.(4),(5)].

For convenience let us first summarize the results of our study in 4.5 which will be needed in our present discussion. From the general theorem (45.12) we only need the known formulae (45.19)-(45.20), or

$$(46.2) \quad f(D)e^{m\theta}u = e^{m\theta}f(D+m)u, \quad f(D)e^{m\theta} = f(m)e^{m\theta},$$

and

$$(46.3) \quad D(D-1)\dots(D-n+1)u = x^n \left( \frac{d}{dx} \right)^n u,$$

where

$$(46.4) \quad D = d/d\theta = xd/dx, \quad x = e^\theta.$$

We also need to remember that every linear differential equation in  $x$  can be reduced by means of the transforms (46.4) and theorem (46.2) to one in symbolical form in  $D$  [see (45.21)-(45.23)]. Further, every equation of the symbolical form (45.23) can be easily reduced to one of similar form without a coefficient in its first term as (45.34). Finally, focusing on the class (45.35), or,

$$(46.5) \quad u + a_1\varphi(D)e^\theta u + \dots + a_n\varphi(D)\dots\varphi(D-n+1)e^{n\theta}u = U,$$

we showed that it can be resolved to a system of binomial equations (46.1) [see (45.35)-(45.40)].

Now, Boole's treatment of (46.1) consists of two distinct theorems. The first, cited hereafter as theorem 1, claims that (46.1) can be converted into

$$(46.6) \quad v + \varphi(D+n)e^{x\theta}v = V$$

by means of the transforms

$$(46.7) \quad u = e^{n\theta}v, \quad U = e^{n\theta}V.$$

Indeed, assuming  $u=e^{n\theta}v$ , after substitution in (46.1), consideration of (46.2) and division by  $e^{n\theta}$ , we obtain immediately (46.6) with  $V=e^{-n\theta}U$ . Thus, given  $\varphi$  and  $U$ , (46.1) is converted into (46.6) if we have (46.7). Or, if  $v$  can be determined from (46.6), then the solution of the given equation (46.1) is provided from (46.7) [1844, 247]. Boole stated in [1877, 418] that "in any binomial equation we can convert  $\varphi(D)$  into  $\varphi(D+n)$ ,  $n$  being any constant".

Theorem 2 establishes the transform of (46.1) into

$$(46.8) \quad v + \psi(D)e^{r\theta}v = V,$$

where  $\psi$  is given, by means of

$$(46.9) \quad u = P_r \frac{\varphi(D)}{\psi(D)} v, \quad U = P_r \frac{\varphi(D)}{\psi(D)} V,$$

where

$$(46.10) \quad P_r \frac{\varphi(D)}{\psi(D)} = \frac{\varphi(D)\varphi(D-r)\varphi(D-2r)\dots}{\psi(D)\psi(D-r)\psi(D-2r)\dots},$$

an "infinite product" [1844, 247; 1877, 419] <sup>(1)</sup>.

The proof of theorem 2 runs as follows. Let

$$(46.11) \quad u = f(D)v.$$

$f$  to be determined. Substituting (46.11) in (46.1), observing that  $e^{r\theta}f(D)v=f(D-r)e^{r\theta}v$  by (46.2) and dividing by  $f(D)$ , we have

$$(46.12) \quad v + \frac{\varphi(D)f(D-r)}{f(D)} e^{r\theta}v = [f(D)]^{-1}U.$$

Comparing (46.12) with the form (46.8) we have evidently a recursive definition of  $f(D)$  by

$$(46.13) \quad f(D) = \frac{\varphi(D)}{\psi(D)} f(D-r).$$

By iteration it follows from (46.13) that  $f(D)$  coincides with the right-hand side of (46.10) and thus, from (46.11)-(46.12), that (46.1) is converted into (46.8) by means of (46.9) [1844, 248; 1877, 419].

Boole observed that <sup>the</sup> form  $u$  to be determined by (46.9),  $\psi(D)$  in (46.8) has to be chosen so as to have the product (46.10) in finite form and also so that equation (46.8) is integrable. For the former it suffices that for every factor  $g(D)$  in the numerator of (46.10) there exists a factor  $g(D\pm ir)$  in the denominator,  $i$  any integer or 0, and vice versa. He called (46.8)

the "transformed" equation and (46.9) the "auxiliary" ones, mentioning that by theorem 2, the given equation is resolved in three equations,  $V, v$  and  $u$  being successively determined. He concluded his theoretical account by giving the "canons which regulate the determination of the constants" [1844, 248-9; 1877, 420-1] (2).

Boole applied these two theorems to several examples. The first of them concerned the determination of the characteristic of those differential equations of the  $n^{\text{th}}$  degree, whose solution depends on that of equation

$$(46.14) \quad \frac{d^nv}{dx^n} \pm q^n v = X,$$

$q$  a constant and  $X=X(x)$ . This is Boole's most important example, for it is implied from the next two examples concerning the EFE which were based on it, that the basic theorem 2 was invented after contemplating on this particular example. Thus, before proceeding to the EFE, it is worthwhile giving a sketch of Boole's theoretical reasoning for (46.14). The treatment which follows below makes indispensable use of both theorem 1 and 2.

Boole obtained first the symbolical form of (46.14),

$$(46.15) \quad v \pm \frac{q^n}{D(D-1)\dots(D-n+1)} e^{n\theta} v = V,$$

$V$  given by

$$(46.16) \quad V = \left[ \frac{d}{dx} \right]^{-1} X.$$

As in the case of the FTD [see (45.21)-(45.23)] this was apparently effected by multiplying (46.14) with  $x^n$ , by the formulae (46.2)-(46.3) and, finally, by division with  $(d/dx)^n$ . The class of equations sought should, on assuming  $x=e^\theta$  [see (46.4)], be reducible to a form similar to that of equation (46.15). In other words, regarding (46.15) as equation (46.8) of theorem 2, the initial equation (46.1) sought should be

$$(46.17) \quad u \pm \frac{q^n}{(D+a_1)\dots(D+a_n)} e^{n\theta} u = U,$$

the conditions concerning the quantities  $a_i$  wanted [1844, 249-250; 1877, 423-4].

Boole put

$$(46.18) \quad u = e^{-a_1\theta} u_1$$

converting thus, according to theorem 1, (46.17) into

$$(46.19) \quad u_1 \pm \frac{q^n}{D(D+a_2-a_1)\dots(D+a_n-a_1)} e^{na}u_1 = e^{a_1}U.$$

Now, regarding as our initial equation (46.1) equation (46.19), we notice that the first factor in the denominator of  $\varphi(D)$  in (46.19) corresponds with the first factor of the denominator of  $\psi(D)$  in (46.15)<sup>(3)</sup>. By the same theorem, in any of the remaining factors  $(D+a_i)$  we can convert  $D$  into  $D \pm i$ ,  $i$  any integer. Hence, the factors in  $\varphi(D)$  will correspond with those of  $\psi(D)$  if the quantities

$$(46.20) \quad \frac{a_1-a_1+i-1}{n}, \quad i = 2, \dots, n$$

are all negative integers<sup>(4)</sup>.

Having determined the characteristic of those equations (46.19), or (46.17), whose solution depends upon that of (46.15), or (46.14), it remains to sketch the procedure of solution of (46.17). Based on theorem 2 Boole showed how the value of  $u$  can be deduced from the known value of  $v$  by "differentiation" only. Letting  $\varphi(D)$ ,  $\psi(D)$  stand for the coefficients of  $e^{na}u_1$ ,  $e^{na}v$  in (46.19) and (46.15) respectively, we have according to (46.9) of theorem 2 that

$$(46.21) \quad u_1 = P_n \frac{\varphi(D)}{\psi(D)} v.$$

Thus, by (46.18), the value of  $u$  sought will be deduced from

$$(46.22) \quad u = e^{-a_1} P_n \frac{(D-1)\dots(D-n+1)}{(D+a_2-a_1)\dots(D+a_n-a_1)} v.$$

Since by (46.20)  $a_2-a_1 < -1$ , we have by (46.10) that

$$(46.23) \quad P_n \frac{D-1}{D+a_2-a_1} = (D-1)(D-1-n)(D-1-2n)\dots(D+a_2-a_1+n).$$

For by the assumption (46.20) for  $i=2$  we have [see (4) above] that

$$(46.24) \quad a_2 - a_1 = -n\mu - 1, \quad \mu > 0.$$

Thus, from the factor  $(D-1-n\mu)$  onwards, all the factors in the numerators of the left-hand side of (46.23) equal to the factors  $(D+a_2-a_1)$ ,  $(D+a_2-a_1-n)\dots$  in the denominator. So, the product (46.23) terminates, with last factor the one preceeding  $(D-1-n\mu) = (D+a_2-a_1)$ , which is  $(D+a_2-a_1+n)$ <sup>(5)</sup>. The same holding for the

other products in (46.22) we have that  $u$  is deduced from  $v$ , in (46.22) by differentiation only [1844, 250; 1877, 424].

Consider now the EFE in the standard form

$$(46.25) \quad \frac{d^2 u}{dx^2} + q^2 u = \frac{6u}{x^2},$$

with its symbolical form obtained readily as

$$(46.26) \quad u + \frac{q^2}{(D+2)(D-3)} e^{2\theta} u = 0.$$

Equation (46.26) is of the binomial form (46.1), but, more specifically of the form (46.17) if we make in the latter  $n=2$ ,  $U=0$  and  $a_1=2$ ,  $a_2=-3$ . Thus, equation (46.26) can either be treated directly by theorem 2, letting as the transformed equation (46.8) be

$$(46.27) \quad v + \frac{q^2}{D(D-1)} e^{2\theta} v = V,$$

or, by both theorems 1, 2 as used in the former example; in other words by (46.21)-(46.22). We will first use the second method which is more convenient, and next the first.

According to the latter method the solution of (46.25) is based directly on that of (46.14) for  $n=2$ ,  $X=0$ ,

$$(46.28) \quad \frac{d^2 v}{dx^2} + q^2 v = 0,$$

which equals

$$(46.29) \quad v = c \sin(qx + c_1).$$

Thus, by (46.22) we deduce the value of  $u$  by differentiation after substituting (46.29) for  $v$  and  $xd/dx$  for  $D$  as follows:

$$u = e^{2\theta} p_2 \frac{D-1}{D-5} v = e^{-2\theta} (D-1)(D-3)v =$$

$$= \frac{1}{x^2} \cdot c \left( x^2 \frac{d^2}{dx^2} - 3x \frac{d}{dx} + 3 \right) \sin(qx + c_1), \text{ or,}$$

$$(46.30) \quad u = c \left[ \left( \frac{3}{x^2} - q^2 \right) \sin(qx + c_1) - \frac{3q}{x} \cos(qx + c_1) \right],$$

which is the known solution of the EFE in the standard form (46.25) [1844, 251] (6).

By the former method (46.26) and (46.27) stand respectively



for the initial and the transformed equation (46.1), (46.8) of theorem 2. Thus, given  $\varphi(D)$  and  $\psi(D)$  we have by (46.10):

$$P_2 \frac{\varphi(D)}{\psi(D)} = P_2 \frac{\frac{q^2}{(D+2)(D-3)}}{\frac{q^2}{D(D-1)}} = P_2 \frac{D(D-1)}{(D+2)(D-3)} =$$

$$= \frac{D(D-2)(D-4) \dots (D-1)(D-3)(D-5) \dots}{(D+2) \cdot D \cdot (D-2)(D-4) \dots (D-3)(D-5) \dots} . \text{ Thus}$$

$$(46.31) \quad P_2 \frac{\varphi(D)}{\psi(D)} = \frac{D-1}{D+2} .$$

Therefore, by (46.9) we have

$$(46.32) \quad u = \frac{D-1}{D+2} v , \quad 0 = \frac{D-1}{D+2} V .$$

Boole claimed that  $V=0$ ; thus  $v$  is given by (46.29). Hence,  $u$  is determined as follows:

$$u = \frac{D-1}{D+2} v = (1-3(D+2)^{-1}) \cdot c \sin(qx+c_1) = c[1-3e^{-2\theta D-1}e^{2\theta}] \sin(qx+c_1)$$

$$[\text{by (46.2)}] = c[\sin(qx+c_1) - \frac{3}{x^2} \int \sin(qx+c_1) dx] ;$$

so  $u$  is found to be (46.30) [1844, 251; 1877, 423] (?).

In the third example provided by Boole in his [1844] we have the equation

$$(46.33) \quad \frac{d^2 u}{dx^2} - \frac{i(i+1)}{x^2} \pm h^2 u = 0 ,$$

$i$  a positive integer. If we take the positive case, then (46.33) is no other than the EFE in its "general" form<sup>(a)</sup>. By multiplication with  $x^2$  and the transform (46.2), equation (46.33) is reduced accordingly to the form

$$(46.34) \quad u \pm \frac{h^2}{(D-i)(D-i-1)} e^{2\theta} u = 0 .$$

Following the method employed in the first example, (46.34) stands for (46.17) where now  $a_1=i$ ,  $a_2=-i-1$ ,  $n=2$ ,  $u=0$  and  $q=h$ . Hence, by (46.22) we have

$$(46.35) \quad u = e^{-1\theta} P_2 \frac{D-1}{D-2i-1} v,$$

where  $v$  is determined again by (46.14) for  $n=2$  and  $X=0$ . Since

$$(46.36) \quad P_2 \frac{D-1}{D-2i-1} = (D-1)(D-3) \dots (D-2i+1)$$

[by the procedure followed for (46.31) above,] we have that  $u$  is determined via (46.35)-(46.36) by the relation

$$(46.37) \quad u = e^{-1\theta} (D-1)(D-3) \dots (D-2i+1)v$$

[1844, 251-2; 1877, 424-5].

The solution of (46.34), (46.37), can be written in a more convenient symbolical form if we replace  $D$  by  $xd/dx$  and  $e^\theta$  by  $x$  after writing

$$(46.38) \quad D-2i+1 = e^{2i\theta} D e^{-(2i-1)\theta}, \quad i=1, \dots, i \quad [\text{sic}]$$

in (46.37) according to property (46.2). We thus have from (46.37) in detail:

$$\begin{aligned} u &= \frac{1}{x^1} \left( x^2 \frac{d}{dx} \right) \frac{1}{x} \dots \left( x^{2i} \frac{d}{dx} \right) x^{-(2i-1)} v = \\ &= \frac{1}{x^{i+1}} x \left( x^2 \frac{d}{dx} \right) \frac{1}{x} \left( x^4 \frac{d}{dx} \right) \frac{1}{x^3} \dots \left( x^{-2i+3} x^{2i} \frac{d}{dx} \right) x^{-(2i-1)} v = \\ &= \frac{1}{x^{i+1}} \left( x^3 \frac{d}{dx} \right) \left( x^3 \frac{d}{dx} \right) \dots \left( x^3 \frac{d}{dx} \right) \frac{v}{x^{2i-1}}, \text{ or} \\ (46.39) \quad u &= \frac{1}{x^{i+1}} \left( x^3 \frac{d}{dx} \right)^i \frac{v}{x^{2i-1}}. \end{aligned}$$

Thus, if we take the positive case of (46.33), its solution is given by (46.39) in the form

$$(46.40) \quad u = \frac{1}{x^{i+1}} \left( x^3 \frac{d}{dx} \right)^i \frac{c \cos(hx) + c_1 \sin(hx)}{x^{2i-1}}.$$

Accordingly for the negative case we have

$$(46.41) \quad u = \frac{1}{x^{i+1}} \left( x^3 \frac{d}{dx} \right)^i \frac{ce^{hx} + c_1 e^{-hx}}{x^{2i-1}}.$$

[1844, 252; 1877, 425].

The study of binomial equations (46.1) was further illustrated in stage 4 of Boole's paper [see 4.5] by another four ordinary differential equations [1844, 253-257]. We would like to

add that in his later textbook he also incorporated the partial differential equation

$$(46.42) \quad \frac{d^2u}{dx^2} - a^2 \frac{d^2u}{dy^2} - \frac{i(i+1)u}{x^2} = 0.$$

This equation was solved by comparison of its form with the ordinary equation (46.33). The solution of (46.42) can readily be determined from (46.40) by changing  $h$  into  $a \, d/dy$  and  $c, c_1$  into arbitrary functions of  $y$  [1877, 425-427]. This example is discussed in detail in 8.8 in connection with Boole's mature views on symbolical methods [see (88.5)-(88.7), 8.8 text before (4)].

The only clue provided by Boole as to how he came to invent the basic theorem 2 was the statement he made in concluding his third example: "Equations of the above class [(46.33)] have been discussed by Mr. Leslie Ellis, in two very ingenious papers [1841a,b; see 4.3] published in the Cambridge Mathematical Journal, and it is just to observe that the first conceptions of the theory developed in [theorem 2: (46.8)-(46.10)], were in some degree aided by the study of his researches" [1844, 252].

Indeed, simplifying the initial form of the given equation in  $x$  by reducing it to a symbolical form in  $\theta$ , Boole effects via theorem 2, in one step, the reduction of the latter equation to one of similar form which is readily integrable. Focusing on the EFE, he sought, like Ellis, those differential equations whose solution depends upon that of (46.14) or (43.1). The latter worked, like Gaskin, by the method of successive reduction until the given equation is reduced to the form (43.1); Ellis by means of the scale of the equation substituting the wanted function by one in series form, Gaskin by differentiation. Thus, in all three cases the underlying motive is essentially the same. We believe that Boole's steps in constructing theorem 2 must have been roughly as follows.

He first studied Ellis [1841a,b], the latter paper being that which incorporated the symbolical solution of the general form of EFE. Next he formed the symbolical equations of both the EFE (46.33) and of (46.14) and tried to see the connection between  $u$  and  $v$ . In so doing, it sufficed to find a formule by means of which to transform  $(D-i)(D-i-1)$  in (46.34) into  $D(D-1)$  in (46.15) for  $n=2$ . This procedure generalized in the first example was consequently incorporated as a method in theorem 2:

theorem 1 used as an evident auxiliary result. Thus Ellis gave the general idea which Boole, after Gregory's and Murphy's own symbolical results (46.2)-(46.3), was to elaborate strictly symbolically. We have to stress, nevertheless, that Ellis's -and thus Boole's- important results might not had been produced had it not been for Gaskin's own ingenious method by the transform (42.14) and successive differentiation which led to (42.18), a form equivalent to (46.14) for  $n=2$ .

So far so good on Boole's motives and basic background in connection with his method studied above. There remains one crucial question-surprisingly absent in his speculations. What about the symbolical solution of the EFE (46.33) provided above? Only if (46.40) is proved to be equivalent to Ellis's result, <sup>is</sup> our study completed. Based on Glaisher [1881; see 1.4, (4); 4.2, (10)-(11)] we notice the following important connections.

Let the relation (46.37) which gives  $u$  in respect with  $v$  in the form (46.40). Now write the factors  $(D-1), (D-2) \dots$  in (46.37) in the reverse order,

$$(46.43) \quad u = e^{-1\theta}(D-2i+1) \dots (D-3)(D-1)v.$$

We have accordingly by (46.2)

$$u = e^{-1\theta} e^{(2i-1)\theta} D e^{-(2i-1)\theta} \dots e^{3\theta} D e^{-3\theta} \cdot e^{\theta} D e^{-\theta} \cdot v = e^{(1+i)\theta} (e^{-2\theta} D)^i e^{-\theta} v,$$

or

$$(46.44) \quad u = x^{i+1} \left( \frac{1}{x} \frac{d}{dx} \right)^i \frac{c \cos(hx) + c_1 \sin(hx)}{x}$$

[Glaisher 1881, 805]. Letting now

$$(46.45) \quad i = m, \quad h = n, \quad u = y$$

and suitably changing the arbitrary constants  $c, c_1$  in C.a, (46.44) readily gives

$$(46.46) \quad y = Cx^{m+1} \left( \frac{1}{x} \frac{d}{dx} \right)^m \frac{\cos(nx+a)}{x}$$

which is the form (42.34). It remains to show that (46.46) corresponds to the symbolical forms provided earlier in our study by Gaskin and Ellis.

Ellis's solution of the EFE, (43.22) or (43.23) is identical with Gaskin's symbolical form (42.28) or (42.33) or

$$(46.47) \quad y = Cx^{-m} \left( \frac{d}{dr} \right)^m \frac{\cos(x\sqrt{r}+a)}{\sqrt{r}}$$

where  $r$  is replaced by  $n^2$  after the differentiation. It remains to

show that (46.46) and (46.47) are equivalent. Indeed from (46.47)

$$y = Cx^{-m} \left( \frac{d}{dr} \right)^m \frac{\cos(x\sqrt{r}+a)}{\sqrt{r}} = Cx^{-m} \left( \frac{1}{n} \frac{d}{dn} \right)^m \frac{\cos(xn+a)}{n} \quad [\text{by (46.47)}]$$

$$= Cx^{-m} \cdot x^{2m+1} \left( \frac{1}{\xi} \frac{d}{d\xi} \right)^m \frac{\cos(\xi+a)}{\xi} \quad (\text{where } \xi = nx)$$

$$= Cx^{m+1} \left( \frac{1}{x} \frac{d}{dx} \right)^m \frac{\cos(nx+a)}{x}$$

which is no other but (46.46) [Glaisher 1881,811]<sup>(9)</sup>. A similar procedure was followed by De Morgan in showing the equivalence between the symbolic results (43.22) and (43.23) [4.3, (3)].

In the postscript of his lengthy paper Boole wrote [1844, 282]:

Fearful of extending this paper beyond its due limits, I have abstained from introducing any researches not essential to the development of that general method in analysis which it was proposed to exhibit. It may however be remarked that the principles on which the method is founded have a much wider range. They may be applied to the solution of functional equations, to the theory of expansions, and, to a certain extent, to the integration of non-linear differential equations. The position which I am most anxious to establish is, that any great advance in the higher analysis must be sought for by an increased attention to the laws of the combinations of symbols. The value of this principle can scarcely be overrated; and I only regret that in the absence of books, and under circumstances unfavourable for mathematical investigation, I have not been able to do that justice to it in this essay which its importance demands<sup>(10)</sup>.

Boole's treatment of binomial equations (46.1) -as based upon theorems 1, 2- was not followed by mid-19<sup>th</sup>-century analysts, but new methods were to be put forward [see 5.3-5.4,5.6]. As far as textbooks are concerned, we discern Carmichael [1855] and the second edition of Hymers [1839], [1858], as the only books to incorporate operator methods in connection with differential equations prior to Boole's own textbook [1859]. While Carmichael avoided Boole's method [see Russell 1857, 181; 5.7], Hymers included theorems 1, 2 together with their application to the EFE (46.33) in the form (42.15) or (43.15) [Hymers 1858,101-2]. Since

1859, the main textbook writer to incorporate symbolical methods was Forsyth in his A treatise on differential equations [1885], (sixth ed. 1928). However, no trace of operator methods for the solution of equations with variable coefficients is to be found. While Boole's own treatise [1859; 1865] is mentioned in its preface, Boole's name is related in the text in connection with the solution of the EFE (46.33) in the form (46.41) which is given as a problem [see Forsyth 1914, 205] <sup>(11)</sup>.

#### 4.7 Further applications of Boole's general theorem of development in 1845.

Boole further investigated his general theorem of development (45.12) in his sequel paper to [1844] entitled "On the theory of developments. Part I" [1845d] <sup>(1)</sup>. He opened his paper with a proof of that theorem [1845d, 217-217] and consequently based on it he proved the formulae

$$(47.1) \quad f\left(x + \varphi'\left(\frac{d}{dx}\right)\right)u = e^{\varphi\left(\frac{d}{dx}\right)} f(x) e^{-\varphi\left(\frac{d}{dx}\right)}$$

and

$$(47.2) \quad f\left(\frac{d}{dx} + \varphi'(x)\right)u = e^{-\varphi(x)} f\left(\frac{d}{dx}\right) e^{\varphi(x)} u,$$

where  $f, \varphi$  are arbitrary and  $\varphi'(d/dx)$  is derived from  $\varphi(d/dx)$  "in like manner as  $\varphi'(t) = d/dt \varphi(t)$ " [1845d, 217, 219]. Notice that these two theorems are but a generalization of the known formula (45.19). Moreover, formula (47.2) was initially given by Murphy as (33.56) by his theory of transmutations. Boole gave no credit to Murphy but simply added after proving (47.1) through a complicated procedure "Perhaps this result might be obtained more simply by induction" [1845d, 219].

Boole's reasoning had as follows. He first wrote the left-hand side of (47.1) as the limit of

$$(47.3) \quad f\left(x + \varphi'\left(\theta + \frac{d}{dx}\right)\right)u,$$

when  $\theta \rightarrow 0$ . Writing next (47.3) in the form  $f(x + e^{d/dx} \cdot \theta \cdot e^{-d/dx} \varphi'(\theta))u$ , he obtained

$$(47.4) \quad f\left(x + \varphi'\left(\theta + \frac{d}{dx}\right)\right)u = f(x + \rho)u$$

where

$$(47.5) \quad \rho = e^{\frac{d}{dx} \frac{d}{d\theta}} \varphi'(\theta).$$

By combination of  $f(x)$  with  $\varphi'(\theta)$  and on substitution of  $\rho$ , he found

$$(47.6) \quad \rho f(x)u = f\left(x + \frac{d}{d\theta}\right)\rho u.$$

Now, (47.6) is but a particular case of the law

$$(47.7) \quad \rho f(n)u = f(\varphi(n))\rho u$$

which the operators  $n$ ,  $\rho$  in  $f(n+\rho)$  obey when the latter is expanded in ascending powers of  $\rho$  [1845d, 217-8; see laws (ii)-(iii) of theorem (45.12)].

We can thus apply formula (45.13), or

$$(47.8) \quad f(n+\rho) = f(n) + \frac{\Delta}{\Delta n} f(n)\rho + \frac{\Delta^2}{\Delta n^2} f(n) \frac{\rho^2}{1.2} + \dots$$

for  $n=x$  and  $\rho$  given by (47.5). By further making use of distinct operator symbols which refer only to  $f(x)$  and  $u$  respectively - as Gregory did for the proof of Leibniz's theorem in (44.11)-(44.12) - Boole arrived after some complicated calculations at the required result (47.1). A similar procedure, he argued, will give (47.2) [1845d, 218-219].

The rest of Boole's paper consisted of some important observations which concerned his two new theorems together with hints of applications to the solution of certain classes of differential equations. As these observations were to be implicitly influential both to Boole's followers - in the realms of operator methods - and to Boole's own work in logic [see (77.10)-(77.11)] it is worth citing those of most importance. If we make  $\varphi(x) = \log x$ , it follows from (47.1) and (47.2),

$$(47.9) \quad f\left(x + \left(\frac{d}{dx}\right)^{-1}\right)u = f'(x)u + f(x) \int u dx$$

and

$$(47.10) \quad f\left(\frac{d}{dx} + \frac{1}{x}\right)u = f\left(\frac{d}{dx}\right)u + \frac{1}{x} f'\left(\frac{d}{dx}\right)u$$

respectively. That is, the development terminates at the second term "whatever may be the form denoted by  $f$ " [1845d, 219-220].

If we let further the special case of  $\varphi'$

$$(47.11) \quad \varphi'\left(\frac{d}{dx}\right) = \frac{d}{dx}$$

and define  $f_0(x)$  by

$$(47.12) \quad f_0(x) = e^{\frac{1}{2} \frac{d^2}{dx^2}} f(x),$$

then, formula (47.1) gives the expansion

$$(47.13) \quad f\left(x + \frac{d}{dx}\right)u = \left(f_0(x) + f'_0(x)\frac{d}{dx} + \frac{1}{1.2}f''_0(x)\frac{d^2}{dx^2} + \dots\right)u.$$

Also, letting  $\phi'(x)=x$  in (47.2), Boole obtained "after a troublesome reduction", which he omitted, the development

$$(47.14) \quad f\left(\frac{d}{dx} + x\right)u = \left(f_0\left(\frac{d}{dx}\right) + f'_0\left(\frac{d}{dx}\right)x + \frac{1}{1.2}f''_0\left(\frac{d}{dx}\right)x^2 + \dots\right)u.$$

He noticed first that the coefficients of these developments after the first term "follow the law of Taylor's theorem, which is a remarkable circumstance, seeing that the symbols  $x$  and  $d/dx$  are not commutative". Further, the two developments "are thus seen to be of precisely the same form, which again is a remarkable circumstance" [1845d, 220-21] (2).

Assume now that we have an equation of the form

$$(47.15) \quad f_0(x)u + f'_0(x)\frac{du}{dx} + \frac{1}{1.2}f''_0(x)\frac{d^2u}{dx^2} + \dots = 0,$$

$f_0(x)$  a "rational and integral function of  $x$ ". Then by (47.13), we can put (47.15) in the form

$$(47.16) \quad f\left(x + \frac{d}{dx}\right)u = 0,$$

$f$  determined in respect with  $f_0$  by (47.12). If  $a_1, a_2, \dots$  are the roots of  $f(x)=0$ , then (47.16) is readily reduced to a system of equations of the form

$$(47.17) \quad (x-a_1)u + \frac{du}{dx} = 0.$$

Let, for example, the equation

$$(47.18) \quad (x^2-5x+7)u + (2x-5)\frac{du}{dx} + \frac{d^2u}{dx^2} = 0.$$

This equation is of the form (47.15). For, let

$$(47.19) \quad f_0(x) = x^2 - 5x + 7.$$

Then  $f'_0(x) = (2x-5)$  and  $f''_0(x) = 1 = 2/1.2$ . By (47.12) we can determine  $f(x)$ :



$$f(x) = e^{\frac{1}{2} \frac{d^2}{dx^2}} f_0(x) = \left(1 - \frac{1}{2} \frac{d^2}{dx^2} + \dots\right) (x^2 - 5x + 7) = x^2 - 5x + 6, \text{ or}$$

$$(47.20) \quad f(x) = (x-2)(x-3).$$

Thus, (47.18) is reduced<sub>to</sub> the system

$$(47.21) \quad (x-2)u + \frac{du}{dx} = 0, \quad (x-3)u + \frac{du}{dx} = 0,$$

and its solution is accordingly the sum of the two particular integrals of (47.21). Or,  $u$  is given by

$$(47.22) \quad u = c_1 e^{\frac{x^2}{2}} + c_2 e^{\frac{3x^2}{2}}$$

[1845d, 221].

The last important remark was that by means of formula (47.10) -which is a particular case of the second theorem (47.2)- we can integrate any equation of the form

$$(47.23) \quad x f\left(\frac{d}{dx}\right)u + f'\left(\frac{d}{dx}\right)u = 0$$

[1845d, 220]. Indeed, by (47.10), equation (47.23) is reduced to the form

$$(47.24) \quad f\left(\frac{d}{dx} + \frac{1}{x}\right)u = 0,$$

a form analogous to (47.16).

Boole did not treat equation (47.23) in this paper. In his [1847e] he gave the solution of the more general class

$$(47.25) \quad x\phi(D)u + \psi(D)u = X,$$

where  $D=d/dx$  and  $X=X(x)$ , but without any proof. His solution motivated Hargreave the following year to invent a method peculiar to the solution of equations of the form (47.23) and (47.25) [see 5.3]. In general, Boole's paper [1845d] would be very influential to Bronwin, as we shall see in 5.2 and 5.4(3).

#### 4.8 An overview of the calculus of operations in England: 1837-1845.

In sections 4.2-4.6 we illustrated the important role of the earth-figure equation in the development of English symbolical methods during the period 1837-1844. We noticed a chain of in-

fluences which linked Gaskin's revolutionary step as a moderator in 1839 with Boole's symbolical method for the solution of certain differential equations with variable coefficients in 1844. Generally we saw how the search for finite solutions for ordinary differential equations involved initially in physical astronomy, in combination with a parallel search for the justification of operator methods, gave rise to Boole's sophisticated method in analysis [1844;1845d;see 4.7] which overshadowed those of his predecessors in generality and range of applications.

Just as Murphy, Gregory and Ellis formed a most significant background for Boole's general method, Boole's own work in 1844-1847 was to motivate English and Irish analysts to further vindicate the value of symbolical methods and to modify and extend Boole's theory, enriching it with new techniques and applications. Before we proceed to study this new era of the calculus of operations in chapter 5, we would like to cast a more general look at this branch of analysis during the period 1837-1845, which was viewed up to this point mainly through the study of the EFE. In so doing, we will connect certain aspects of our previous work in chapters 1-3, together with that carried out in 4.2-4.7, and thus prepare the basic background for the next chapter.

For convenience we provide a list of the subjects discussed in this section in the order of their exposition:

- (1) Finite difference calculus (Herschel's theorem; generating functions),
- (2) Ordinary differential equations,
- (3) Functional equations (exam papers; textbooks), and
- (4) Partial differential equations; the Laplace equation.

Above all we want to give an idea of the knowledge of British analysts on these topics in the mid 1840's through a commentary on the current textbooks and exam papers. A few words are added in the end on the work of French analysts of that time on the EFE<sup>(1)</sup>.

Up to Hymers [1839], the only textbooks on the calculus to include the method of separation of symbols were Lacroix [1816] and Herschel [1820]. Instances from the latter books were incorporated in the Cambridge curriculum in the 1820's and 1830's [3.2]. Aspects of the calculus of operations revived in Murphy [1837; 3.3]. But, despite the similarities between Murphy's work and that promoted by the Analytical Society in the

mid 1810's. Herschel's own results in the calculus of finite differences were noticeably absent in Murphy's memoir. Herschel's expansion formula (23.18) was discovered by Hamilton in 1837. Calling it after Herschel, Hamilton generalized the theorem (23.18), providing a symbolical expression for Maclaurin's theorem, (23.30), later named after him [see 2.3].

In our study, the main theorems of the calculus of finite differences considered are the following:

- (i) Lagrange's theorem (15.3) [(22.16), (23.16)]
- (ii) Herschel's theorem (23.18) [(23.21)]
- (48.1) (iii) Hamilton's theorem (23.30)
- (iv) The expansion of  $(e^t-1)^n$  (22.18), for any integer  $n$ , and
- (v) The expansion of  $(t/(e^t-1))^n$  (23.23),  $n>0$ .

These theorems formed an essential component of the development of the calculus of operations up to the late 1830's. From then onwards, their demonstration in a more concise way challenged mathematicians to vindicate the power of new symbolical methods. Let us now see the state of these theorems in textbooks during 1839-1850.

The first to incorporate Herschel's theorem and related topics in a textbook since 1820, was Hymers in 1839. The demonstrations he provided were the most lucid available from those followed by other textbook writers, with one exception, Pearson<sup>(2)</sup> [see Hymers 1839, 1-23; 1858, 1-26; 4.2 and (9)]. Gregory gave a rather awkward, concise proof of (23.18), including Lagrange's theorem and Brinkley's formula (22.15) in his [1839e, 120-22; see 4.4]. However, none of the theorems listed in (48.1) appeared in his Examples [1841]<sup>(3)</sup>. The richest source available of such theorems was De Morgan's [1842c; see 3.9, (2)].

Expansions concerning the Bernoulli numbers and Herschel's theorem were among the examination questions in the years 1838-1849 [see Cambridge 1849; 3.2, (7)]. Another subject, closely related to the finite difference calculus, which featured prominently in the Tripos from 1830 onwards, was that of generating functions. A representative problem posed in 1840 was:

If the generating function of  $x$ ,  $G(x)$  is  $\phi(t)$ , then prove the formulae

$$(48.2) \quad \frac{\phi(t)}{a - \log_a t} = G(a^{x-1} \int_x e^{ax} X), \quad (i)$$

$$\frac{t\phi(t)}{1-at} = G(a^{x-1} \sum \frac{X}{a^x}) \quad (ii)$$

[Cambridge 1849, 67] (4).

A very comprehensible exposition of Laplace's theory of generating functions [see (15.30)-(15.44)] was given in Hymers [1839, 14-18, 36-46], first as an alternative method for the proof of certain formulae, and consequently as a method applicable for the solution of finite difference equations. Few years later we come across Pearson's book The elements of the calculus of finite differences [see (2) above]; a similar but even more extensive and lucid exposition is to be found in his [1850, 1-17, 20-60; see also Hymers 1858, 13-19, 38-42]. Gregory omitted this theory in his Examples [1841], confining in his [1839e, 115-6] only to a specific illustration of the utility of generating functions in connection with diverging series. On the other hand, De Morgan included in his [1842c, 337-40, 146-50] only a few instances from that theory which he applied to combinatorial analysis. He did not delve into the connection between the calculus of generating functions with that of operations.

In connection with differential and other linear equations with constant coefficients, Gregory's Examples [1841] was the first and richest account available at that time. All the known methods were given, including symbolical ones, together with a wide range of examples [4.4]. Few basic instances from it were included in De Morgan [1842c, 751-58] and since then in Hymers [1858], Carmichael [1855] and Boole [1859]. As we saw in 4.6, Hymers was the only one to include Boole's theory of binomial equations (46.1). Surprisingly, Hymers's lucid textbooks were not given any credit in later works (5).

In the Tripos exams we see instances of Gregory's symbolical methods, such as a question regarding the proof of (44.15) for  $n < 0$ . More specifically, in 1845 we come across the question requiring the proof of the formulae:

$$(48.3) \quad \sum a^{-x} u_x = a^{-n} (D-a)^{-n} u_x ; \quad (D-a)^{-n} 0 = a^x (c_1 + c_2 x + \dots)$$

[Cambridge 1849, 220-30; on D see (44.10))]. In general, Gregory's formulae (44.14)-(44.15) -incorporated in Boole's general theorem (45.12)- were among the most useful results of the theory of the integration of equations by symbolical methods. This theory was enriched by the results of Boole's paper "On the theory of developments. Part I" [1845d; see 4.7].

We now switch briefly to the subject of functional equations. With De Morgan's treatise [1836] remaining virtually unread [3.5-3.9], the only novelty effected in this domain was Gregory's symbolical treatment of linear functional equations (44.41). However, as we saw in 4.4 [text and (8)], this treatment still depended on Laplace's reduction to finite difference equations. Further research was carried on by Wallace in 1840. On his study of equation

$$(48.4) \quad f(x)f(y) = C[f(x+y) + f(x-y)]$$

see reference in [Koppelman 1971, 208] <sup>(6)</sup>.

From the mid 1810's well up to Boole [1860], the only standard methods provided in textbooks, and so required in the Tripas, were Herschel's or Laplace's method and Babbage's method of elimination [see 2.4, 2.5, 3.2, 3.9]. For example, in 1839 we come across a question requiring the solution of

$$(48.5) \quad \varphi(x) + \varphi(x+\varphi(x)) = 0,$$

and in 1846 of

$$(48.6) \quad \varphi(x) + \varphi\left(\frac{1}{1-x}\right) = c$$

[Cambridge 1849, 80-90] <sup>(7)</sup>. As far as the new textbooks are concerned, Gregory omitted this subject altogether in his Examples [1841], while De Morgan's account in [1842c, 737] was too concise and far from satisfactory. <sup>U<sub>p</sub></sup> to Pearson [1850] we have not traced functional equations in Cambridge textbooks other the Herschel's and Babbage's Examples [1820]. A variety of equations of the form (37.54) and (25.38) were to be included in [Boole 1860, 218-229; 2.5, (13); Hymers 1858, 93-100; Pearson 1850, 62-66].

We end with a few words concerning the Laplace and the earth-figure equations; the latter seen individually and also as an outcome of the partial differential equation

$$(48.7) \quad \frac{d^2 z}{dt^2} = a^2 \left[ \frac{d^2 z}{dx^2} - \frac{i(i+1)z}{x^2} \right].$$

Equation (48.7) was solved by the series method by Poisson in 1823 in definite integral form. Among the cases distinguished by him was that of one variable deduced from (48.7) by transformation, in other words that of the EFE [see (17.35)–(17.38)]. The latter equation was reconsidered in his [1835, 158–160]. However, Poisson's definite-integral solution of the EFE—just like Murphy's solution of the Riccati equation (33.2)—were not known to mid-19<sup>th</sup>-century English analysts.

As we mentioned in 1.4, Liouville studied the Riccati equation (14.4) in 1841 transforming it to the form (14.6) which includes as a subcase the EFE (14.8). Based upon Liouville's study Lebesgue provided a symbolical solution of equation

$$(48.8) \quad \frac{d^2y}{dx^2} \pm \frac{2i}{x} \frac{dy}{dx} + ny = 0$$

in his [1846] published in the Journal de Mathématiques Pures et Appliquées. Equation (48.8) is no other but the reduction of the EFE (42.13) after the transform (42.14), that is Gaskin's equation (42.15). Lebesgue gave the integrals of (48.8) in the form

$$(48.9) \quad y = -\left(\left(\frac{1}{x} \left(\frac{1}{x} \left(\frac{1}{x} \dots \left(\frac{1}{x} v'\right)'\right)'\dots\right)'\right)'\right)'$$

and

$$(48.10) \quad y = x^{2i+m} \left(\left(\frac{1}{x} \left(\frac{1}{x} \dots \left(\frac{1}{x} v'\right)'\right)'\dots\right)'\right)'$$

respectively, where  $v$  stands for

$$(48.11) \quad v = c \sin x\sqrt{n} + c_1 \cos x\sqrt{n}$$

[Lebesgue 1846, 339]. Surprisingly, Lebesgue's paper was not known to Boole and his followers up to the 1860's. Glaisher was apparently the first to discover it in [1872, 137]. He further included Lebesgue's solution of the EFE in his [1881, 811–12].

Boole mentioned Poisson in his [1859; see 1877, 424] in connection with the latter's solution of the EFE (46.33) as in Poisson [1835, 158]. Notice also that Boole solved (48.7) —in the form (46.42)— in his textbook. Contrary to Poisson, he based the solution of the partial differential equation (46.42) upon the symbolic solution of the EFE (46.33). As we saw in 1.3, the EFE in its general form (13.33) —or (46.33)— was an outcome of

Laplace's theory of attractions which was based on the properties of the coefficients  $U^{(4)}$  named after him [see (13.5)-(13.6)]. This theory appeared in textbooks from 1833 onwards [3.2]. Problems concerning the Laplace coefficients appeared accordingly in the Tripos exams in the early 1840's [Cambridge 1849, 129, 199-200] (e) but due to Whewell's reaction they were excluded from 1845 onwards [Becher 1980b, 39]. In other words, Cambridge University saw a peak in analytics and pure mathematics in the period under study in this chapter.

Our last concern is with the equation of Laplace's coefficients (13.6), cited hereafter as LE. Murphy, Pratt and O'Brien confined in 1833, 1836 and 1840 respectively to an approximate solution by means of the series method. For, the direct solution in finite form presented with insurmountable difficulties [3.2]. Hargreave was the first to provide a solution of (13.6) in finite form in his [1841]. His method was not rigorous. Moreover, as he did not make use at that stage of symbolical methods, we omit a reproduction of his procedure. However, Hargreave's bold attempt was an extreme stimulus for Boole to extend his general method -as given in his [1844]- and to apply it to the LE. He first communicated his symbolical solution in a note read to the Report to the British Association in 1845 under the title "On the equation of Laplace's functions" [1845e]. As the main theory connected with the solution provided was published in 1847, we postpone a sketch of it until 5.8.

## Chapter 5

### Operator methods with English and Irish analysts after Gregory and Boole: 1846-1860.

#### 5.1 Introduction.

Operator methods, as pursued by Gregory and Boole in the early 1840's, were to have a strong impact on English and Irish mathematicians from 1846 until 1860. While these developments were initially conceived within Cambridge University [3.2-3.3, 4.1-4.4], the scene of action gradually moved to Trinity College Dublin with exceptional participation from graduates of Oxford University and University College London. Accordingly, Gregory's journal was succeeded in 1846 by the Cambridge and Dublin Mathematical Journal which was called from 1855 The Quarterly Journal of Pure and Applied Mathematics edited by Sylvester. Several papers on the calculus of operations appeared in the Philosophical Transactions, the Philosophical Magazine and in the Proceedings of the Royal Irish Academy.

Boole remained active in the late 1840's involved with the integration of the Laplace equation, while the prolific Bronwin was engaged with extending and elaborating Boole's methods and results up to 1852. Among the new authors on D-operator methods we note the Englishmen C.Hargreave, W.Donkin, W.H.L.Russell and W.Spottiswoode, and the Irishmen C.Graves, R.Carmichael, A.Curtis and B.Williamson. In addition, figures such as H.Jellett, W.Walton, A.Cayley, J.Sylvester, G.Salmon, M.O'Brien, H.Goodwin, A.J.Ellis, De Morgan, W.R.Hamilton and P.Kelland applied the calculus of operations to the calculus of variations, geometry, analysis and algebra. Further, new authors including J.Blissard, R.Greer, G.Scott and S.Roberts contributed minor papers on differential equations, fractional differentiation, the Bernoulli numbers and on the calculus of operations in the Quarterly Journal from 1860 up to 1870. But by 1860 onwards interest in the calculus of operations distinctly started fading away.

The material pertinent to our study which was published in the above journals up to 1860 is remarkably large. It displays a very intricate network of mutual influences, a close similarity in techniques and very often an identity of results. One wonders



about the aims of these new researchers as well as about the fate of their numerous and largely forgotten papers. We have seen that symbolical methods had come to the forefront of research in the early 1840's out of (i) insufficiency of known methods for the solution of differential equations prominent in physics [3.2, 4.1, (42.13)], and (ii) a need for justification and foundation of the calculus of operations [3.3, 4.1, 4.4]. These two concerns were successfully accomplished by 1844 [chapter 4]. The EFE—which partly had led to this new trend in analysis [4.6]—no longer demanded any drastic means of treatment, but could be satisfactorily solved together with a wide class of equations with variable coefficients. What was then still to be demanded of symbolical methods other than the theory and range of applications put forward by Gregory and Boole?

There still remained the unsettled problem of the finite solution of the LE, which however was to occupy a small part of the material produced. Surprisingly, the EFE was still at the centre of attention. In any case, the reasons (i) and (ii) mentioned above were no longer the ones to motivate new research; what prevailed was an obsession with further vindicating the power of symbolical methods on lines perceived by Greatheed and Ellis earlier on [4.2, (3); 4.3, (5)]. Boole's general method in analysis would challenge to further extensions and applications. The new authors were occupied with one or more of the following concerns:

- 1) A general study of specific non-commutative operations,
- 2) Invention of many particular methods peculiar to certain classes of equations,
- 3) Invention of methods not dependent on artificial theorems of the calculus of operations,
- 4) A search for new results and for interpretation of symbolic forms beyond the differential and integral calculus,
- 5) Elegance, rapidity, brevity and symmetry of both methods and results.

Accordingly, the chapter is designed as follows.

Section 5.2 covers Bronwin's attempt to assimilate, apply, extend and even criticize Boole's methods and results during the period 1846–1848. In 5.3 we discuss Hargreave's paper [1848] where the method introduced was illustrated through the solution of the earth-figure, the Riccati and the Laplace equations. We

focus principally on Hargreave's theory, invented after Boole's note on binomial equations [1847e;4.7], illustrating it with the solution of the EFE. We conclude our study of Bronwin's and Hargreave's work on ordinary differential equations in 5.4 discussing their papers produced between 1850 and 1853. These papers, marking the peak of generalization and abstraction in the realms of the D-operator calculus, were strongly motivated by Boole's techniques and concerns. As we shall see, these three sections form a unity in our study of the consequences of Boole's general method -as further extended in the late 1840's - over his contemporaries in England.

Section 5.5 is devoted to Donkin's and Graves's parallel and independent investigations on non-commutative operations between 1847 and 1857. There follows in 5.6 a study of the new treatments of the EFE by Curtis, Williamson, Donkin and Graves in 1854-1857. At this stage we have evidence of the influence of Boole, Bronwin and Hargreave, as well as of a close similarity in the results of the new researchers.

In 5.7 we introduce Carmichael. Motivated by Sylvester and Jellett, Carmichael produced new original work in the field of partial differential equations with homogeneous functions as coefficients. The core of his work in the early 1850's was included in his textbook [1855], the sole book in the wider period of our study devoted exclusively to the calculus of operations. With a noticeable avoidance of Boole's general method, Carmichael's book drew heavily on Herschel, Murphy, Gregory, Graves, Curtis, Hargreave and Bronwin, forming a kind of monument of that historical era.

Section 5.8 is devoted to a study of the LE as treated by Hargreave, Boole, Donkin, Carmichael and Graves between 1846 and 1851 rescuing from oblivion the most important results. Finally, 5.9 covers the work of Boole's and Carmichael's followers -introducing Spottiswoode, Russell and Greer- while a concluding section 5.10 will sum up the impact and utility of symbolical methods in the period under study in the wider realms of mathematical sciences.

## 5.2 Bronwin on symbolical methods: 1846-1848; Boole's influence and comments.

In 4.3 we studied Bronwin's work on differential equations in 1841-1843. We had evidence at that stage of an implicit influence from Gaskin and Ellis, but at the same time an absence of explicit symbolical methods. Interested in equations with variable coefficients, Bronwin was apparently not tempted to apply Gregory's method of separation of symbols. For three years he focused on other topics [see 4.3,(7)]. Gradually assimilating Boole's work [1844;1845d] he set off to apply operator methods. At first, he used a combination of Gaskin's technique of successive reduction with his own early methods, also aided by Gregory's symbolic formulae from Examples [1841]. Boole's influence, implicit in 1846, becomes more evident in 1847, and by 1848 Bronwin produced his first original work nearly superseding Boole's abstraction and sophistication, and getting as far as criticizing the latter's paper "On a certain symbolical equation" [1847c].

Bronwin's first paper under study is entitled "On the integration and transformation of certain differential equations" [1846a]. Variations on this theme were to be published as [1846b; 1847a,b,c]. The period 1846-1847 was to be a transitional one between Bronwin's early work and his mature, original work around 1848. We will first draw representative instances from this unity of five papers which, with a slight exception [1847b], are based upon the same concern. Papers [1846a] and [1847c] were published in the Philosophical Magazine, [1847b] in The Mathematician, while all the other papers to be discussed were contributed to the Cambridge and Dublin Mathematical Journal.

Letting  $D$  stand for  $d/dx$ , Bronwin put forward the key-formulae of his new method

$$xD^ny = D^nx^ny - nD^{n-1}y$$

$$(52.1) \quad x^2D^ny = D^nx^2y - 2nD^{n-1}xy + n(n-1)D^{n-2}y$$

$$xD^{-n}y = D^{-n}xy + nD^{-n-1}y$$

and so on, claiming for verification by "performing the differentiations" [1846a,494]. These formulae could be directly deduced from Leibniz's theorem (33.48) -by putting  $v=x$  and  $u=y(x)$ - a fact which he mentioned as an alternative demonstration in [1847a, 42-43].

Bronwin's first application of (52.1) was to the equation

$$(52.2) \quad x\left(\frac{d^2y}{dx^2} + k^2y\right) + 2p \frac{dy}{dx} = 0$$

which is none other than a transformed form of the EFE (42.15) or (43.15) <sup>(1)</sup>. Putting

$$(52.3) \quad y = (D^2+k^2)u,$$

he applied (52.1) in order to determine the terms  $pd y/dx$ ,  $xk^2y$  and  $xd^2y/dx^2$ . Substituting the equivalent of these terms in D and u in (52.2) he obtained (52.2) in the form

$$(52.4) \quad (D^2+k^2)[D^2xu+2(p-2)Du+k^2xu] = 0.$$

Erroneously discarding the first factor, he claimed that the given equation (52.2) is equivalent to

$$(52.5) \quad D^2xu + 2(p-2)Du + k^2xu = 0;$$

or, by transforming (52.5) in its normal form, to

$$(52.6) \quad x\left(\frac{d^2u}{dx^2} + k^2u\right) + (2p-2)\frac{du}{dx} = 0,$$

"an equation similar to the given one, p being changed into p-1". Hence, by repetition, the last term in (52.6) vanishes <sup>(2)</sup>. Bronwin's conclusion was the following:

if therefore

$$(52.7) \quad y = (D^2+k^2)^pu \text{ (i), we shall have}$$

$$\frac{d^2u}{dx^2} + k^2u = 0 \text{ (ii), the integral of which is known}$$

[1846a, 495] <sup>(3)</sup>.

The erroneous inference of (52.5) from (52.4) did not pass unnoticed. In 1847 Boole and Hargreave had their remarks consequitively published in the Philosophical Magazine restoring properly Bronwin's fallacy. Discussing the solution of (52.2) in Bronwin's context, Hargreave showed in simple terms that it is not (52.7) which gives the complete integral of (52.2), as it amounts, in fact, to  $y=0$ , but

$$(52.8) \quad y = (D^2+k^2)^{p-1} \left[ \frac{1}{x} (D^2+k^2)^{-p} 0 \right]$$

[Hargreave 1847, 8-9] <sup>(4)</sup>.

Boole's final remark coincides with Hargreave's but the style is more general as it draws on his [1847c] which was to appear few months later. In brief, Boole considered the symbolic

equation

$$(52.9) \quad \pi_m u = 0,$$

where  $\pi_m$  and  $\rho$  were two "compound factors" which operate in subsection to the relation

$$(52.10) \quad \pi_m \rho = \rho \pi_{m-1}.$$

Solving (52.9) in the form

$$(52.11) \quad u = \rho^m \pi_0^{-1} \rho^{-m} 0,$$

Boole let

$$(52.12) \quad \pi_m = x(D^2 + k^2) + 2mD, \quad \rho = D^2 + k^2, \quad D = d/dx$$

which substituted in (52.9) give Bronwin's equation (52.2). By means of (52.12), (52.11) was reduced accordingly to (52.8) -if  $m=p$  [Boole 1847d, 6-7]. Boole went on to claim that such symbolical methods, "limited in their individual application, and apparently indefinite in their number" are "chiefly valuable as exercises in symbolical algebra". Considering Bronwin's attempt in the realms of his own general investigation in [1844] he nevertheless added that "Mr. Bronwin's method displays considerable ingenuity"<sup>(5)</sup>.

Bronwin took notice of these comments -concerning all the equations solved in his first paper- in his [1847c] where he used his previous inquiries for the treatment of similar equations in partial differences. The first equation solved was

$$(52.13) \quad x\left(\frac{d^2 z}{dx^2} - a^2 \frac{d^2 z}{dy^2}\right) + 2p \frac{dz}{dx} = 0.$$

Letting

$$(52.14) \quad D = d/dx, \quad D' = d/dy \text{ and } aD'\sqrt{-1} = k,$$

(52.13) was reduced to the form (52.2) integrated according to the instructions given by Hargreave above [1847c, 107-108; 113]<sup>(6)</sup>.

A slightly different approach was taken in Bronwin [1846b]. Instead of (52.1), he made use of the formula (45.20), as it appeared in [Gregory 1841, 31-2]. He proved it by first getting (45.19) in the form

$$(52.15) \quad e^{ax} f(d/dx + a)y = f(d/dx) e^{ax} y.$$

Changing  $e^x$  into  $x$ ,  $d/dx$  into  $xd/dx$  and letting  $D=d/dx$ , he wrote (52.15) in the form

$$(52.16) \quad x^a f(xD + a)y = f(xD) x^a y.$$

By means of (52.16) he obtained inductively

$$(52.17) \quad D^n y = (xD+1)(xD+2)\dots(xD+n)x^{-n}y,$$

which is formula (45.20). Letting next  $y$  replaced by  $D^{-n}y$  in

(52.17) he obtained an analogous form for  $D^{-n}y$  [1846b, 154].

Mentioning Gregory's Examples as a source of influence for the above formulae, Bronwin went on to apply them to the solution of second order differential equations with variable coefficients, such as equation

$$(52.18) \quad (1-x^2)\frac{d^2y}{dx^2} + p(p+1)y = 0.$$

By a procedure reminiscent of Boole's in [1844], Bronwin multiplied (52.16) by  $x^2$  and, based upon (52.16)–(52.17), he reduced (52.18) to the form

$$(52.19) \quad xD(xD-1)y - (xD+p-2)(xD-p-3)x^2y = 0.$$

He then let the transform

$$(52.20) \quad y = (xD+1)(xD+2)\dots(xD+p-1)x^{-p+1}u = D^{p-1}u$$

and substituted accordingly the values of  $y$  and  $x^2y$  in (52.19). The result in non-symbolic form is

$$(52.21) \quad (1-x^2)\frac{du}{dx} + 2pxu = 0$$

which by solution gives a particular integral of (52.18) as

$$(52.22) \quad y = D^{p-1}u = c\left[\frac{d}{dx}\right]^{p-1}(1-x^2)^p.$$

In the same manner another particular integral was provided, and hence the complete solution of (52.18) [1846b, 157–8] (?).

A combination of the techniques employed in his first two papers was further applied in his [1847a] to the solution of finite difference equations. Bronwin displayed his initial formulae (52.1) in  $\Delta$  instead of  $D$  and applied them to the equation

$$(52.23) \quad (1-x^2)\Delta^2y + p(p+1)y = 0$$

which corresponds to (52.18). Then, as in [1846b] he found two particular solutions of (52.23) [1847a, 42–43]. In fact, the actual procedure used for the solution of (52.23) was communicated to The Mathematician for the solution of differential equations simultaneously with the method published in [1846a]. Having omitted its illustration for the case of (52.23) above, we display it now for (52.18) as published in [1847b, 204].

Let equation (52.18) with  $d/dx = D$ , or

$$(52.24) \quad D^2y - x^2D^2y + p(p+1)y = 0.$$

Put  $y = D^{p-1}u$  in (52.24) and determine all its terms by means of (52.1). Then, (52.24) is reduced to

$$(52.25) \quad Du - Dx^2u + (2p+2)xu = D^{-p}0 = CD^{-p+1}.$$

Bronwin preferred instead of determining directly the complete integral of (52.24) from (52.25), to take equation

$$(52.26) \quad Du - Dx^2u + (2p+2)xu = 0, \text{ or,}$$

$$(52.27) \quad (1-x^2)\frac{du}{dx} + 2pxu = 0,$$

as in (52.21) and obtain a particular integral, (52.22). The same procedure led to the second integral, and thus to the solution of (52.24) or (52.18), as above [1847b, 204; see (7) above].

Indeed, this method is an elaboration of those displayed in [1846a] and [1846b]. However, the importance of Bronwin's [1847b] did not lie in his variation of the initial technique based on (52.1). This latter paper is a turning point in his early work on D-operator methods for it improves for the first time over Boole [1845d] by including both straight-forward demonstrations of Boole's theorems (47.1) and (47.2), and further generalizations and applications of them. So, we conclude our study of Bronwin's transitional period by briefly referring to his main results on theorems of development as in his [1847b].

If  $\varphi$  and  $\lambda$  are any functions of  $x$ , Bronwin claimed without proof that the formula

$$(52.28) \quad e^{-\int \frac{\lambda}{\varphi} dx} (\varphi D) e^{\int \frac{\lambda}{\varphi} dx} y = (\varphi D + \lambda) y \quad (e)$$

holds true. Indeed, by performing the operation  $\varphi D$ , or  $\varphi(x).D$  [see (8) above], the left-hand side of (52.28) becomes:

$$e^{-\int \frac{\lambda}{\varphi} dx} \left[ e^{\int \frac{\lambda}{\varphi} dx} (\varphi D) y + y \varphi D e^{\int \frac{\lambda}{\varphi} dx} \right] = (\varphi D) y + y \varphi D \int \frac{\lambda}{\varphi} dx.$$

But  $D \int \frac{\lambda}{\varphi} dx = \int \left( \frac{\lambda}{\varphi} \right)' dx = \frac{\lambda}{\varphi}$ ; thus (52.28) is readily proved. By ite-

ration he proved the general formula

$$(52.29) \quad e^{-\int \frac{\lambda}{\varphi} dx} f(\varphi D) e^{\int \frac{\lambda}{\varphi} dx} = f(\varphi D + \lambda) y,$$

where  $f$  any polynomial function [1847b, 206].

Assuming an equation in the form

$$(52.30) \quad (\varphi D + \beta)(\psi D + \lambda) y = 0,$$

where  $\varphi, \beta, \psi$  and  $\lambda$  are all functions of  $x$ , he claimed that with the aid of (52.29) we can divide successively by  $\varphi D + \beta$ ,  $\psi D + \lambda$  and

operate upon 0 in the second member. Or, if we have the equation

$$(52.31) \quad f(\varphi D + \lambda)y = 0,$$

we may put it in the form

$$(52.32) \quad (\varphi D + \lambda - r_1)y = 0, (\varphi D + \lambda - r_2)y = 0, \dots$$

where  $r_1, r_2, \dots$  the roots of  $f(x)=0$ , and add the particular integrals for the complete solution.

Bronwin mentioned that (52.30)-(52.32) formed part of Boole's method, adding that perhaps by means of (52.29) integration of (52.32) is easier [1847b, 207; see (47.15)-(47.17)]. Letting next  $D, D'$  to operate only upon  $y$  and  $x$  respectively, he multiplied both sides of

$$(52.33) \quad e^{\varphi(D)} x e^{-\varphi(D)} y = xy$$

by

$$(52.34) \quad e^{\varphi(D+D')} - e^{\varphi(D)},$$

obtaining after straight forward calculations

$$(52.35) \quad e^{\varphi(D)} x e^{-\varphi(D)} y = (x + \varphi'(D))y.$$

By iteration, as in the case of (52.28), he arrived at Boole's theorem (47.1), or

$$(52.36) \quad e^{\varphi(D)} f(x) e^{-\varphi(D)} y = f(x + \varphi'(D))y$$

[1847b, 207] ('9').

Stimulated by the convenience and power of symbolical methods, Bronwin was to produce more papers; [1847d] and [1848b] on the symbolic representation of functions and [1848a] on theorems of development. The two former were related to the determination of definite integrals, while the latter to the integration of differential equations on lines similar to those in [1847b]. These papers mark the beginning of his mature stage giving evidence of his assimilation of Boole's work and also of his ambition to extend it.

In [1847d] he set Taylor's theorem in the form

$$(52.37) \quad \varphi(x) = e^{x D} \varphi(0), \quad D = d/d0.$$

Changing  $\varphi(x)$  into  $\varphi(e^x)$  and  $e^x$  into  $x$ , he obtained from (52.37),

$$(52.38) \quad \varphi(x) = x^D \varphi(e^0).$$

On similar lines he got

$$(52.39) \quad \varphi(x) = (1+\Delta)^{1/\Delta} x \varphi(e^0)$$

claiming next the truth of the formula

$$(52.40) \quad \frac{d^n \varphi(x)}{dx^n} = \varphi(x+\Delta) 0^n,$$

verified by developing its second member. (52.40) gives



$$\frac{d^n \varphi(0)}{d0^n} = \varphi(D)0^n, \text{ or } \varphi(x) = \varphi(0) + \frac{x}{1} \frac{d\varphi(0)}{d0} + \frac{x^2}{1.2} \frac{d^2\varphi(0)}{d0^2} + \dots =$$

$$= \varphi(D) \left(1 + \frac{0x}{1} + \frac{0^2 x^2}{1.2} + \dots\right), \text{ or the theorem}$$

$$(52.41) \quad \varphi(x) = \varphi(D)e^{0x}$$

[1847d, 134-136; on the notation by means of 0 see 2.2-2.3].

Bronwin applied the theorems (52.38), (52.41) for the determination of definite integrals. But he went further to obtain known symbolical theorems by means of the techniques put forward in the beginning. That is, changing  $\varphi(x)$  into  $\varphi(e^x)$  and putting next

$$(52.42) \quad e^D = 1 + \Delta = E$$

in (52.41), he obtained

$$(52.43) \quad \varphi(e^x) = \varphi(E)e^{0x}$$

which is Herschel's symbolic version (23.21) of his theorem (23.18) [1847d, 137]. On similar lines he proved Hamilton's theorem (23.30), or,

$$(52.44) \quad \varphi(x) = \varphi(E)x^0.$$

He went as far as deducing purely symbolic forms such as

$$(52.45) \quad \varphi(a+x) = \left(\frac{d}{da}\right)^x \frac{d}{dx} [\varphi(a)e^x]$$

upon which he commented as follows: "This appears a mere curiosity; but we do not know what may prove useful" [1847d, 139; see (11) below].

This paper was published, like the two which follow below, in the Cambridge and Dublin Mathematical Journal whose editor was W. Thomson. Thomson often used Boole as a referee for certain papers exchanging with him around 40 letters on such matters during the period 1845-1848. In connection with Bronwin [1847d] Boole wrote in April 1847 "Although I do not think Mr Bronwin's paper contains any thing very useful it ought to be printed because it is desirable to have records of our own progress even in directions in which nothing is to be hoped for" (10). A propos, Boole himself made use of a similar notation as Bronwin's (52.45) in 1847 in connection of Fourier's theorem:

It is proved that the value  $v$  of the definite integral

$$v = \frac{1}{2\pi} \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} da du e^{(a-x)u-1} f(a, u-1)$$

is symbolically expressed by the equation

$$v = \left(\frac{d}{dx}\right)^{\alpha} f(x, e^{\theta}) \text{ provided that } \theta=0^{(11)}.$$

In his paper "Application of certain symbolical representations of functions to integration" [1848b] Bronwin, based on the known formula (45.19), used the technique put forward in [1847d] of suitably changing the function under consideration in order to prove "beautiful" theorems such as

$$(52.46) \quad f\left(\frac{d}{dx}\right)\varphi(x)e^{rx} = \varphi\left(\frac{d}{dr}\right)f(r)e^{rx}.$$

This theorem proved useful for the determination of definite integrals, such as,

$$(52.47) \quad \int_0^{\infty} \varphi(x)f(x^2)e^{-rx}\cos nx = \varphi(-D)f(D'^2)\frac{r}{n^2+r^2}$$

where  $D=d/dr$  and  $D'=d/dx$ . The paper concluded with another "handsome theorem",

$$(52.48) \quad f\left(\frac{d}{dx} + r\right)\varphi(x) = \varphi\left(\frac{d}{dr} + x\right)f(x),$$

which was not further discussed [1848b, 245-251].

In [1848a] Bronwin proved theorems by the aid of which "we may sometimes put a differential equation under a more convenient [integrable] form". This paper, sequel to his [1847b], is a direct influence from Boole [1845d] referred to in the opening page. Letting  $D=d/dx$  and  $\rho, n, \tau$  functions of  $x$  and  $D$ , he proved that

$$(52.49) \quad \text{if } \rho n \rho^{-1} u = \tau u, \text{ then } \rho f(n) \rho^{-1} u = f(\tau) u,$$

where  $f(n)$  was a function developable in integer powers of  $n$ . Next, by means of Taylor's theorem, he proved that

$$(52.50) \quad \varphi(D)X\varphi(D)^{-1}y = \left(X + X'\frac{\varphi'(D)}{\varphi(D)} + \frac{1}{2.1}X''\frac{\varphi''(D)}{\varphi(D)} + \dots\right)y,$$

where  $X$  an integral function of  $x$  of degree  $n$  and  $\varphi(D)$  any function of  $D$ . In virtue of (52.49), (52.50) was generalized into

$$(52.51) \quad \varphi(D)f(X)\varphi(D)^{-1}y = f\left(X + X'\frac{\varphi'(D)}{\varphi(D)} + \dots + \frac{X^{(n)}}{n!}\frac{\varphi^{(n)}(D)}{\varphi(D)}\right)y$$

[1848a, 36-37].

Formula (52.51) was proved for  $D$  and  $x$  interchanged,

$$(52.52) \quad \varphi(x)^{-1} \pi(D) \varphi(x) y = \left( \pi(D) + \frac{\varphi'(x)}{\varphi(x)} \pi'(D) + \dots + \frac{\varphi^{(n)}(x)}{n! \varphi(x)} \pi^{(n)}(D) \right) y,$$

followed by generalizations in two variables [1848a,41-43]. Bronwin included in [1848a,37-41] applications of his theorems of development to the solution of differential equations on lines fairly similar to those in Boole [1845d;4.7].

In the same letter to Thomson quoted in (10) above, Boole wrote in connection with Bronwin's enquiries:

It is very easy by means of the known properties of  $x$  and  $d/dx$  to construct equations involving these symbols under functional signs and admitting of complete solutions. In general such labor is useless because it is impossible to foresee whether an equation presented under the ordinary form can be reduced to one of those functional forms or not..... What we want is a genuine and complete method<sup>(12)</sup>.

Regarding his own method [1844;4.5-4.6] as "in its present state incomplete", Boole stated: "If Mr Bronwin would undertake this, for which he appears to have both the leisure and ability, he would render a service to analysis"<sup>(13)</sup>. Indeed, Bronwin was to exploit both "leisure and ability" rendering part of Boole's desires true. In fact, Bronwin [1848a] was the only of his papers to be referred to in Boole's textbook [1877,456; 4.5,(5)] as an "interesting memoir" on examples of expansions of functions with non-commutative symbols. Moreover, despite Bronwin's later more sophisticated inquiries [5.4], it was mainly aspects from his [1848a,b] which appeared in the work of later researchers [see 5.5, 5.7].

The next paper by Bronwin to see the light of publication in 1848, made Boole furious as Bronwin had the audacity to perceive "errors" in his latest paper [1847c]. In that paper Boole had assumed the symbolical equation

$$(52.53) \quad \pi_m \pi_n u + q \rho u = 0,$$

where  $q$  a constant, and  $\pi_m$ ,  $\pi_n$ ,  $\rho$  operators which combine as

$$(52.54) \quad \pi_m \rho = \rho \pi_{m+1}, \quad \pi_n \rho = \rho \pi_{n+1}, \quad \pi_m \pi_n = \pi_n \pi_m + a(n-m) \rho$$

where  $a$  constant [Boole 1847c, 8]<sup>(14)</sup>. Bronwin let in his [1848c] equation (52.53) in the more general form

$$(52.55) \quad \pi_m \pi_n u + \lambda u = X,$$

$p$  constant and  $\lambda$ ,  $X$  functions of  $x$ , under the conditions

$$(52.56) \quad \pi_m \pi_n = \pi_n \pi_m + f(m)\lambda, \quad \pi_m \lambda = \lambda \pi_{m+1}.$$

Rightly generalizing upon Boole's procedures, Bronwin carelessly made remarks such as "Mr Boole has fallen into an error... " [1848c, 256-8]<sup>(15)</sup>.

The fact that Bronwin dared to comment upon Boole's work show that by 1848 he had gained enough maturity and confidence in the realms of symbolical methods. Boole admitted in his response "Remarks on a paper by the Rev. Brice Bronwin" [1848a] that Bronwin had indeed "added to the completeness" of his own investigations. However, he refuted Bronwin's arguments by pointing out that they were based upon "both a false analogy and an erroneous principle" [1848a, 414]. Grasping the opportunity, he concluded by calling attention in general to the abuse of symbolical methods and to the error lying behind such an abuse. Explaining this sad phenomenon as an outcome of the "unmeasured capabilities" that such methods offer in analysis, Boole added:

But it may be doubted whether this is a sufficient explanation of the fact in question. A far more influential cause is, I believe, to be found in the almost entire absence of any direct study of the laws of correct reasoning in connexion with the practical discipline of modern science. But this is a topic which I do not venture upon the present occasion more fully to discuss<sup>(16)</sup>.

Bronwin's work saw its climax in sophistication and generality in the early 1850's [5.4]. However, the intermediate stage 1846-1848 is very interesting as, by being prolific and bold, Bronwin provides us with a wide range of results which alone are mostly indicative of Boole's influence and of the gradual obsession of mid-19<sup>th</sup> century analysts with D-operator methods. While acknowledged by a few of his contemporaries -including Boole- for his mature work in 1848 [see 5.2 text above (5); 5.5; 5.7], Bronwin's work on the whole remained virtually unknown from 1860's to our days<sup>(17)</sup>. The unique exception is [Koppelman 1971, 200] where the only instance recorded is Bronwin's fallacious solution of (52.2). Indeed, the case of that erroneous inference [(52.4)-(52.5)] shows that, though justified by Gregory and Boole, the method of separation of symbols could very easily lead to unhappy fallacies and wrong results. Moreover, there still remained the intricate problem of interpretation of symbolic results which Hargreave was the first to call attention to

in 1848 after Boole's remarks at that time<sup>(18)</sup>. Though of limited originality and impact, Bronwin's work, is highly significant both as a representative sample of symbolical procedures at that time and as implicitly giving rise to Boole's and Hargreave's remarks which ascended one step higher the calculus of operations in the scale of rigour.

### 5.3 Hargreave's symbolical method [1848]; the earth-figure equation.

Bronwin was the first after Boole to inquire into the properties of  $d/dx$  and  $x$  and to invent new theorems of development as useful in the integration of certain differential equations. Notice particularly the theorems (52.51) and (52.52) in which  $x$  and  $d/dx$  are interchanged. This mode of investigation was further developed by Hargreave on very different lines, and independently of Bronwin, in his paper "On the solution of linear differential equations" published in the Philosophical Transactions in 1848. By means of the method proposed in that paper, Hargreave was able to tackle a wide class of differential equations including the EFE and the Riccati equation.

Though not prolific, Hargreave's work was highly esteemed by his contemporaries and his <sup>the early</sup> name featured also in a few studies of the late 19<sup>th</sup> century and 20<sup>th</sup> century. Still, very little is nowadays known of his interesting work. It is worth while starting with a brief biographical portrait. A lawyer by profession, C.H.Hargreave held the post of professorship of jurisprudence in University College London during the period 1843-1849. A graduate of that College with honours, Hargreave had attended De Morgan's classes. Gifted with a sound judgment and an eye for logical fallacies, he attracted the attention of his old teacher who informs us about the remarkable change in his handwriting; up to his teens, De Morgan said, it required a microscope to decipher, but, as soon as Hargreave emerged from his legal studies, the handwriting was of average size and legible. In the court his passion for order and his fine mathematical mind at once arranged the rights of the parties with a certainty approaching mathematical demonstration. In fact "He never seemed happier than when he was engaged in a subtle mathematical analysis, or in determining the rights arising from a deed when every event occurred except those

contemplated by the conveyancer who drew the instrument" <sup>(1)</sup>.

Hargreave is an interesting case of an amateur mathematician who spent leisure in solving mathematical problems. Even more interesting is his choice to solve the LE at the age of 21 in finite form, a problem which up to 1841 had remained unsolved in a direct way [see 1.3,4.8,5.8]. The first instance where Hargreave used operator methods was in his remarks on Bronwin's erroneous solution of (52.2) in 1847 [5.2]. Published together with Boole's pertinent remarks, these papers were followed by Boole's "Note on a class of differential equations" [1847e] as a "proper supplement" to the remarks "offered in common" by Boole and Hargreave [Boole 1847e,96]. This note concerned the solution of the binomial equation (47.25) given without demonstration. Stimulated by Boole's solution, Hargreave composed his own symbolic approach in his [1848], gaining the ~~Royal~~ medal of the Royal Society. There followed two more papers on the calculus of operations in 1850 and 1853 [5.4] together with <sup>a</sup> few on the theory of numbers, algebraic and differential equations including the problem of three bodies. Hargreave died in 1866 in the early age of 46.

While Boole tackled the equation

$$(53.1) \quad x\phi(D)u + \phi'(D)u = X, \quad D = d/dx$$

in his [1854d; 1847e; see (47.23) and (53.16) below], Hargreave undertook as a main task the study of the more general equation

$$(53.2) \quad x\phi(D)(\psi(x).u) + m\phi'(D)(\psi(x).u) = X$$

with  $\phi(D)$  a polynomial function of  $D$ ;  $\psi, X$  known functions of  $x$  and  $u$  the sought function. His main tools were the theorems

$$(53.3) \quad \phi(D)[\psi(x).u] = \psi(x).\phi(D)u + \psi'(x)\phi'(D)u + \frac{1}{1.2}\psi''(x)\phi''(D)u + \dots$$

and

$$(53.4) \quad \phi(x)[\psi(D).u] = \psi(D)(\phi x.u) - \psi'(D)(\phi'x.u) + \frac{1}{2!}\psi''(D)(\phi''x.u) + \dots (2).$$

Hargreave's first remark after stating these two theorems was that whenever a differential equation assumes the form of the right-hand side of (53.3) or (53.4), then its solution is readily determined. For example

$$(53.5) \quad \text{If } \phi(D)(\psi x.u) = P, \text{ then } u = (\psi x)^{-1}(\phi(D))^{-1}P$$

where  $P=P(x)$  [1848, 31].

Hargreave stated (53.3)-(53.4) as two evident theorems, the one independent from the other, without proof. As these two im-

portant theorems are nowadays known after Hargreave<sup>(3)</sup> we sketch their proof. Let

$$(53.6) \quad \varphi(D) = A_0 + A_1 D + \dots + A_r D^r + \dots$$

and  $\varphi(x)=u$ . Then, by Leibniz's theorem we have

$$\varphi(D)[u.u] = A_0(u.u) + A_1 D(u.u) + \dots + A_r D^r(u.u) + \dots =$$

$$A_0 u u + A_1 (u D u + u D u) + \dots + A_r (u D^r u + r D u D^{r-1} u + \frac{r(r-1)}{1.2} D^2 u D^{r-2} u + \dots + u D^r u) + \dots$$

$$= u A_0 u + u A_1 D u + \dots + u A_r D^r u + \dots + u' (A_1 u + \dots + r A_r D^{r-1} u + \dots) + \dots =$$

$u.\varphi(D)u + u'\varphi'(D)u + \dots$ , or (53.3) is proved.

For the proof of (53.4) let

$$(53.7) \quad u D u = D(u u) - D u u.$$

Substitute in (53.7)  $D u$  for  $u$  and by iteration we get a formula for  $u D^r u$ . Multiply these  $r$  formulae by  $A_0, A_1, \dots$  respectively and add them. Then by (53.6) we arrive at (53.4) for  $u, u, \varphi(D)$  instead of  $\varphi(x), u$ , and  $\psi(D)$ . The second mode of proof was applied for both theorems by Carmichael in [1855, 18-20]. Attributing these results to Hargreave, Carmichael observed that they prove that " $F'(D)$  bears the same relation in point of character to  $F(D)$  as  $F'(x)$  bears to  $F(x)$ " [1855, 19].

Stressing the assumption that  $\varphi(D)$  and  $\psi(D)$  in (53.3)-(53.4) are expressed in integral powers of  $D$ , Hargreave wrote:

In conformity with recognized principles of reasoning, when the subjects of the process are regarded merely as symbols, we may assume that these propositions are true generally; and we shall therefore not hesitate to pronounce any interpretable result derived from the free use of these theorems true, although the intermediate steps of the process are not capable of a rational interpretation<sup>(4)</sup>.

Having mentioned the proposition (53.5) as a first outcome of his theorems, Hargreave proceeded to another important observation: that if in (53.3) we put  $D$  for  $x$  and  $-x$  for  $D$  we obtain (53.4). Or, if a differential equation can be written in either of the two forms, (53.3)-(53.4), then changing  $x$  and  $D$  as above in both the equation and its symbolic solution, we obtain another differential equation accompanied by its symbolical solution. He added that an important condition for this procedure is that the solutions "are to be preserved in a symbolical form";

for it would be an error to write  $(x^{-1}).0=0$  if in a subsequent stage  $x$  is to be converted into  $D$  [1848, 31-32]. For convenience, let us summarize Hargreave's theorem of conversion as stated by him in [1850, 270-1]:

"That if, in a linear differential expression  $\phi(x,D)u=X$  and its solution  $u=\psi(x,D)X$ , the letter  $x$  be changed into  
(53.8) the operative symbol  $D$  and  $D$  into  $-x$ , we shall then obtain another linear differential expression  $\phi(D,-x)u=X$ , the solution of which will be  $u=\psi(D,-x)X$ ".

He let the equation of the first order

$$(53.9) \quad \phi x.Du + \psi x.u = X$$

which gives by solution

$$(53.10) \quad u = e^{-\sigma x} D^{-1} [e^{\sigma x} (\phi x)^{-1} X], \quad g x = e^{\int \frac{\phi x}{\phi x} dx}.$$

Effecting the transform put forward in (53.8), we have that

$$(53.11) \quad -\phi(D)(xu) + \psi(D)u = X$$

has by (53.10) and (53.8) the solution

$$(53.12) \quad u = -e^{-\sigma D} [x^{-1} e^{\sigma D} (\phi D)^{-1} X]$$

[1848, 32] (5).

But, by theorem (53.3) we can write equation (53.11) in the more convenient form

$$(53.13) \quad x\phi(D) + \lambda(D)u = X_0$$

if we put

$$(53.14) \quad \lambda(D) = \phi'(D) - \psi(D), \quad X_0 = -X.$$

Solving (53.14) for  $\psi(D)$  and effecting the operation  $g(D)$  by (53.10), we have after substitution in (53.12) the solution of (53.13)

$$(53.15) \quad u = [\phi(D)]^{-1} e^{\int \frac{\lambda(D)}{\phi(D)} dD} [x^{-1} e^{-\int \frac{\lambda(D)}{\phi(D)} dD} X_0].$$

This solution was given, mentioned Hargreave, by Boole in [1847e, 96] adding that "It indicates in a striking manner the interchange of symbols which are here proposed as a general theory [(53.8)]; and leads naturally to the inquiry, whether such a conversion may not be extended to other forms" [1848, 32; on Hargreave's notation see (5) above].

Among the subcases of (53.13) we have evidently the following "immediately interpretable" ones: first the equation

$$(53.16) \quad x\phi(D)u + m\phi'(D)u = X$$

which gives, if we put  $\lambda(D)=\phi'(D)$  in (53.15), the solution

$$(53.17) \quad u = [\phi(D)]^{m-1} x^{-1} [\phi(D)]^{-m} X,$$



and secondly equation

$$(53.18) \quad x\varphi(D)u + m \frac{\varphi(D)}{D}u = X$$

which gives respectively

$$(53.19) \quad u = [\varphi(D)]^{-1}D^m[x^{-1}D^{-m}X].$$

Notice that (53.16) was hinted at as integrable by Boole in his [1845d, 220; see (47.23)] and was consequently solved as a particular case of (53.13) in his [1847e, 96-97]<sup>(6)</sup>.

Hargreave's plan in [1848] has as follows:

In this paper I propose to apply the formula [(53.3)], by the aid of Mr. Boole's solution above given [(53.15)], to the discovery of soluble forms of linear equations with variable coefficients; I shall also show that by the use of the conversion of symbols [(53.8)], many forms of solution apparently incapable of interpretation may be made to give useful results; and I shall point out a remarkable connection between the solutions thus obtained and the solutions of the same equations in the form of definite integrals<sup>(7)</sup>.

He undertook first to solve equation (53.16) in the generalized form (53.2). Putting

$$(53.20) \quad \varphi(D) = D^2 + bD + c^2,$$

b, c constants, he determined  $\varphi(D)(\psi x.u)$  and  $\varphi'(D)(\psi x.u)$  via theorem (53.3) and substituting them in (53.2) he obtained the equation

$$(53.21) \quad D^2u + \left(b + \frac{2m}{x} \frac{\psi'x}{\psi x}\right) Du + \left(c^2 + \frac{bm}{x} + \left(b + \frac{2m}{x} \frac{\psi'x}{\psi x}\right) \frac{\psi''x}{\psi x}\right) u = (x\psi x)^{-1}X = P.$$

Since (53.16) gives by solution (53.17), if we consider (53.20) and (53.5) the solution of (53.21) is obtained in the form

$$(53.22) \quad u = (\psi x)^{-1} [D^2 + bD + c^2]^{m-1} (x^{-1} [D^2 + bD + c^2]^{-m} (x\psi x.P)).$$

In the case when  $P=0$ , he obtained by developing the right-hand side of (53.22) the solution of (53.21) in a series form [1848, 33].

The form (53.21), he claimed, "deserves particular attention, as it will be found to include the most remarkable of the equations of the second order, which have heretofore been integrated by artificial methods" [1848, 34]. Indeed, as he was to stress in [1850, 271; see 5.4, (1)], Hargreave's procedure, involving the theorem of conversion (53.8) and the formula (53.3), was devoid of any theorems peculiar to the calculus of operations and

artificial devices of reduction to simpler cases; nor did he suppress terms and factors of the equation under consideration as both Bronwin and Boole occasionally did. Moreover, the technique of a procedure based upon the solution of equation (43.1) was no more—as in the case of Gaskin, Ellis, Bronwin and Boole—the ultimate scope of the method to be implied from the very beginning<sup>(8)</sup>. As we shall see, given the far from complicated solution of (53.16), Hargreave could readily determine the solution of many important cases of (53.21) in the most lucid and direct way.

Hargreave put in (53.21)

$$(53.23) \quad \psi x = e^{ax} \quad \text{and then } a = b = 0$$

reducing it thus to the "well-known equation"

$$(53.24) \quad D^2u + \frac{2m}{x} Du \pm c^2u = P$$

the solution of which is according to (53.22) and (53.23),

$$(53.25) \quad u = (D^2 \pm c^2)^{m-1} [x^{-1} (D^2 \pm c^2)^{-m} (xP)].$$

For the case of (53.24) when the coefficient of  $u$  is positive and  $P=0$ , that is for the EFE in the form (43.15), the solution is

$$(53.26) \quad u = (D^2 + c^2)^{m-1} [x^{-1} (k \sin cx + k' \cos cx)].$$

The process used above is based upon Hargreave's general theory as applied to (53.21). However, he observed that (53.24) can be treated directly as a most simple subcase of (53.16) [if we make  $\phi(D)=D^2 \pm c^2$ ] by means of (53.17) [1848,34]<sup>(9)</sup>.

Through a different procedure, he obtained the solution of the EFE in the form (42.13) by letting in (53.21) and (53.22)  $\psi x = x^n$ ,  $b = 0$  and  $n = -m$ . Thus (53.21) becomes (42.13) or

$$(53.27) \quad D^2u \pm c^2u = \frac{m(m-1)}{x^2} u$$

and its solution accordingly (53.22) is obtained as

$$(53.28) \quad u = x^m (D^2 + c^2)^{m-1} [x^{-1} (k \sin cx + k' \cos cx)]$$

for the positive sign, and

$$(53.29) \quad u = x^m (D^2 - c^2)^{m-1} [x^{-1} (k e^{cx} + k' e^{-cx})]$$

for the negative respectively [1848,35]. Hargreave's form (53.29) was briefly mentioned by Glaisher as another form for the solution of the EFE in [1881,818; 4.2,(11)].

Letting in (53.21)

$$(53.30) \quad b = 0, \quad \frac{\psi'x}{\psi x} + \frac{m}{x} = Q$$

he reduced it to the "most convenient" form

$$(53.31) \quad D^2u + 2QDu + [c^2 + Q^2 + Q' - \frac{m(m+1)}{x^2}]u = P$$

where  $Q' = d/dx Q$ . Then, making

$$(53.32) \quad Q_1 = \int Q dx$$

we have

$$Q_1 = \int \frac{\psi'x}{\psi x} dx + \int \frac{m}{x} dx = \log \psi x \cdot x^m \quad \text{or} \quad \psi x \cdot x^m = e^{Q_1}, \quad \text{thus} \quad (\psi x)^{-1} = x^{-m} e^{-Q_1}.$$

Substituting the value of  $(\psi x)^{-1}$  in the solution of (53.21), (53.22), we have the solution of (53.31)

$$(53.33) \quad u = x^m e^{-Q_1} (D^2 + c^2)^{m-1} [x^{-1} (D^2 + c^2)^{-m} x^{-(m-1)} e^{Q_1} P]$$

[1848, 35] <sup>(10)</sup>.

Letting  $Q = n/x$  in (53.31), it was possible to deduce from (53.33) the solution of the Riccati equation after the additional transforms

$$(53.34) \quad \frac{dt}{dx} = x^{-2n} \quad \text{and} \quad z = -(2n-1)t.$$

As we have mentioned in 1.4, the Riccati equation is a transformation of the EFE and vice versa. Thus, it is hardly surprising that Hargreave obtained a solution closely similar in form to (53.28) [1848, 35-36] <sup>(11)</sup>.

In a similar manner he applied next the equation (53.18) in conjunction with the "original" theorem (53.3) by making first

$$(53.35) \quad \varphi(D) = D^2 + bD.$$

Considering linear equations of any order, he stated "it is obvious, however, that the generality of the soluble forms becomes less, as the order of the equation rises" [1848, 37]. He further studied the soluble cases of the "general equation of the second order"

$$(53.36) \quad \varphi x \cdot D^2 u + \psi x \cdot Du + \chi x \cdot u = P$$

through his principle (53.8) [1848, 38-41] <sup>(12)</sup>.

Omitting his work on partial differential and finite difference equations [1848, 42-44, 48-51] which is based directly on his inquiries on ordinary differential equations, we conclude with his use of definite integrals [1848, 44-48]. He stated that "it is well known that many of the differential equations in-

tegrated by the above processes, and whose integrals are in some cases capable of an expression merely symbolical by reason of the number of operations to be performed being fractional, may be integrated generally, when there is no second term, by means of definite integrals" [1848, 44].

To explain his conclusion that the symbolical forms given above are capable of "instantly", almost "mechanically", converted into the form

$$(53.37) \quad u = \int_a^b \phi z e^{zx} dx,$$

Hargreave took the known equation (53.24) for  $P=0$ , or,

$$(53.38) \quad D^2u + \frac{2m}{x}Du - c^2u = 0,$$

its solution by (53.25) being

$$(53.39) \quad u = k(D^2 - c^2)^{m-1} [x^{-1}(D^2 - c^2)^{-m}].$$

His assertion is that a solution of (53.38) in the form (53.37) is obtained by writing

$$(53.40) \quad \phi z = k(z^2 - c^2)^{m-1}$$

and "selecting the limits properly". Based upon the fact that

$$(53.41) \quad u = k \int_{-c}^c (z^2 - c^2)^{m-1} e^{zx} dz$$

is a partial solution, he obtained the complete integral of (53.38) for  $m$  fractional in the form [1848, 44-5; see also 10 above]

$$(53.42) \quad u = k \int_{-c}^c (z^2 - c^2)^{m-1} e^{zx} dz + k' x^{-2m+1} \int_{-c}^c (z^2 - c^2)^{-m} e^{zx} dz = \\ = k \int_{-1}^1 (z^2 - 1)^{m-1} e^{cxz} dz + k' x^{-2m+1} \int_{-1}^1 (z^2 - 1)^{-m} e^{cxz} dz.$$

We have seen so far, that several English analysts had attempted to integrate an equation in definite integral form, among them Murphy [3.3], Gaskin [4.2] and Bronwin [4.3]. The latter had tried in vain to amend Gaskin's incomplete integral of equation (53.38) or (42.15) given as (42.27) or (42.30). Glaisher provided the correct result in (42.32). Hargreave's formula (53.42) was incorporated in Glaisher [1881, 818] accompanied by the comments: "One or other of the definite integrals in [(53.42)] is however

always infinite, except when  $m$  lies between 0 and 1".

#### 5.4 The peak of generalization and abstraction; Hargreave and Bronwin on symbolical methods: 1850-1853.

With this section we complete our study of Bronwin's and Hargreave's work on differential equations. Their papers are above all characterized by a great tendency for abstraction. However, in their common pursuit for extension -on different lines- of Boole's general method, both writers realized the difficulty in reaching what Boole meant by "a genuine and complete method" in the realms of differential equations [5.2, (12)]. In principle, Hargreave is nearer to Boole as he does believe in the existence of a single general method [1850, 284; see (4) below]. However, he dispenses with artificial techniques peculiar to the calculus of operations. By contrast, Bronwin is very dependent on Boole's techniques and doubts the existence of a more general method. The main views of these two writers are evident in their long papers, Hargreave [1850] and Bronwin [1851a], published in the Philosophical Transactions. Both papers mark the climax not only of their authors' work in the realms of symbolic methods, but also of that of all English and Irish mid-19<sup>th</sup>-century writers on that subject.

We start with Hargreave's paper on "General methods in analysis for the resolution of linear equations in finite differences and linear differential equations" [1850]. As the title shows, Hargreave's aim is very close to Boole's own in [1844]. In fact, following a reverse order from that of Boole, he first investigated finite difference equations and next solved differential ones by reducing them to equations of the former kind. As the procedures are even more general and complicated than those followed by Boole in [1844], we will only focus on his most important claims and conclusions.

The first part of [1850] opened with the statement ;

With the exception of a few cases capable of solution by partial and artificial methods, there does not at present exist any mode of solving linear equations in finite differences of an order higher than the first; and with reference to such equations of the first order, we are obliged to be content with those insufficient forms

of functions which are intelligible only when the independent variable is an integer, and which may be obtained directly from the equation itself by merely giving to the independent variable its successive integer values. It is in this insufficient and qualified sense that the solutions here given are to be taken, and the first part of the following investigations may be considered as an extension of this form of solution from the general equation of the first order to the general equation of the  $n^{\text{th}}$  order<sup>(1)</sup>.

look

Hargreave <sup>A</sup> the equation of the first order

$$(54.1) \quad u_x = P_x u_{x-1} + G_x,$$

where the index  $x$  indicates a function of  $x$ , arriving at its solution

$$u_x = (P_x \dots P_m) \left[ u_{m-1} + \frac{G_m}{P_m} + \frac{G_{m+1}}{P_m P_{m+1}} + \dots + \frac{G_x}{P_m \dots P_x} \right] \quad \text{denoted by}$$

$$(54.2) \quad u_x = P_x \dots P_m \left[ \sum \frac{G_{x+1}}{P_m \dots P_{x+1}} + c \right],$$

"not properly by any analytical methods", but by giving to  $x$  the successive values  $x, x-1, \dots, m, m-1$  and "eliminating all values of  $u_x$ , between the first and the last" [1850, 262-3].

By applying the same process to the general equation

$$(54.3) \quad u_x = P_x u_{x-1} + Q_x u_{x-2} + \dots + Z_x u_{x-n} + G_x,$$

he arrived at an "exactly similar solution"

$$(54.4) \quad u_x = e^v \left[ P_x \dots P_m \left( \sum \frac{G_{x+1}}{P_m \dots P_{x+1}} + c \right) \right].$$

The exponent  $v$  "denotes the sum of distributive operations"  $V_1, \dots, V_{n-1}$  which are capable of being performed only upon factorial expressions containing consecutive values of  $P_x$ . Calling  $V_1$  as of not a "strictly algebraic character", he went on to define them. For example,  $V_1$  denotes an operation of the following properties

$$(54.5) \quad V_1(P_{x-1}P_x) = Q_x, \quad V_1^2(P_{x-1}P_x) = 0.$$

Further  $V_{n-1}$  denotes the change of  $P_{m-n+1} \dots P_m$  into  $Z_m$ . Under these definitions and properties, the solution (54.4) was provided after a complicated procedure in the series form

$$(54.6) \quad M_0 G_x + \dots + M_p G_{x-p} + \dots$$

where

$$(54.7) \quad M_0 = 1, \quad M_p = e^v (P_{x-p} \dots P_x)$$

[1850, 262-267] (2).

Hargreave's obscure theory was applied to several equations, among them equation

$$(54.8) \quad u_x + a_1 P_x u_{x-1} + a_2 P_x P_{x-1} u_{x-2} + \dots + a_n (P_x \dots P_{x-n+1}) u_{x-n} = G_x.$$

Equation (54.8) was reduced via (54.6)-(54.7) to a set of first order equations

$$(54.9) \quad \begin{aligned} u_x - a P_x u_{x-1} &= a^{n-1} A G_x \\ u_x - b P_x u_{x-1} &= b^{n-1} B G_x \\ &\dots \dots \dots \end{aligned}$$

where a, b, A, B, ... numerical constants given by the partial fractional expansion

$$(54.10) \quad (t^n + a_1 t^{n-1} + \dots + a_n)^{-1} = \frac{A}{t-a} + \frac{B}{t-b} + \dots;$$

Hargreave's condense process lacked clarity [1850, 270].

Appealing to his theorem of conversion, he opened the second part of his paper by applying (53.8) to equation (54.8). Indeed, bearing in mind that

$$(54.11) \quad u_{x-n} = e^{-nD} u_x, \quad D = d/dx$$

the proposed interchange of x and D in (54.8) gives Boole's equation (45.35), or

$$(54.12) \quad u + a_1 \phi(D) (e^x u) + \dots + a_n \phi(D) \dots \phi(D-n+1) (e^n u) = G,$$

where  $P_x$  in (54.8) is now replaced by  $\phi x$ , whose solution depends -according to (54.9)- upon that of (45.40), that is upon

$$(54.13) \quad u - a \phi(D) (e^x u) = a^{n-1} A G.$$

For Hargreave this was an illustration of the "remarkable property of this mechanical interchange of symbols, that it simultaneously convert a linear equation in finite differences into a linear differential equation; so that wherever the former is soluble, the latter is soluble also, provided the result be intelligible, a condition always satisfied when the functions employed are rational algebraical functions" [1850, 271].

Hargreave felt proud of this "singular analytical process" of interchange of x and D, stressing that so far as the process is legitimate, "it is to be observed that it is founded on reasoning of a purely analytical character. It does not in any manner whatever flow from the calculus of operations, or depend for its validity upon the soundness of the logical basis on which this calculus rests". He added below (54.12)-(54.13) that these formulae consist "a proposition established by Mr. Boole by the

methods of the Calculus of Operations" [1850, 271; see (45.35)-(45.40)].

After Boole, Hargreave reduced a linear differential equation of the  $n^{\text{th}}$  order with polynomial coefficients (45.21) to its symbolical form (45.23). Then, by (53.8) he reduced the latter to an equation of the form (54.3) which gives by solution (54.6). The latter was accordingly transformed to a particular solution in series of the initial differential equation under consideration. His basic theorem (53.3) was made use of, as well as the known formula (45.20) [1850, 271-275]. The rest of the paper consisted of specific cases of (45.21), and still of a general nature, most of them taken from Boole [1844; 1850, 275-284]<sup>(3)</sup>.

Hargreave claimed that the material developed by Boole in [1844] is "closely connected" with the subject matter of his paper, "though the methods exhibited are distinct". This fact should prove,

that the interchange of the symbol of operation and the independent variable [(53.8)], and the general relation exhibited by Mr. Boole's fundamental theorem of development [(45.26)] connecting any system of linear differential equations with a corresponding system of equation in finite differences, are merely different representations of a part of some more general method or process<sup>(4)</sup>.

Stimulated by Boole, Hargreave adopted his general spirit but avoided techniques founded on the calculus of operations, offering thus a kind of complementary study to Boole's [1844]. He was the only one to produce a "genuine", though "incomplete" general method in analysis as alternative to Boole's own<sup>(5)</sup>.

Bronwin's elaborate paper "On the solution of linear differential equations" [1851a] opened as follows:

If we consider the very different forms which the solutions of differential equations differing very little from each other frequently take, and the very different processes often required in each particular case to obtain the solution, we shall be led to conclude that the discovery of any universal or general method of solving them must be a hopeless case. We cannot therefore regard particular methods, especially when applicable to a large number of cases, as useless speculations. The present paper contains the solution of several classes of these equations effected by means of general theorems in the Calculus of Operations adapted to each par-



ticular class<sup>(6)</sup>.

This paper, followed by two brief ones with applications [1851b,1852], crowns Bronwin's inquiries as shaped in 1846-1848 [5.2]. Following on these lines he not only generalized and improved over Boole's method [5.2,(15)], but by the invention of new theorems he effected the integration of a large variety of symbolical equations which "when reduced to the ordinary form, are very complex; but when particular forms are assigned to the arbitrary functions, results sufficiently simple may be obtained" [1851a,477]. To a large extent the raw material of Bronwin's work is borrowed from Boole [1844;1845d;1847c]; he also implicitly drew on Hargreave [1848]. We now present his main theorems and procedures hinting at their application.

Bronwin let first the operations

$$(54.14) \quad \omega = \varphi D + \lambda, \quad \tau = e^{\int \frac{dx}{\varphi}}$$

where  $\varphi$  and  $\lambda$  functions of  $x$  [as in 5.2,(8)]. Claiming that

$$(54.15) \quad (\omega+k)u = \tau^{-k}\omega\tau^ku,$$

can be easily verified, he proved inductively

$$(54.16) \quad f(\omega+k)u = \tau^{-k}f(\omega)\tau^ku,$$

for any polynomial function  $f(\omega)$  [1851a, 461-2]<sup>(7)</sup>.

Notice that (54.15) and (54.16) are variants on (52.28) and (52.29) respectively.

Calling (54.16) his "first general theorem in the calculus of operations", Bronwin applied it to various equations, a representative case being

$$(54.17) \quad f(\omega)\omega u + pf_1(\omega)(\omega+nk)\tau^ku = X,$$

$p$  constant,  $X=X(x)$  and  $f, f_1$  polynomial functions of  $\omega$ . He then let

$$(54.18) \quad u = (\omega+k)\dots(\omega+nk)v,$$

determined accordingly  $\tau^ku$  by (54.16) and (54.18), and substituting in (54.17) he obtained

$$(54.19) \quad f(\omega)v + pf_1(\omega)\tau^kv = \omega^{-1}(\omega+k)^{-1}\dots(\omega+nk)^{-1}X.$$

Equation (54.19) is one order lower than the proposed (54.17). Thus all equations of the second order included in (54.17) may be considered as integrated by this process [1851a, 462-3].

Bronwin's procedure is an extension and sophistication over his early symbolical approach in 1846 [see (52.15)-(52.20)]. He observed that the factorial expression at the right-hand side of (54.19) can be resolved into partial fractions and that the

operations that thus result are to be performed "by a well known theorem due to Mr. Boole [(47.2)]" as follows [1851a, 462] :

$$(\omega+m)^{-1}X = (\varphi D + \lambda + m)^{-1}X = (D + \frac{\lambda+m}{\varphi})^{-1}\varphi^{-1}X = e^{-\int \frac{\lambda+m}{\varphi} dx} D^{-1} e^{\int \frac{\lambda+m}{\varphi} dx} \varphi^{-1}X.$$

By means of Taylor's theorem, he proved

$$(54.20) \quad f(\omega'+k)u = \tau'^{-k}f(\omega')\tau'^{-k}$$

where  $\omega'$  and  $\tau'$  stand for the operations  $\omega$  and  $\tau$  defined by (54.14) when  $D$  and  $x$  are interchanged. After demonstrating (54.20), which is the dual theorem of (54.16), he observed that (54.20) follows immediately from (54.16) if we change

$$(54.21) \quad x \rightarrow D, \quad D \rightarrow x \quad \text{and} \quad e \rightarrow e^{-1}$$

[1851a, 467]. The interchange (54.21) is Hargreave's theorem of conversion (53.8), to which Bronwin did not refer since he replaced it by an independent direct proof.

The application of the second theorem (54.20) demanded the dual of Boole's theorem (47.2) used above, (47.1). Thus, the factors of the form  $(\omega'+rk)^{-1}$  are written as

$$(\omega'+rk)^{-1} = [\varphi(D)x + \lambda(D) + rk]^{-1} = [x + \frac{\lambda(D) + rk}{\varphi(D)}]^{-1}\varphi(D)^{-1} \text{ or}$$

$$(54.22) \quad (\omega'+rk)^{-1} = e^{g(D)}x^{-1}e^{-g(D)}\varphi(D)^{-1}$$

where

$$(54.23) \quad g(D) = \int \frac{\lambda(D) + rk}{\varphi(D)} dD.$$

Stressing for once the interpretability of symbolic forms, Bronwin assumed that  $\varphi(D)$ ,  $\lambda(D)$  and  $\tau'^{-k}$  must be rational functions of  $D$ . Moreover, we should have

$$(54.24) \quad e^{\int \frac{\lambda(D)}{\varphi(D)} dD} = \Phi(D)e^{mD},$$

where  $\Phi(D)$  a rational function of  $D$  and  $m$  constant. Solving (54.24) for  $\lambda(D)$  it follows that its most general <sup>form</sup> can be

$$(54.25) \quad \lambda(D) = \frac{\varphi(D)}{\Phi(D)}(m\Phi(D) + \Phi'(D))$$

[1851a, 468] (e).

If  $\rho$  and  $n_1$  are defined by

$$(54.26) \quad \rho = e^{\int \frac{\lambda}{\varphi} dx}, \quad n_1 = \varphi D + \psi + i\lambda,$$

where  $\lambda, \varphi, \psi$  are functions of  $x$  then, the third theorem amounts to

$$(54.27) \quad \rho^n f(n_{m+n})u = f(n_m)\rho^n u;$$

its dual for D interchanged with x deduced separately as for the second theorem (54.20). An immediate outcome of (54.26) was

$$(54.28) \quad \pi_m \pi_n u = \pi_n \pi_m u + (n-m) a p u$$

where a constant [1851a, 470-1] (9).

Among Bronwin's main concerns in this paper was the solution of the equation

$$(54.29) \quad \pi_m \pi_n u + p p u = X,$$

particular cases of which were solved by Boole [1847c] and then by Bronwin [1848c]. Putting

$$(54.30) \quad u = \pi_{m+1} u_1,$$

equation (54.29) was reduced by means of the theorems (54.27) and (54.28) to the form

$$(54.31) \quad \pi_{m+1} \pi_n u_1 + p_1 p u_1 = \pi_m^{-1} X; \quad p_1 = p - a(n-m-1).$$

After i transformations we have

$$(54.32) \quad \pi_{m+i} \pi_n u_1 + p_i p u_1 = \pi_{m+i-1}^{-1} \dots \pi_m^{-1} X$$

easily solved if  $p_i = 0$ . Reducing (54.29) to the ordinary [non-symbolic] form he observed that after substitution of the specific values of  $\phi$  and  $p$  it would become "very complicated" [1851a, 472-3].

Omitting the last two theorems which are very awkward in form and too abstract for any useful application [1851a, 478-482], we conclude our study of Bronwin's paper with few of his observations. Taking an instance of Hargreave's theorem (53.4) for  $\phi(D) = D^n$ , he applied it, following Hargreave [1848], to the solution of the equation

$$(54.33) \quad \psi_2 D^2 u + \psi_1 D u + (\psi_1' + \psi_2'') u = X$$

obtaining

$$(54.34) \quad u = \psi_2 e^{-\int \frac{\psi_1 dx}{\psi_2}} D^{-1} \psi_2^{-2} e^{\int \frac{\psi_1 dx}{\psi_2}} D^{-1} X.$$

He noticed that (54.33) is "immediately integrable" if put in the form

$$(54.35) \quad \psi_2 D u + (\psi_1 - \psi_2') u = D^{-1} X$$

[1851a, 477-8] (10).

On similar lines Bronwin commented on Boole's equation (45.35) extending it as with the case of (52.53) [1851a, 475-7]. At another instance he observed the facility with which equations of the form solvable by the first theorem (54.15) can be reduced to forms solvable by its dual (54.20) by the conversion of symbols (54.21), a fact which is "not a little remarkable" [1851a, 468]. He finally observed that "the method which has been applied

to the solution of each particular class of equations will not apply to either of the other classes", adding:

We see the same thing when employing other methods, and we see no reason to suppose that our means of integrating equations will ever be greatly extended otherwise than by multiplication or aggregation of particular methods. Such methods therefore ought not to be considered as possessing little interest. The same thing may be inferred from the various conditions of integrability at which we arrive in those cases where we can treat the same sort of equations by different methods<sup>(11)</sup>.

Slightly modifying the first two general theorems (54.16) and (54.20), Bronwin applied them in his next paper [1851b] to differential equations of the form

$$(54.36) \quad X = f(n)u + f_1(n)xu + f_2(n)x^2u + \dots,$$

and to their duals with  $x$  replaced by  $d/dx = D$ . All the equations tackled in this paper remained as abstract and general as those treated in his [1851a]; however in his last paper Bronwin [1852] resumed again his favourite theme, that is the solution of those equations cases of which are second order ones prominent in physics like those tackled in his early papers [4.3; 5.2].

He opened his [1852] with the equation

$$(54.37) \quad xD^m u - p m D^{m-1} u + k x u = 0,$$

$k, p$  integers, observing that "By the conversion of symbols this may be solved as an equation of the first order, but it serves to show the use which may be made of the symbolical function  $\lambda(D)$ " in the dual of (54.14). He thus put

$$(54.38) \quad \omega = Dx + \lambda(D)$$

and suitably substituting in (54.37) he obtained

$$(54.39) \quad \omega D^m u - \lambda(D) D^m u - p m D^m u + k \omega u - k \lambda(D) = D0.$$

Then he assumed

$$(54.40) \quad \lambda(D) D^m + p m D^m k \lambda(D) = 0$$

which by solution gives the wanted value of  $\lambda(D)$ ; accordingly

$$(54.39) \text{ is reduced to the form}$$

$$(54.41) \quad u = (D^m + k)^{-1} \omega^{-1} D0.$$

Determining  $\omega^{-1}$  by means of (54.38) and the solution of (54.40), he operated by it on  $D0$  by means of Boole's theorem (47.1) arriving finally to the solution of the initial equation

$$(54.42) \quad u = (D^m + k)^{-p-1} x^{-1} (D^m + k)^p 0$$

[1852, 187-8].

As Bronwin mentioned equation (54.37) was solved initially by Ellis in [1841b, 193; see (43.14)]. Notice also the similarity in form between (54.42) and the solution of (52.2) [which is (54.37) where  $m=2$  and  $p=-p$ ] given properly by Hargreave and Boole in 1847 as (52.8). Bronwin went on to comment upon the factor  $(D^m+k)$  remarking that we cannot immediately operate with the form (54.43)  $\omega = xD + \lambda(D)$ ,

which is a case of (54.20), but with

$$(54.44) \quad \omega = Dx + \lambda(D) = xD + 1 + \lambda(D)$$

[1852, 188].

Setting  $\omega$  as in (54.44) he effected next the integration of

$$(54.45) \quad x^2 D^m u - p(p-1) D^{m-2} u + kx^2 u = 0$$

when  $p$  or  $p-1$  is divisible by  $m$ . Again this equation was treated by Ellis [(43.13)] as one dependent upon the form (43.1) [see 5.3,(8)]. Notice also that for  $m=2$ , it becomes (42.13), the EFE in its general form. Bronwin repeatedly stressed -regardless of Hargreave- that "It will often happen that the conversion of symbols will very greatly facilitate the solution of an equation" [1852,188-191]. As far as applications are concerned this is the most important of Bronwin's papers as based on an ingenious combination of Boole's and Hargreave's techniques sophisticatedly provided in his [1851a].

We conclude this section with a few words on Hargreave's paper "Applications of the calculus of operations to algebraical expansions and theorems" [1853] published, like Bronwin's [1851b; 1852] in the Philosophical Magazine. Hargreave opened his paper by introducing his theorem (53.3) as "a leading theorem in the Calculus of Operations" but still as one which is not derived from any principles peculiar to this calculus [1853, 351-352]. He next proposed "to denote the operation of passing from any function of  $D$  to its derived function by the symbol  $\nabla$ ", or

$$(54.46) \quad \varphi'(D) = \frac{d(\varphi D)}{dD} = \nabla(\varphi D).$$

It thus follows that

$$(54.47) \quad (a_0 + a_1 \nabla + \dots + a_n \nabla^n) \varphi D = a_0 \varphi D + \dots + a_n \varphi^{(n)} D$$

and

$$(54.48) \quad e^{\pm n \nabla}(\varphi D) = \varphi(D \pm n)$$

-among other evident results. It is "scarcely necessary to remark", he added that " $\nabla$  obeys the algebraical law of indices.

and is distributive in its operation" (1853, 352-3;  $\phi D$  stands for  $\phi(D)$ ).

Hargreave argued that we "treat  $x$  and  $\nabla$  as constants to each other; but it must be remembered that  $\nabla$  and  $D$  stand in the same relation to each other as  $D$  and  $x$ , and are therefore not commutative". Under these considerations theorem (53.3) can be "placed under the condensed symbolical form"

$$(54.49) \quad \phi(D)(\psi x.u) = e^{D\nabla}(\psi x.\phi(D)u).$$

He then observed that (54.49) leads to (53.4) by means of the transform (53.8). Moreover, in its expanded form (54.49) is

$$(54.50) \quad \phi D(\psi x.u) = \psi \nabla(\phi D)u + x\psi' \nabla(\phi D)u + \frac{x^2}{2}\psi'' \nabla(\phi D)u + \dots$$

at the right-hand side of which  $\psi x$  is extricated from all operations; "thus  $\psi x$  is no longer a part of the subject of operation, but  $\psi$  becomes an instrument in determining the form of the operations to be performed on  $u$ " [1853, 353-4].

Let  $x$  in  $\psi x$  be replaced by  $a$ ; then the expansion in (54.49) will produce a series in powers of  $a$ , "the coefficients of which are functions of  $x$  determinable by means of known operations performed upon  $u$ ". On the other hand,  $\phi D(\psi x.u)$  represents, upon this substitution, the following operation: multiplication by an expression containing  $x$  and  $a$ ; differential operations with regard to  $x$ ; lastly, the change of  $x$  into a specific value  $a$  whenever  $x$  appears explicitly,  $u$  remaining throughout an implicit functions of  $x$ . If this operation is denoted by  $[\phi D.\overline{\psi(x,a)}]$ , then we have from (54.50):

$$(54.51) \quad [\phi D.\overline{\psi(x,a)}]u = \psi \nabla(\phi D)u + a\psi' \nabla(\phi D)u + \dots = \psi(\nabla + a)(\phi D)u. \\ [1853, 354].$$

Hargreave discussed the utility of (54.50) and then applied it in the form

$$(54.52) \quad \phi D[\psi x.u](\text{when } x=a) = \psi(\nabla + a)(\phi D)u.$$

By means of (54.52), particularly for  $a=0$ , he proved one after the other most of the formulae prominent in the calculus of finite differences and differentials, such as Burmann's theorem, Lagrange's and Herschel's theorems, the expansion of  $x/e^x - 1$  and  $(e^x + 1)^{-1}$  [1853, 355-363] (12).

We end with Hargreave's own concluding remarks:

It will be seen from the examples which have been given that the object has been to exemplify the peculiar powers of the new process

rather than to exhibit any new result, though many of those above given will be found to present themselves in a new form<sup>(13)</sup>.

Unfortunately and unluckily the work of Hargreave and Bronwin discussed in this section was neither reproduced nor referred to in later works on the calculus of operations. The only instance cited in recent historical studies is Hargreave's definition of  $\nabla$  in (54.45) by Koppelman [1971, 202]; see also 5.2.(17)].

#### 5.5 Donkin and Graves on non-commutative operations: 1847-1857.

Boole's and Bronwin's expansions of functions of non-commutative operations, together with Hargreave's interchange of the operators  $x$  and  $d/dx$ , were to have a strong impact on Donkin and Graves respectively. Donkin's generalization of known symbolic relations between distributive operations was incorporated in his paper "On certain theorems in the calculus of operations" [1850] published in the Cambridge and Dublin Mathematical Journal. Graves, on the other hand, contributed several papers between 1847-1857; our main source for the material under study here forms the summary of his papers in the Proceedings of the Royal Irish Academy between 1847-1857<sup>(1)</sup>.

the work produced by these two figures is similar -if not identical- but apparently Graves worked independently from Donkin. We first give a summary of Donkin [1850], concluding with the most important aspects of Graves's work.

Born in Oxford in 1814, W.F. Donkin was by profession an astronomer at Oxford University. He was also keen in music theory, known in acoustics and particularly in harmonic analysis. Closer to mathematics by profession than Hargreave, he was also an occasional mathematician . produce interesting papers on pure mathematics. Besides his [1850], the most abstract of his papers, he contributed an elaborate one on the LE in the Philosophical Transactions [1857]. Donkin's treatment of the LE -including a study of the EFE- is a direct outcome of Boole's influence, showing additionally Donkin's originality as a mathematician. Postponing the discussion of his work on the EFE and LE to 5.6 and 5.8 respectively, we confine here to his [1850].

Donkin opened his paper with a general study of two operations  $\pi$  and  $\rho$  which operate upon a subject  $u$  according to the law

$$(55.1) \quad n p u - p n u = p_1 u.$$

As with the studies discussed so far, the operations  $n$  and  $p$  are implicitly assumed to be distributive;  $p_1$  defined by means of  $n$  and  $p$  via (55.1). For example, let  $n=D$ ,  $p=x$  and  $u=f(x)$ . Then  $(Dx)f(x) - (xD)f(x) = f(x) = p_1 f(x)$ , or  $p_1 = 1$ . Dropping the subject  $u$ , Donkin deduced from (55.1)

$$(55.2) \quad \begin{aligned} n p - p n &= p_1 \\ n p_1 - p_1 n &= p_2 \\ &\dots\dots\dots \\ n p_n - p_n n &= p_{n+1} \end{aligned}$$

[1850, 10].

Since

$$(55.3) \quad \left(-\frac{1}{p} n p\right)^2 = \frac{1}{p} n p \frac{1}{p} n p = \frac{1}{p} n^2 p,$$

it follows easily by iteration that

$$(55.4) \quad f\left(-\frac{1}{p} n p\right) = \frac{1}{p} f(n) p$$

for any polynomial function  $f^{(2)}$ . Moreover, from (55.1), if  $u$  is dropped, we obtain

$$(55.5) \quad \frac{1}{p} f(n) p = f\left(n + \frac{1}{p} p_1\right), \quad p f(n) \frac{1}{p} = f\left(n - p_1 \frac{1}{p}\right).$$

Again, equations (55.2) give by iteration

$$(55.6) \quad n^n p = p n^n + n p_1 n^{n-1} + \frac{n(n-1)}{1.2} p_2 n^{n-2} + \dots + p_n,$$

and a similar formula for  $p n^n$  [1850, 12-13].

Now, formula (55.6) is immediately generalized into

$$(55.7) \quad f(n) p = p f(n) + \frac{p_1}{1} f'(n) + \frac{p_2}{1.2} f''(n) + \dots$$

for any polynomial function  $f$ , while its analogue for  $p f(n)$  can be derived from that for  $p n^n$ . Similarly from (55.5) we obtain, based on (55.7),

$$(55.8) \quad f\left(n + \frac{1}{p} p_1\right) = f(n) + \frac{1}{p} p_1 f'(n) + \dots$$

and its analogue for  $f(n - p_1 1/p)$  [1850, 10, 14].

This general and abstract theory, derived on so simple lines, render redundant the previous complicated proofs of impor-



tant. known theorems. For example, since

$$e^{-\varphi(x)} \frac{d}{dx} e^{\varphi(x)} u = e^{-\varphi(x)} \left[ e^{\varphi(x)} \frac{d}{dx} u + u \varphi'(x) e^{\varphi(x)} \right],$$

we have evidently

$$(55.9) \quad \left[ \frac{d}{dx} + \varphi'(x) \right] u = e^{-\varphi(x)} \frac{d}{dx} e^{\varphi(x)} u.$$

Hence, by (55.9) and (55.4) we have

$$(55.10) \quad f\left(\frac{d}{dx} + \varphi'(x)\right) u = e^{-\varphi(x)} f\left(\frac{d}{dx}\right) e^{\varphi(x)} u,$$

that is Boole's basic formula of development (47.2). Omitting computational details, Boole attributed the possibility to infer (55.10) from (55.9) to Donkin in his [1877, 454; 4.5, (5)].

Donkin wrote that particular cases of (55.5) are the "known theorems"

$$(55.11) \quad \frac{1}{X} f(D) X = f\left(D + \frac{X'}{X}\right), \quad X f(D) \frac{1}{X} = f\left(D - \frac{X'}{X}\right)$$

where  $X=X(x)$  and  $X'$  its derivative  $dX/dx$  [1850, 13]. In fact, the first of (55.11) is formula (55.10) if we make in the latter  $\varphi(x)=\log(X(x))$ . More known formulae can be derived; for example, if we let in (55.2)

$$(55.12) \quad n = D \quad \text{and} \quad \rho = X, \quad \text{then} \quad \rho_1 = X', \dots$$

Substituting these values in (55.8) we have readily

$$(55.13) \quad f\left(D + \frac{X'}{X}\right) = f(D) + \frac{X'}{X} f'(D) + \frac{X''}{X} \frac{f''(D)}{1.2} + \dots$$

which is Bronwin's formula (52.52), if in the latter we read  $X'/X$  for  $\varphi(x)$  [1850, 11; Bronwin 1848a; 5.2].

Donkin attributed to Bronwin the formula (55.13)—among other similar results. However, he claimed that particular results, such as (55.13), had occurred to him before he was acquainted with Bronwin [1848b]. But it was after reading Bronwin's work that he got motivated to generalize over it and produce the formulae (55.1)–(55.8) by means of which known results were easily obtainable [Donkin 1850, 10–11].

Donkin concluded his paper noticing that the developments of  $(1/\rho) \cdot \rho_n$ ,  $\rho_n \cdot (1/\rho)$  are "remarkable". Taking under account the formulae (55.2) and (55.4) he found after letting

$$(55.14) \quad \frac{1}{\rho} n p = \sigma \quad \text{and} \quad p n \frac{1}{\rho} = \tau$$

that  $(1/\rho).p_n$  and  $p_n.(1/\rho)$  "are found by developing  $(\sigma-n)^n$ ,  $(n-\tau)^n$  as if the symbols are commutative, with the condition that in each term of the first,  $\sigma$  must precede  $n$ , and in each term of the last  $n$  must precede  $\tau$ ", denoting the latter operations by  $(\sigma-n)_n$  and  $(n-\tau)_n$  respectively [1850, 15].

Under these observations, theorem (55.8) can be written as

$$(55.15) \quad f(\sigma) = f(n) + (\sigma-n)f'(n) + \frac{(\sigma-n)^2}{1.2} f''(n) + \dots$$

a true formula "whatever be the operations denoted by  $n, \sigma, \tau$ ; for, the two operations  $\sigma$  and  $n$  being given, there must exist some operation  $\rho$  such as to verify the relation"  $\sigma = (1/\rho)np$ . Thus formula (55.15) corresponds to Taylor's theorem [1850, 15]:

$$(55.16) \quad f(a) = f(b) + (a-b)f'(b) + \frac{(a-b)^2}{1.2} f''(b) + \dots$$

Having produced (55.15)-(55.16), he remarked:

It seems to result from these investigations, and from the researches of Mr. Boole (4.1), Mr. Bronwin (5.2), and others, in the same department, that there is much more analogy that might have been expected a priori between the laws of commutative and non-commutative symbols<sup>(3)</sup>.

Thus, following on the lines of Bronwin [1848a], Donkin established a simple, general method by means of which many symbolic theorems in the realms of the differential and integral calculus can be obtained. His main novelty lies in the formulae (55.1)-(55.5) from which the rest of his theorems follow. His paper was too abstract and devoid of any applications to make impact. However he was acknowledged by Carmichael [1855, 132-4] and Boole [1877, 454; see (55.10)] for the laws (55.2), to be mentioned for the same reason in Koppelman [1971, 203]<sup>(4)</sup>.

An independent study of non-commutative operations was carried out in the mid 1850's by Graves. While Donkin had generalized over Bronwin's theorems, Graves produced an excellent study on the lines of Hargreave on the interchange of symbols. Besides generalizing over the latter's results, establishing the theorem of conversion (53.8) in a new, rigorous way, Graves

showed great concern for interpretation of symbolic formulae, as well as for applications of such forms to ordinary and partial differential equations and analytic geometry.

Born in Dublin in 1812, Charles Graves graduated from Trinity College in 1834 as the first senior moderator and gold medallist in mathematics and mathematical physics. In 1834 he was appointed professor of mathematics in the University of Dublin, and died in 1899 after obtaining several distinctions of honour. He was a brother of the jurist and mathematician J.T.Graves -mentioned in our study of functional equations [2.7,(12)]- both being in friendly terms with W.R.Hamilton (mentioned in connection with Herschel's theorem in 2.3). C.Graves had only one memoir published on conic sections, much admired by Sylvester, while several papers on the calculi of operations, variations and quaternions were published by the Royal Irish Academy. Moreover, Graves held a correspondence with Boole on logic offering helpful suggestions [7.1,(10); 7.6,(5)-(6); 8.3,(4)]. He was much esteemed for his admirable tact and temper and it is said that he had "much literary and artistic taste, and so to these were largely due the symmetry and elegance, both of method and results, which are marked characteristic of his mathematical work" (5).

Due probably to a lack of publications in the principal journals of his time [see (1) and (5) above]. Graves is nowadays known only for his defence of the calculus of operations against Young's criticism, as in his [1849; 4.4 above (9)], and for a symbolic formula (55.45) involved in a treatment "almost identical with that in modern texts on quantum theory, no less rigorous, and no more" [Cooper 1952,11]. Though both these contributions are essential, a study of his actual mathematical work is missing and we will partly fill this gap by discussing first <sup>this study of non-commutative</sup> postponing <sup>five</sup> samples of his work on the EFE, analytic geometry and the LE until 5.6, 5.7 and 5.8 respectively.

We start with Graves's note "on the development of a function in factorials" published as [1847; see (1) above] in which he linked Herschel's study of finite differences with the operator  $xd/dx$ . He noticed that Herschel's methods for developing a function  $F(x)$  in a series of factorial terms furnish "interesting examples of the use of separations"; and "it is for this latter reason, rather than on account of any novelty in the results arrived at", that the following study was published

[1847, 456].

Graves opened his paper with the known formula

$$(55.17) \quad F(x+n) = (1+\Delta)^n F(x)$$

which expanded about  $x=0$  gives

$$(55.18) \quad F(n) = F(0) + \frac{\Delta F(0)}{1}n + \frac{\Delta^2 F(0)}{1.2}n(n-1) + \dots$$

A particular case of (55.18) is the theorem

$$(55.19) \quad x^n = \frac{\Delta 0^n}{1}x + \frac{\Delta^2 0^n}{1.2}x(x-1) + \dots,$$

"commonly given in treatises on the calculus of finite differences" [1847, 456]. He then assumed the evident equation

$$(55.20) \quad x^n n = x \frac{d}{dx} x^n,$$

a consequence of which is

$$(55.21) \quad x^n F(n) = F\left(x \frac{d}{dx}\right) x^n,$$

where  $F$  a polynomial function [1847, 457; see (2) above]. The application of theorem (55.21) led to several interesting results.

He first wrote  $1+x-1$  instead of  $x$  in (55.21) and expanded the right-hand side by the binomial theorem. Putting next  $x=1$  he obtained the development of  $F(n)$  in the desired factorial form. Comparing the expansion of (55.21) and (55.19) he found

$$(55.22) \quad \Delta^m F(0) = F(m) - mF(m-1) + \frac{m(m-1)}{1.2}F(m-2) - \dots,$$

"a formula which might be obtained directly by making  $x=0$  in the fundamental equation of the calculus of finite differences" (22.10); the latter giving the expansion of  $\Delta^m u_x$  [1847, 457]. Notice the similarity between (55.22) and formula (22.14) which gives Brinkley's  $\Delta^n 0^m$  numbers.

By means of  $xd/dx$  "we may obtain another interesting development"; in virtue of (55.21) we have

$$(55.23) \quad e^{hx \frac{d}{dx}} x^n = e^{hx} x^n = (e^h x)^n.$$

It is thus plain that the symbol  $e^{hnd/dx}$  operates on any function of  $x$  by changing  $x$  into  $e^h x$ ; or

$$(55.24) \quad F(e^h x) = e^{hx \frac{d}{dx}} F(x),$$

where by development [putting for convenience  $xd/dx=D$ ] we get

$$(55.25) \quad F(e^h x) = F(x) + \frac{DF(x)}{1} \cdot h + \frac{D^2 F(x)}{1.2} h^2 + \dots$$

Thus "As Taylor's theorem gives the altered state of  $F(x)$ , after  $x$  has received an increment  $h$ , so the theorem just announced exhibits the new value of  $F(x)$  after  $x$  has been multiplied by a number whose logarithm is  $h$ " [1847, 458; see also (3) above].

Graves stressed the difference between  $x^2 d^2/dx^2$  and  $(xd/dx)^2$ , the latter to be read as  $(xd/dx)(xd/dx)$ . He claimed that the relation between  $x^n d^n/dx^n$  and  $xd/dx$  can be directly deduced from (55.21) as

$$(55.26) \quad x^n \frac{d^n}{dx^n} = \left(x \frac{d}{dx} - 1\right) \left(x \frac{d}{dx} - 2\right) \dots \left(x \frac{d}{dx} - n + 1\right)$$

[1847, 458]. Indeed, if we let

$$(55.27) \quad F\left(x \frac{d}{dx}\right) = x \frac{d}{dx} - n$$

we have by making  $n=1, \dots, n-1$  in (55.27) and (55.21) the relation

$$(55.28) \quad x^i x \frac{d}{dx} = \left(x \frac{d}{dx} - i\right) x^i.$$

Thus in virtue of (55.28)

$$x^n \left(\frac{d}{dx}\right)^n = x^n \frac{d}{dx} \dots \frac{d}{dx} = \left(x \frac{d}{dx} - n + 1\right) x^{n-1} \overset{n-1 \text{ times}}{\frac{d}{dx} \dots \frac{d}{dx}},$$

and by continuing the same process we arrive at the known formula (55.26) in a most direct way [see also (33.57) and (45.20)].

He concluded his note by mentioning Herschel's theorem (23.18) in the form

$$(55.29) \quad F(e^h) = F(1) + \frac{h}{1} F(1+\Delta)0 + \frac{h^2}{1.2} F(1+\Delta)0^2 + \dots$$

Comparing (55.29) with (55.25) it follows that

$$\left(x \frac{d}{dx}\right)^n F(x) = F(x(1+\Delta)) 0^n$$

further generalized into the theorem

$$(55.30) \quad f\left(x \frac{d}{dx}\right) F(x) = F(x(1+\Delta)) f(0)$$

[1847, 459; on Herschel see also Graves 1849, 62 and (1) above].

Similar concerns occupied his paper "On a generalization of

the symbolic statement of Taylor's theorem" [1853]. Starting with the known theorem

$$(55.31) \quad \varphi(x+h) = e^{h \frac{d}{dx}} \varphi(x)$$

he investigated the determination of a more general symbol which will change  $x$  into any given function  $\psi(x)$  by means of the following "synthetic course":

As the effect of the symbol

$$e^{\frac{d}{dx}}$$

is to change  $x$  into  $x+1$ , it appears that

$$e^{\frac{d}{df(x)}} \quad \text{or} \quad e^{\frac{1}{f'(x)} \frac{d}{dx}} \quad (a)$$

will change  $f(x)$  into  $f(x)+1$ ; and consequently will have the effect of changing  $x$  into  $\psi(x)$  where

$$\psi(x) = f^{-1}[f(x)+1] \quad (b) \quad (6)$$

Developing (a) we get an extension of Taylor's theorem

$$(55.32) \quad \varphi[\psi(x)] = \varphi(x) + \frac{1}{f'(x)} \frac{d\varphi(x)}{dx} + \frac{1}{1.2} \frac{1}{f'(x)} \frac{d}{dx} \frac{1}{f'(x)} \frac{d\varphi(x)}{dx} + \dots$$

As in Taylor's theorem, each term is here deduced from its predecessor in a regular way [see (3) above]. Given  $\psi$ , we determine  $f$  from the functional equation (b) which, by making  $x=f^{-1}(y)$ , can be reduced to the finite difference equation

$$(55.33) \quad \psi[f^{-1}(y)] = f^{-1}(y+1).$$

Or, given (a), we first "integrate"  $f'(x)$  and then "invert"  $f(x)$  so as to determine  $\psi$ . "Upon the possibility of effecting these two operations depends the success of this attempt to interpret the symbol  $\{(a)\}$ " [1853, 286].

Pursuing further this method, Graves mentioned some "interesting results", among them

$$(55.34) \quad \text{The effect of } e^{x \log x \frac{d}{dx}} \text{ is to change } x \text{ into } x^e.$$

He stressed that the general solution of (b) "would lead to important results in the theory of functional equations". For, from (b) we have

$$(55.35) \quad \psi^n(x) = f^{-1}[f(x)+n];$$

it hence follows that the functional equation

$$(55.36) \quad A_n \psi^n(x) + \dots + A_1 \psi(x) + A_0 x = 0,$$

$A_1$  constants, "may be reduced at once to a linear equation in finite differences with constant coefficients" [1853, 286-7] (7).

In his last paper Graves [1857b] dealt with the principles which regulate the interchange of symbols in certain symbolic equations. It opened with the study of two distributive operations  $\pi, \rho$  which combine according to the law

$$(55.37) \quad \rho\pi = \pi\rho + a,$$

where 'a' a constant, "or at least a symbol of distributive operation commutative with both  $\pi$  and  $\rho$ ". If we change in (55.37)

$$(55.38) \quad \pi \longrightarrow \rho \text{ and } \rho \longrightarrow -\pi,$$

we obtain the same equation (55.37). Thus we have the first fundamental principle:

$$\text{"In any symbolical equation } \varphi(\pi, \rho) = 0 \quad (i)$$

which has been directly deduced from the fundamental equation [(55.37)], without any further assumption as

$$(55.39) \quad \text{to the nature of the operations denoted by } \pi \text{ and } \rho, \text{ we may change } \pi \text{ into } \rho, \text{ and } \rho \text{ into } -\pi; \text{ so as at once to form the correlative equation, } \varphi(\rho, -\pi) = 0 \quad (ii)$$

"The value of this principle", went on Graves, "must depend upon the extent of its application; and this will be found much wider than might at first sight be supposed". Making  $a=1$  we get from (55.37) by successively operating with  $\pi$ ,

$$(55.40) \quad \rho\pi^n = \pi^n\rho + n\pi^{n-1},$$

for  $n$  positive integer. It was further shown that (55.40) "holds good for any integer value of  $n$ " [1857b, 144-6].

If  $\psi\pi$  any function of integer powers of  $\pi$ , we infer from (55.40)

$$(55.41) \quad \rho\psi\pi = \psi\pi\rho + \psi'\pi.$$

From (55.41) "we can ascend to the more general theorem"

$$(55.42) \quad \varphi\rho\psi\pi = \psi\pi\varphi\rho + \psi'\pi\varphi'\rho + \frac{1}{1.2}\psi''\pi\varphi''\rho + \dots$$

In virtue of (55.38) we can also obtain the correlative of (55.42). These equations are but extensions of Hargreave's formulae (53.3) and (53.4). Graves mentioned that having obtained (53.3) and (53.4) "separately", Hargreave observed that the latter is deduced from the former by changing  $x$  into  $D$  and  $D$  into  $-x$  and thus arrived at the conclusion (53.8). Now, Graves's general investigation establishes "on what seems to be its real foundation the validity of the proposed method of deriving formulae one

from the other" [1857b, 146-7].

He let  $f(x)$  be any function of  $x$ . Then

$$(55.43) \quad D(xfx) = xf'x + fx,$$

or, detaching  $fx$  we have

$$(55.44) \quad Dx = xD + 1.$$

The latter is a case of the general law (55.37), whence  $x$  and  $D$  can be interchanged according to (55.38), thus Hargreave's theorem (53.8) is established. In fact from (55.44) we can deduce "the principal symbolic formulae of the Differential Calculus" where  $fD=f(D)$ ,  $fx=f(x)$  [1857b, 147].

He next deduced other changes of symbols from the equation

$$(55.45) \quad p\pi = \pi p + 1.$$

Since  $p$  is distributive,

$$(55.46) \quad pfp = fp p$$

for any polynomial function  $f(p)$ . Adding (55.45) and (55.46) we get

$$(55.47) \quad p(\pi + fp) = (\pi + fp)p + 1,$$

an equation of the form (55.37). In virtue of (55.39) we are at liberty to effect the change

$$(55.48) \quad x \longrightarrow x + fD \quad \text{or} \quad D \longrightarrow D + fx$$

$$[1847b, 147-8; fD = f(D), f'x = f(x)].$$

Further, as a specific case of (55.41) we have  $pe^{\psi\pi} = e^{\psi\pi}p + e^{\psi\pi}\psi'\pi$  or  $p + \psi'\pi = e^{-\psi\pi}pe^{\psi\pi}$ , hence

$$(55.49) \quad f(p+\psi'\pi) = e^{-\psi\pi}fpe^{\psi\pi}.$$

Also, after (55.38) we find from (55.49)

$$(55.50) \quad f(\pi+\psi'p) = e^{\psi p}fne^{-\psi p}.$$

If we make

$$(55.51) \quad x = \pi \quad \text{and} \quad D = p$$

we get from (55.50) the theorem

$$(55.52) \quad e^{\psi D}fxe^{-\psi D} = f(x + \psi'D)$$

useful "in the interpretation of symbolical expressions which are met with in the solution of differential equations" [1857b, 149-150] (e).

One can see the proximity between Donkin's and Graves's work on non-commutative operators. Once more we have an independent deduction of Boole's theorem of development (47.1) in Graves's formula (55.52), which was provided by Donkin via (55.5). Notice also that (55.49) is a generalization over Boole's theorem (47.2). Moreover, Graves's fundamental formula (55.44) was implicitly given by Bronwin in (54.44). Despite all these similari-



ties, Graves's grasp of the spirit of the calculus of operations ranging from Herschel up to Boole and his followers, is large original and formed a most useful material for Carmichael's own treatment [5.7]. Besides his proof of the validity of Hargreave's theorem (53.8) - which renders redundant all the proofs of dual symbolic theorems<sup>(9)</sup> and facilitates the integration of certain classes of differential equations- Graves dealt systematically with the interpretability of symbolic expressions. While for previous mathematicians this was a marginal problem, for Graves it was a particular concern. As evident in Carmichael [1855, chapter 8], he showed a <sup>great</sup> concern for application of exponential operators [see (6) above] to analytic geometry. Finally, as we shall see in 5.7, he provided a method for the solution of equations of the form

$$(55.53) \quad F\left(x \frac{d}{dx}\right) y = X(x).$$

5.6 Curtis, Williamson, Donkin and Graves on the earth-figure equation: 1854-1857.

In this section we comment upon papers of mid-19<sup>th</sup>-century English and Irish mathematicians on the EFE. These attempts either presented the solution of this equation under a slightly different symbolic form, or suggested another solution of Hargreave's equation (53.31), of which a case is the EFE. Williamson and Donkin belong to the former category, while Curtis and Graves to the latter. We will discuss these attempts in chronological order: Curtis [1854], Williamson [1856], Donkin [1857] and Graves [1857a]. In the process we introduce Curtis and Williamson.

A.H.Curtis graduated Trinity College Dublin in 1850 and was appointed professor of natural philosophy at Queen's College Galway from 1857 to 1880. Principally known for his treatise on the properties of the gyroscope in 1862, Curtis contributed several papers on pure and applied mathematics. Under study in this section is his paper "On the integration of linear and partial differential equations" [1854] published in the Cambridge and Dublin Mathematical Journal. The first part of [1854] concerns a modification of Carmichael's method for the solution of partial differential equations with homogeneous coefficients [see 5.9].

Curtis collaborated with Carmichael providing useful suggestions for the latter's textbook [1855] [see 5.7]. We focus here on the second part of Curtis [1854] devoted to the study of Hargreave's general equation (53.31). Throughout his paper Curtis drew on Gregory's Examples [1841] for illustrations<sup>(1)</sup>.

Curtis is the only mathematician under study not only to incorporate Boole's symbolic treatment of the EFE (46.33), but also to use Boole's solution (46.39) in order to suitably integrate a wide symbolical class equations. He claimed that most of the important second order and first degree differential equations are reducible to Hargreave's form (53.31), or,

$$(56.1) \quad \left[ \frac{d^2}{dx^2} + 2Q \frac{d}{dx} + Q^2 + Q' \pm c^2 - \frac{m(m+1)}{x^2} \right] u = 0,$$

where  $Q=Q(x)$  and  $Q'=d/dxQ$ . Instances of (56.1), he mentioned, were solved by Gregory by the tedious method of series. Moreover, Hargreave's solution of (56.1), (53.33), needs to perform  $2m$  differentiations so that it becomes interpretable. He thus set off to provide a solution involving only  $m$  differentiations, rendering thus the final result more immediately interpretable [1854, 279-80; see (2) below].

Equation (56.1) was put in the form

$$(56.2) \quad \left[ \left( \frac{d}{dx} + Q \right)^2 \pm c^2 - \frac{m(m+1)}{x^2} \right] u = 0,$$

and next via theorem (47.2) to

$$(56.3) \quad \left[ e^{-Q_1} \left( \frac{d}{dx} \right)^2 e^{Q_1} \pm c^2 - \frac{m(m+1)}{x^2} \right] u = 0,$$

where  $Q_1 = \int Q dx$ . Multiplication by  $e^{-Q_1}$  <sup>of (56.3)</sup> gives

$$(56.4) \quad \left[ \frac{d^2}{dx^2} \pm c^2 - \frac{m(m+1)}{x^2} \right] e^{Q_1} u = 0.$$

Finally, through the transform

$$(56.5) \quad z = e^{Q_1} u,$$

the initial equation (56.1) is reduced to

$$(56.6) \quad \left[ \frac{d^2}{dx^2} \pm c^2 - \frac{m(m+1)}{x^2} \right] z = 0,$$

in other words to the EFE (46.33) [1854, 280].

Curtis observed that equation (56.6) had been solved by a

"very ingenious process" by Boole in the "elegant form" (46.39)

$$(56.7) \quad z = \frac{1}{x^{m+1}} \left( x^3 \frac{d}{dx} \right)^m \frac{1}{x^{2m-1}} \left[ \frac{d^2}{dx^2} \pm c^2 \right]^{-1} 0.$$

Noticing Boole's "ingenious" mode of solution of (56.6) in a footnote [1854, 281], Curtis, taking under consideration the transform (56.5) and the solution of (56.6), deduced readily the solution of Hargreave's equation (56.1) for both cases of  $\pm c^2$  [1854, 282]<sup>(2)</sup>.

Suitably replacing  $Q$  and  $m$  in (56.1), he further provided the solution of numerous ordinary differential equations with variable coefficients, among them the Riccati equation. The paper ended with partial differential equations deduced from (56.1) by replacing  $Q$  by a function of  $x$  and  $d/dy$ . All the examples were solved in Gregory [1841] by means of various techniques including series, definite integrals, reduction to simpler cases and so on, as a unified method was missing up to 1844 [see 4.4].

We now consider another neglected figure B. Williamson. Born at Cork in 1827, Williamson matriculated in Trinity College, Dublin in 1843, an institution with which he was closely associated for over 70 years until his death in 1916. While a graduate he gained several distinctions, culminating in a "First Senior Moderatorship in Mathematics" in 1848. He contributed in the calculus of variations, but he was best known for his treatises on the differential and integral calculus published in 1871 and 1874 respectively. The treatises, outstanding for the clearness and elegance of style, enjoyed a remarkable circulation, their latest edition being in 1900 and 1906 respectively. His biographer informs us that "No man ever gained more friends". Moreover, Williamson was a devoted teacher who invited to a student "friendship and affection in an extraordinary degree"<sup>(3)</sup>.

Williamson contributed a single paper on this subject entitled -like most of Bronwin's papers- "On the solution of certain differential equations" [1856] in the Philosophical Magazine. The paper opened with the equation

$$(56.8) \quad (D^2 - \frac{2n}{x}D + a^2)y = 0, \quad D = d/dx,$$

which is the EFE in the form (42.15). Multiplying (56.8) by  $x^2$  we have

$$(56.9) \quad xD(xD-(2n+1))y + a^2x^2y = 0.$$

Assuming the transform

$$(56.10) \quad y = (xD-1)(xD-3)\dots(xD-(2n-1))z,$$

(56.9) is reduced to the form

$$(56.11) \quad (xD-3)\dots(xD-(2n-1))(D^2+a^2)z = 0.$$

Assuming from (56.11) that  $(D^2+a^2)z=0$ , or that  $z = [\cos(ax+b)]$ ,

(56.10) gives the solution of (56.8) in the form

$$(56.12) \quad y = C(xD-(2n-1))\dots(xD-1)\cos(ax+b).$$

By means of (45.19), and a suitable change of the independent variable, (56.12) can be cast into

$$(56.13) \quad y = A\left(\frac{d}{da}\right)^n a^{-1} \cos(ax+b),$$

where  $A, b$  arbitrary constants [1856, 364-5].

The procedure followed above is fairly close to Boole's for the solution of the EFE (46.33). Indeed, the factors  $(xD-1)\dots$  in (56.10) stand in place of Boole's factors  $(D-1)\dots$ ,  $D=d/d\theta$ , in (46.36). Moreover, the form (56.12) is identical to (46.37). The difference lies in that the factors in (56.12) are in the reverse order from that followed in (46.37), as well as in the final symbolic form. We omit further clarifications as similar techniques were repeatedly used before<sup>(4)</sup>.

It follows from (56.13), that the solution of the EFE in the form (46.33) [(56.6)] or

$$(56.14) \quad [D^2 + a^2 - \frac{n(n+1)}{x^2}]y = 0$$

is

$$(56.15) \quad y = Ax^{-n}\left(\frac{d}{da}\right)^n a^{-1} \cos(ax+b),$$

since (56.8) is deduced from (56.14) by means of the transform (42.14),  $y=u/x^n$ , if (56.8) is considered in  $u$  instead of  $y$  [1854, 365-6]. Williamson effected further the integration of a wider class of equations of the form (56.14), namely of

$$(56.16) \quad [D^2[f(x)u] - (\frac{n(n+1)}{x^2} - a^2)f(x)u] = 0 ;$$

the above gives by solution

$$(56.17) \quad u = \frac{A}{x^n f(x)} \left(\frac{d}{da}\right)^n a^{-1} \cos(ax+b).$$

Among his examples the Riccati equation was included [1856, 365-

Williamson's concerns for the equation (56.16) are very close to those displayed by Hargreave [1848] and Curtis [1854], two papers which are cited in his [1856]. But his method is mainly an outcome of his study of Boole [1844] and partly of the two papers mentioned above. Ignored by his contemporaries, he was acknowledged for his result (56.15) in Glaisher [1881,812]. Moreover, Glaisher incorporated Williamson's formula

$$(56.18) \quad (Da^{-1})^n = a^{-n}D^n - \frac{n(n+1)}{2}a^{-(n+1)}D^{n-1} + \\ + \frac{(n-1)n(n+1)(n+2)}{2.4}a^{-(n+2)}D^{n-2} - \dots \pm 1.3\dots(2n-1)a^{-(2n-1)}(D-a^{-1})$$

useful for the expansion of  $(d/da.a^{-1})^n$  involved in his symbolic solution (56.17), where  $D=d/da$  [1856,368; Glaisher 1881,827].

The EFE (56.14) was further studied by Donkin in the form

$$(56.19) \quad \frac{d^2u}{dx^2} + (k^2 - \frac{n(n+1)}{x^2})u = 0$$

in his paper on the LE published in the Philosophical Transactions in 1857. Donkin claimed to have introduced in that paper a simple method for the integration of the LE [see 5.8]. In a footnote he illustrated this method by applying it to (56.19), holding that it contains no novelty "except in detail" [1857,44,fn].

Letting  $D=d/dx$ , equation (56.19) was written as

$$(56.20) \quad [(D - \frac{n}{x})(D + \frac{n}{x}) + k^2]u = 0.$$

Since  $(D + \frac{n}{x})(D - \frac{n}{x}) = (D - \frac{n-1}{x})(D + \frac{n-1}{x})$ , he put

$$(56.21) \quad u = (D - \frac{n}{x})v$$

and operated on (56.20), after the substitution, with  $(D-n/x)^{-1}$ . It followed that

$$(56.22) \quad [(D - \frac{n-1}{x})(D + \frac{n-1}{x}) + k^2]v = (D - \frac{n}{x})^{-1}0.$$

Assuming that the right-hand side of (56.22) equals zero, and putting

$$(56.23) \quad v = (D - \frac{n-1}{x})\omega,$$

and so on successively, he arrived at

$$(56.24) \quad u = (D - \frac{n}{x})(D - \frac{n-1}{x}) \dots (D - \frac{1}{x})z,$$

where

$$(56.25) \quad (D^2 + k^2)z = 0.$$

Finally, by means of (56.24), (56.25) and the theorem (47.2) - according to which  $D-i/x = x^{-1}Dx^{-1}$  [for  $\varphi(x) = i \log x$ ] - we obtain the solution of (56.19) in the form

$$(56.26) \quad u = x^n (D - \frac{1}{x})^n [C_1 \sin kx + C_2 \cos kx],$$

$C_1, C_2$  arbitrary constants [1857, 44, fn].

Donkin, concise in his account, mentioned that (56.26) agrees with Boole's (46.40). Let us confirm Donkin's claim. We showed in 4.6 that Boole's form (46.40) equals (46.46) which in Donkin's notation is

$$(56.27) \quad u = C_1 x^{n+1} \frac{1}{x} \frac{\cos(kx + C_2)}{(-D)^n}.$$

To show the equivalence between (56.26) and (56.27) it suffices to prove that

$$(56.28) \quad x^n (D - \frac{1}{x})^n f(x) = x^{n+1} \frac{1}{x} \frac{f(x)}{(-D)^n}$$

where  $f(x) = \cos(kx + C_2)$ . Indeed we have

$$x^{n+1} \frac{1}{x} \frac{f(x)}{(-D)^n} = x^n (x - D) \frac{1}{x} \frac{f(x)}{(-D)^{n-1}} = x^n D \frac{1}{x} \frac{f(x)}{(-D)^{n-1}} \quad \text{and}$$

$$x^n (D - \frac{1}{x})^n f(x) = x^n D \frac{1}{x} \frac{f(x)}{(-D)^{n-1}}, \text{ or (56.28) is proved.}$$

If we change Williamson's variable  $a$  to  $x$  in (56.15) we can establish the equivalence between his and Boole's solutions [see (4) above]. Donkin's solution of the EFE was also incorporated in Glaisher [1881, 812]. Further, Donkin's form (56.26) for the case of  $-k^2$  in (56.19) was given in the form of an unsolved example in Forsyth [1914, 205].

We now conclude our survey with Graves [1857a; see 5.5, (1)]

which concerned the solution of the class

$$(56.29) \quad [(D+\phi)(D+g) - \{(n+1)c \frac{n(n+1)}{2} rk\} \psi^{-r}] y = X ;$$

$X=X(x)$  and  $\phi, g, \psi$  functions of  $x$  connected by the relation

$$(56.30) \quad g' - \phi' = c\psi^{-r},$$

$c$  constant,  $n$  integer and  $r$  any whole or fractional number, and also under the condition

$$(56.31) \quad \left(\frac{\psi'}{\psi}\right)' = k\psi^{-r},$$

$k$  constant [1857a, 34-37].

Graves's method is rather confusing and apparently of not any impact. For this reason we sketch it briefly concluding with an interesting application. He based his procedure upon the theorem

$$(56.32) \quad (D+\phi + \frac{m\psi'}{\psi}) \psi^r = \psi^r (D+\phi + \frac{(m+r)\psi'}{\psi}) ,$$

where  $\phi, \psi, r$  as above and  $m$  any whole or fractional number, which is a case of Boole's theorem (47.2). Putting

$$(56.33) \quad A_m = D + \phi + \frac{m\psi'}{\psi}$$

he showed that the solution of (56.29) is

$$(56.34) \quad y = A_0^{-1} A_r^{-1} \dots A_{nr}^{-1} (D+g)^{-1} A_{nr} \dots A_{2r} A_r X$$

[1857a, 34-5].

To exemplify this theory he assumed  $\psi(x)=x^m$ . It followed hence from (56.30) and (56.31) that

$$(56.35) \quad \left(\frac{\psi'}{\psi}\right)' = -\frac{m}{x^2}, \quad r = \frac{2}{m}, \quad k = -m \quad \text{and} \quad g-\phi = c_1 - \frac{c}{x} \quad (5).$$

In this case (56.29) becomes

$$(56.36) \quad [(D+\phi)(D+\phi+c_1 - \frac{c}{x}) - \frac{(n+1)(c+n)}{x^2}] y = X.$$

By making  $c=0$ , and writing  $-m$  in place of  $n$ , (56.36) becomes

$$(56.37) \quad [(D+\phi)(D+\phi+c_1) - \frac{m(m-1)}{x^2}] y = X$$

"which is equivalent to a general soluble form [(56.2)] which Dr.Hargreave has obtained by an entirely different method"

[1857a, 36]. Thus, while Curtis integrated (56.38) in the form (56.2) obtaining as result (56.7), under the condition (56.5), Graves provided as solution of the more general equation (56.37) the form (56.34) where

$$(56.38) \quad A_m = D+\varphi + \frac{m^2}{x}, \quad D+g = D+\varphi + c_1 - \frac{c}{x},$$

$\varphi$  an arbitrary, given function. Evidently Graves's solution is more complicated than Curtis's own as it involves a double number of differentiations. According to Graves, the interest of his procedure lay in the possibility to apply it further to the LE<sup>(6)</sup>.

## 5.7 Carmichael's development and diffusion of the calculus of operations: 1851-1855; a new epistemological attitude.

R.B.B. Carmichael was born in Dublin in 1828. Awarded a scholarship in 1847, he graduated from Trinity College in 1849, becoming a Fellow in 1852. Despite his short career, which ended with his early death in 1861, Carmichael played a prominent role in the history of the calculus of operations. Primarily motivated by Jellett's elementary textbook on the calculus of variations [1850;1.2,(7)], he orientated his study towards the solution of partial differential equations with homogeneous coefficients in 1851. A fruitful collaboration with C. Graves and Curtis helped him produce his own symbolical calculus suitably incorporated for students in his A treatise on the calculus of operations [1855], the only 19<sup>th</sup>-century British textbook devoted exclusively to this subject<sup>(1)</sup>. It was Carmichael's method that featured in the book, enriched with theorems and suggestions by Graves and Curtis. He drew additionally on Murphy, Herschel and Gregory, and included the most important results by Bronwin, Hargreave and Donkin. Carmichael contributed several papers to the Cambridge and Dublin Mathematical Journal, cited hereafter as [1851; 1853a,b;1854], and to the Philosophical Magazine, cited as [1852; 1853c;1857c]. He further contributed in the solution of the LE by means of Hamilton's calculus of imaginaries [see 5.8].

The core of Carmichael's methodology is concisely presented in his first paper "On the index symbol of homogeneous functions" [1851]; this account was slightly enriched in his second paper



which bore the same title [1852]. The basic idea developed in these papers was to extend Boole's operator calculus which was based so far on the properties of

$$(57.1) \quad x \frac{d}{dx} \text{ or } xD_x.$$

Carmichael assumed  $u_m$  to be an arbitrary homogeneous function of the  $m^{\text{th}}$  degree between  $x_1, \dots, x_n$ , claiming that  $u_m$  satisfies

$$(57.2) \quad x_1 \frac{du_m}{dx_1} + \dots + x_n \frac{du_m}{dx_n} = mu_m,$$

where  $d/dx_1$  stands for what we commonly denote by  $\frac{\partial}{\partial x_1}$ . Defining next the "index symbol"  $\nabla$  by

$$(57.3) \quad \nabla = x_1 \frac{d}{dx_1} + \dots + x_n \frac{d}{dx_n},$$

he cast (57.2) into the symbolic form

$$(57.4) \quad \nabla u_m = mu_m.$$

By iteration it follows easily from (57.4) that

$$(57.5) \quad f(\nabla)u_m = f(m)u_m,$$

for any polynomial function  $f$  [1851, 277-8; 1852, 130; see also (2) below].

Carmichael observed that the theorem (57.5) is but an extension of Boole's "fundamental" principle (45.19),

$$(57.6) \quad f\left(x \frac{d}{dx}\right)x^m = f(m)x^m.$$

Acknowledging Boole's work as based on (57.6) he proposed an extension of it based on the properties of the operator  $\nabla$  given by (57.3). Drawing throughout on analogy, he solved certain classes of partial differential equations by appealing to the solution of the corresponding ordinary ones. Before we switch to his study of differential equations, let us quote the most basic components of his theoretical approach besides his theorem (57.5).

Let  $U$  be "any mixed rational function" of  $x, y, \dots$  assumed under the form

$$(57.7) \quad U = u_0 + \dots + u_m,$$

where the index  $i$  <sup>marks</sup> a homogeneous function of the  $i^{\text{th}}$  degree. Then, in virtue of (57.5) we have

$$(57.8) \quad F(\nabla).U = F(0)u_0 + \dots + F(m)u_m,$$

for any polynomial function  $F$  of  $\nabla$ . It is here implied that  $F(\nabla)$  is a distributive operator, a fact carefully clarified in his treatise [1855] as we shall see below [in (4) and text above (14)]. A particular case of the theorem (57.8) is

$$(57.9) \quad a^{\nabla} . U = u_0 + au_1 + \dots + a^m u_m;$$

thus "the operation of  $a^{\nabla}$  upon the mixed rational function  $U$  converts the several variables  $x, y, z, \dots$  throughout it into  $ax, ay, az, \dots$ " [1851, 278; 1852, 130-1]. In his [1855, 14-5] Carmichael attributed the formula (57.9) in the case of one variable to Graves.

The next step amounted to the extension of Boole's theorem (45.20). Claiming that since  $y, z, \dots$  "are constant relative to  $x$ ", then  $d/dx, d/dy, \dots$  are commutative, he defined  $\nabla_2$  as

$$(57.10) \quad \nabla_2 = x^2 \frac{d^2}{dx^2} + y^2 \frac{d^2}{dy^2} + \dots + 2xy \frac{d^2}{dxdy} + \dots$$

and so on symmetrically for  $\nabla_3, \nabla_4, \dots$ . He then noticed that  $\nabla(\nabla-1) = \nabla_2$ ,  $\nabla(\nabla-1)(\nabla-2) = \nabla_3$ , or in general

$$(57.11) \quad \nabla(\nabla-1) \dots (\nabla-n+1) = \nabla_n$$

which is an extension of (45.20),

$$(57.12) \quad xD(xD-1) \dots (xD-n+1) = x^n D^n$$

[1851, 278-9] (2).

In a similar manner Carmichael extended Boole's "second fundamental principle" (45.19)

$$(57.13) \quad f\left(x \frac{d}{dx}\right) x^m \omega = x^m f\left(x \frac{d}{dx} + m\right) \omega,$$

where  $\omega = \omega(x)$ , to

$$(57.14) \quad F(\nabla) . \Theta_m W = \Theta_m F(\nabla + m) W,$$

where  $\Theta_m$  a known homogeneous function of degree  $m$  and  $W$  any function of  $n$  variables. Further, Hargreave's theorem (53.3) would have its analogue in

$$(57.15) \quad \Phi(\nabla) . U \omega = U \Phi(\nabla) \omega + \frac{\nabla U}{1!} \Phi'(\nabla) \omega + \dots$$

[1852, 136-7] (3).

Carmichael showed that given the solution of

$$(57.16) \quad Ax^\alpha \frac{d^\alpha y}{dx^\alpha} + Bx^\beta \frac{d^\beta y}{dx^\beta} + \dots = X,$$

"we can at once write down the solution of a partial differential equation of the class represented by"

$$(57.17) \quad A(x^\alpha \frac{d^\alpha z}{dx^\alpha} + \alpha x^{\alpha-1} y \frac{d^\alpha z}{dx^{\alpha-1} dy} + \dots) + \\ + B(x^\beta \frac{d^\beta z}{dx^\beta} + \beta x^{\beta-1} y \frac{d^\beta z}{dx^{\beta-1} dy} + \dots) + \dots = 0,$$

where  $X=X(x)$ ,  $\theta=\theta(x,y)$ ,  $A,B, \dots$  constants and  $\alpha,\beta,\dots$  positive integers. "by substituting for each term in the solution" of (57.16) in which "an arbitrary constant is introduced, such as  $C_m X^m$ , a homogeneous function of the same degree, but of arbitrary form in  $x$  and  $y$ ". He added that in all cases the solution of the corresponding class (57.16) is "furnished by the method of Professor Boole" [1851,282; see also 1851,279-280;1852,132-133].

Despite his appeal to Boole at that early stage, Carmichael persistently avoided any application of the symbolical methods which flourished from 1844 onwards. Using exclusively the theorems (45.19)-(45.20) in the new extended form, he put forward a distinct method applicable only to certain classes of equations of a symmetrical form such as (57.16)-(57.17). As his account is too concise in the papers cited above we switch to his more detailed presentation in [1855] focusing first on (57.16).

In virtue of theorem (47.12), equation (57.16) is solved symbolically as

$$(57.18) \quad y = \frac{1}{F(xD)} X + \frac{1}{F(xD)} 0,$$

where  $D=d/dx$  and  $F(xD)$  stands for

$$(57.19) \quad F(xD) = Ax^D(xD-1)\dots(xD-\alpha+1) + Bx^D(xD-1)\dots(xD-\beta+1)\dots$$

Assuming next that

$$(57.20) \quad X = L + Mx + \dots + Tx^n,$$

$L,M, \dots$  constants, he found by theorem (57.6) that

$$(57.21) \quad \frac{1}{F(xD)} X = \frac{L}{F(0)} + \dots + \frac{Tx^n}{F(n)},$$

implicitly assuming that  $F(0), \dots, F(n)$  are  $\neq 0$ .

To determine the second term of the solution (57.18) he distinguished three cases for the roots of the equation

$$(57.22) \quad F(xD) = 0.$$

Let (57.22) have real, unequal roots  $a,b,\dots,i$ . Then since

$$(57.23) \quad (xD-m)A_m x^m = 0$$

for any constant  $A_m$ , it follows that

$$(57.24) \quad \frac{N}{xD-a} 0 = \frac{1}{xD-a} 0 = C_a x^a,$$

or, finally

$$(57.25) \quad \frac{1}{F(xD)} 0 = C_a x^a + C_b x^b + \dots + C_i x^i,$$

$C_a, \dots, C_i$  arbitrary constants [1855, 21-23].

For the case of  $p$  equal roots Carmichael used the theorem

$$(57.26) \quad (xD-m)^p A_m x^m (\log x)^{p-1} = 0,$$

proved inductively. If the  $p$  equal roots have the common value  $a$ , then, in virtue of (57.26) we have that

$$(57.27) \quad \frac{1}{F(xD)} 0 = C_a x^a (\log x)^{p-1} + C'_a x^a (\log x)^{p-2} + \dots + C_b x^b + \dots + C_i x^i,$$

where  $b, \dots, i$  the unequal roots of (57.22). A similar notation was applied to the case of imaginary roots [1855, 23-4].

On similar lines ordinary equations with the last term in trigonometrical form were tackled, and several examples followed, such as equation

$$(57.28) \quad x^2 D^2 y = ax^m + bx^n,$$

which gives by solution according to (57.21) and (57.25),

$$(57.29) \quad y = \frac{ax^m}{m(m-1)} + \frac{bx^n}{n(n-1)} + C_0 + C_1 x;$$

it was implicitly assumed that  $m, n$  are distinct positive integers  $\geq 2$  [1855, 24-26].

Comparing Carmichael's treatment with Gregory's own for linear differential equations with constant coefficients [(44.16)-(44.19)] we perceive a great similarity. Indeed, Carmichael attributed the "germ of this method" to Gregory [1841], adding "That it was never matured seems to have been due to the circumstances that the distributive character of inverse functions was not then recognized, and consequently the method was only applied to the case in which the right-hand member [of (57.16)] consists of but a single term" [1855, 25] (4).

On similar lines he tackled the corresponding partial differential equation (57.17) cast into the form

$$(57.30) \quad F(\nabla)z = \theta.$$

The solution of (57.30) is symbolically expressed in the form (57.18) where  $xD$  is now replaced by  $\nabla$  after using theorem (57.11). If  $\theta$  stands for the function  $U$  given by (57.7), then via

theorem (57.8) we have that

$$(57.31) \quad \frac{1}{F(\nabla)} \theta = \frac{u_0}{F(0)} + \frac{u_1}{F(1)} + \dots + \frac{u_m}{F(m)}$$

in analogy with (57.21), the index  $i$  in  $u_i$  always implying homogeneity of order  $i$ . On similar lines, the analogues of (57.25) and (57.27) are

$$(57.32) \quad \omega_p + \omega_n + \dots$$

and

$$(57.33) \quad u_m(\log x + \log y)^{\kappa-1} + u_m(\log x + \log y)^{\kappa-2} + \dots + \omega_n + \omega_p + \dots$$

respectively, where  $n, p, \dots$  the distinct roots of  $F(\nabla) = 0$ , and  $m$  the common value of the  $\kappa$  equal roots [1855, 34-37; 1851, 280; 1852, 133].

Drawing on Gregory's Examples, Carmichael solved numerous partial differential equations from that book in a more direct and concise symbolical way. Take, for example,

$$(57.34) \quad x^2 \frac{d^2 z}{dx^2} + 2xy \frac{d^2 z}{dx dy} + y^2 \frac{d^2 z}{dy^2} = \theta_m + \theta_n.$$

Converting it into the form (57.30),

$$(57.35) \quad \nabla (\nabla - 1) z = \theta_m + \theta_n,$$

we obtain by means of (57.31) and (57.32) the solution

$$(57.36) \quad z = \frac{\theta_m}{m(m-1)} + \frac{\theta_n}{n(n-1)} + u_0 + u_1.$$

Similarly

$$(57.37) \quad x D_x \omega + y D_y \omega + z D_z \omega - a \omega = \frac{xy}{z}$$

gives by solution

$$(57.38) \quad \omega = \frac{1}{1-a} \frac{xy}{z} + u_a$$

[1851, 280-1; 1852, 134; 1855, 40; compare with Gregory 1846, 364-5].

A new method was introduced by Carmichael in his paper "Remarks on integration" [1854], reproduced in the 5<sup>th</sup> chapter of his treatise [1855]. It involved ordinary equations of the form

$$(57.39) \quad F(xD)y + Mx^m y = X,$$

and their corresponding partial ones

$$(57.40) \quad F(\nabla)z + \theta_m z = W,$$

where  $D = D_x$ ,  $M$  constant,  $X = X(x)$ ,  $W = W(x, y)$  and  $\theta_m$  a given homogeneous function in  $x$  and  $y$  of the  $m^{\text{th}}$  degree. In brief, the method proposed consisted of division of (57.39) by  $F(xD)$ , con-

sequent division by  $(1-Mx^m/F(xD))$  and finally of expansion of the latter factor in series. This method, as applied to ordinary differential equations of the form (57.39) (or (55.53)) was initially suggested by Graves in 1847 [Carmichael 1854,230]. Carmichael's originality lay in extending the solution of (57.39) to that of (57.40).

We will illustrate this new method with an example. Equation (57.41)  $x D^2 y + D y + y = 0$  is converted into the form

$$(57.42) \quad (xD)^2 y + xy = 0.$$

Dividing both sides by  $(xD)^2$  and next by  $(1+(1/(xD)^2)x)$  we arrive, after expansion of the latter factor in series, at

$$(57.43) \quad y = [1 - \frac{1}{(xD)^2}x + \frac{1}{(xD)^2}x - \frac{1}{(xD)^2}x - \dots](C_1 \log x + C_2).$$

According to theorem (57.13) we have

$$\frac{1}{(xD)^2}x = x \frac{1}{(1+xD)^2} \quad \text{and further} \quad \frac{1}{(1+xD)^2} \log x = (\log x - 2)$$

and so on. Thus, (57.43) acquires finally the form

$$(57.44) \quad y = (C_1 \log x + C_2) [1 - \frac{x}{1^2} + \frac{x^2}{1^2 \cdot 2^2} - \dots] + 2C_1 [\frac{1}{1^2}x - \frac{1}{1^2 \cdot 2^2}x^2 + \frac{1}{1^2}x^2 - \dots]$$

[1854, 230; 1855, 53-54] (5).

In analogy with (57.41) he integrated the partial differential equation

$$(57.45) \quad \nabla (\nabla - 1)z + \nabla z + \theta_m z = 0,$$

readily reduced to the form

$$(57.46) \quad \nabla^2 z + \theta_m z = 0$$

which corresponds to (57.42). The solution of (57.46) is in analogy with (57.43)

$$(57.47) \quad z = (1 - \frac{1}{\nabla^2} \theta_m + \frac{1}{\nabla^2} \theta_m - \frac{1}{\nabla^2} \theta_m - \dots) (u_0 - \frac{\log x + \log y}{z} + u_0),$$

properly restored in non-symbolical form [1854,231; see (5) above].

Carmichael claimed in his textbook that the student "will find no difficulty in applying this method" to specific cases of (57.39), such as to the EFE in the form (42.20). He provided the solution of the latter equation in series form, adding, however, the following statement:

It must be allowed that although, in the method of integration we

put forward, no mathematical artifice is employed, and although the result appears to be obtained in the most direct manner, yet the ultimate reduction of the solution to its most compact form often demands considerable analytical skill<sup>(6)</sup>.

Drawing on his [1853a,b]. Carmichael devoted chapter 7 of his treatise to the interpretation of complicated symbolic forms. A basic concern was the evaluation of the quantity

$$(57.48) \quad e^{\phi(x)D_x + \psi(y)D_y + \dots} f(x, y, z, \dots).$$

Based on Graves's work, he showed that (57.48) stands for

$$(57.49) \quad f[\Phi^{-1}(\Phi^{-1}x+1), \Psi^{-1}(\Psi y+1), \dots]$$

where  $\Phi(x) = \int dx/\phi(x)$  and so on [1853b, 166-7; 1855, 93-4]. There followed applications of (57.49) for specific values of  $\phi(x), \dots$  together with the proof of

$$(57.50) \quad e^{\Theta} \nabla F(U) = F(e^{\Theta} \nabla U),$$

where  $\Theta, U$  any functions of  $x, y, z, \dots$  and  $F$  a polynomial function, a theorem which facilitates the determination of the quantity (57.48) [1853a, 82; 1853b, 168-9; 1855, 96-98]<sup>(7)</sup>.

Again after Graves's work [see 5.5, (1)], Carmichael devoted the next chapter to applications of symbolical expressions of the form (57.48) to analytical geometry. For example, let the equation of a plane curve in rectangular coordinates be

$$(57.51) \quad F(x, y) = 0.$$

It was shown that the operation of the symbol

$$(57.52) \quad e^{\omega(xD_y - yD_x)}$$

upon  $F(x, y)$  "is equivalent to the rotation of the curve in its plane through an angle  $\omega$  round an axis passing through the origin and perpendicular to the plane" [1855, 106].

Carmichael also contributed in the solution of systems of differential equations, as well as in the evaluation of definite integrals [1855, chap. 6]. He further incorporated Bronwin's formulae (52.46)-(52.47) [1855, 88], Donkin's theorems (55.2), (55.13) [1855, 133-5], Lagrange's and Herschel's theorems (23.16), (23.18) [1855, 139-152] together with applications of the latter two theorems to finite difference equations in a form analogous to (57.18).

In his textbook Carmichael acknowledged all his predecessors and contemporaries who contributed in the development of the calculus of operations [1855, ix-x; 7], including in due place the most

important results of their work. However, the total absence of any hint to Boole's general method of analysis is noticeable. In his review of Carmichael [1855], Russell pointed out the omission of Boole's study of the binomial equation (46.1) and of the development of  $f(n+p)$  [see 4.6-4.7] among the weak points of the treatise; however, he concluded that "On the whole, we think this treatise will prove useful to the students" [Russell 1857b, 182-3].

Contrary to most of the mid-19<sup>th</sup> century British analysts mentioned so far, Carmichael favoured Gregory's methods, excluding those advocated by Boole in 1844. To a great extent the reasons why he avoided Boole's treatment of differential equations can be seen in the following statement which first appeared in his [1854, 227]:

It is proposed, in the following paper, to offer some practical remarks upon the subject of integration, more especially in reference to three capital defects under which many of the methods in common use appear to me to labour. The first defect in the methods, to which allusion is made, is their extremely artificial character, which occasions much embarrassment to the student at first, and considerable difficulty in his effort to retain them. The second great defect in these methods is, that they seem wholly unsusceptible of useful generalization. The third defect is less common, and consists in this, that some of the processes employed are circuitous, terms being introduced which subsequent operations cause to disappear. The method here put forward appears to be free from these defects, and, in so far as it is calculated to reduce and simplify the labours of the student, a practical good<sup>(8)</sup>.

In illustration there followed the solution of equation (57.41) drawn from Gregory's Examples [1841]. "In quoting from the manual just named", wrote Carmichael, "I would desire to express a sense of its general merit as well as personal obligation to its study" [1854, 228]. He concluded his account with a quotation from Gregory [1841, 314] in connection with the transcendents involved in the solution of (57.41), (57.44); "It would appear then, that before we are able to make any further progress in the solution of differential equations, we must create new transcendents in the same way as the ordinary transcendents  $e^x$ ,  $\cos x$ ,  $\log x$ , ... have been created; we must study



their properties, and endeavour to express the integrals of differential equations by means of them" [1854, 232-3; 1855,59].

In connection with Carmichael's critical approach towards the calculus of operations, we would say that in most cases the methods proposed were largely devoid of the "three capital defects" he had pointed out. However, on one hand he had been too selective in the classes of equations under study, and on the other hand his study of equations of the form (57.39) involved a new method which was hardly simple or susceptible to generalization. Take for example the EFE, which Carmichael studied solely in the ordinary form (42.20) [see 4.2.(1)]. Employing a method suggested by Curtis, he showed how the series-solution can be cast into the known finite form (42.24) [1855,57-8; see also (5),(6) above]. In this respect, Carmichael hardly effected any improvement over Ellis [1841a]. Moreover, contrary to Ellis's series method, Carmichael's own method is not susceptible to generalization; had it had been, he would have included the general form of the EFE (42.13) or (46.33) [see 4.3; 4.6].

Another minor remark applies to his advocacy of Gregory's views on the necessity of new transcendents, an outcome of his new method, which coincides with De Morgan's own views on this necessity in the development of functional equations in 1836 [passage below (37.79)]. But a more remarkable similarity between Carmichael's and De Morgan's epistemological approach is revealed in their common emphasis on the indispensability of symmetry. Carmichael wrote in [1853b,166]

When the questions [equations] to be investigated have a symmetrical character [form], not only should the results be symmetrical, but symmetrical methods should be employed for their deduction. A regard to this latter point might possibly have not only precluded some errors and many incomplete results, but also led the way to the discovery of higher and more elegant methods of analysis<sup>(9)</sup>.

This passage, stressed again in his treatise [1855,4], is almost identically given in De Morgan [1836;3.6,(10)].

The general appeal to symmetry in method and notation originated in Lagrange [1.1,1.2], revived by French semiotic philosophers [1.8] and was first practised in England by Babbage and Herschel [2.8-2.9]. De Morgan was the next to stress its necessity in the realms of his foundational study of functions

[3.6], but only in Carmichael's work do we perceive such an ob-  
 sessional concern with this property of symbols<sup>(10)</sup>. Elegance and  
 brevity in notation were indeed among the concerns of the cul-  
 tivators of operator methods from Greatheed onwards [see (42.13);  
 5.1] but of the analysts mentioned so far only Carmichael  
 selected the material under study<sup>so as</sup> to permit symmetrical treat-  
 ment. Could it had been De Morgan who influenced Carmichael in  
 that direction?

As we shall see in 5.10, the work of both De Morgan and  
 Gregory played an important role in the wider symbolical approach  
 of mid-19<sup>th</sup> century mathematicians. However, though Gregory's  
 work was worshipped by Carmichael from 1854 onwards, we have no  
 evidence whatsoever on any alleged impact of De Morgan's own, as  
 his name is omitted by Carmichael. We have full evidence though  
 of the influence of Graves's work and of Hamilton's calculus of  
 imaginaries in Carmichael's study of exponential operators and  
 the solution of the LE respectively -the latter to be studied in  
 5.8. The work of both these analysts was strongly characterized  
 by symmetry of expression and structure respectively  
 [5.5.(5);5.8]. It is quite probable that Carmichael borrowed  
 Hamilton's symbol  $\Delta$  defined by

$$(57.53) \quad i \frac{d}{dx} + j \frac{d}{dy} + k \frac{d}{dz} = \Delta$$

in 1846<sup>(11)</sup>.

Moreover we can not ignore a probable influence from the new  
 English algebraists, Cayley, Sylvester and Spottiswoode whose  
 concern for elegance and symmetry of forms is evident throughout  
 their theory of invariants and determinants which flourished soon  
 after Carmichael's graduation in 1849. Carmichael might had also  
 been aware of Spottiswoode's treatise on determinants [1851]  
 described as "the incarnation of symmetry" [Rix 1898;5.9,(1)].  
 The symbol  $\nabla$  features in that work denoting a determinant [1851,  
 7], as well as in Cayley's pertinent papers in the late 1840's.  
 Carmichael definitely knew Sylvester's paper "On certain general  
 properties of homogeneous functions" [1851] published in the  
Cambridge and Dublin Mathematical Journal nine months before  
 Carmichael's first paper [1851]. Distinguishing the object of his  
 research from Sylvester's own, Carmichael mentioned that the par-  
 tial differential operator (57.3) was applied in Sylvester's

paper [see Carmichael 1852,129.fn]. Further, Carmichael was to refer in his treatise [1855,7,123-4] to the application of the calculus of operators in the "calculus of forms" [theory of invariants] and analytic geometry by Boole, Sylvester and Salmon for additional information on the utility of differential operators<sup>(12)</sup>.

Thus Carmichael had pretty good knowledge of the work of his predecessors and contemporaries early in the 1850's. But as he stressed in [1855,ix], it was Jellett the one to motivate his "independent and original investigation" as initially introduced in his first paper. In his A treatise on the calculus of variations [1850;1.2,(7)], Jellett dealt with partial differential equations with homogeneous coefficients in the chapter "On maxima and minima of functions of two or more independent variables" [1850, 253-62]. Instances from this chapter were referred to in Carmichael [1851,280.fn;1853,143.fn;1855,38-40]. Devoid of symbolical methods, Jellett's chapter gave not the tools but the idea of an interesting area of research. It should be mentioned, though, that Jellett included some Notes [1850,355-362] where he did apply operator methods, a fact "unnoticed by most writers on the calculus" [Greer 1866,327]<sup>(13)</sup>.

Carmichael's work was soon taken up by Spottiswoode and Curtis in 1853-1854 and further studied by Greer in the 1860's [see 5.9]. Nowadays Carmichael is mostly known for his textbook [1855;see (1) above] as his actual method proposed is regarded as "not particularly significant with respect to the development of general theories" [Koppelman 1971,205-6]. Postponing an overall critical review of the general state of the calculus of operations -including Carmichael's role-to 5.10, we conclude our study with a few remarks on the merits of his textbook, which seem to have escaped the notice of recent studies.

The most interesting aspect of Carmichael's textbook was its parallel concern for foundations and applications. Critical, as we saw in (8) above, Carmichael pointed out that the neglect of the study of inverse functions had contributed "to the retardation of the Calculus of Operations as well in its theory as in its practical applications" [1855,2-5]. He was the only textbook writer to devote a chapter on Murphy's study of inverse operations, reproducing the latter's theorems (33.21), (33.22) and (33.38) [1855, chap.2]. After Boole and Graves he also dealt

with the intricate matter of the interpretation of symbolic forms [1855.2-5;chapt.7], devoting consequently chapter 8 to the applications of such forms to analytic geometry<sup>(14)</sup>. Finally chapters 9-10 and three appendices were devoted to applications of the calculus of operations to the differential and integral calculus, the calculus of finite differences, the calculus of variations, the rectification of curves and to definite integrals respectively.

With the noticeable omission of Boole's general's method, Carmichael's book was the richest source available on the current theory and applications of the calculus of operations, his solution of systems of equations and the theorem (57.50) being his most original and useful of his own results [Russell 1857b, 181-2]. It should be added that his solution of the partial differential equation (55.17) attracted the interest of Boole who incorporated in his treatise [1877,403-6] a generalization of it.

#### 5.8 The Laplace equation treated by symbolical methods: 1846-1857.

Up to 1841 the equation of Laplace's coefficients (13.6) -hereafter cited as LE- was only approximately integrated in series form [1.3;3.2;4.8]. It was first solved in finite form by Hargreave in his paper "On the calculation of attractions, and the figure of the earth" [1841] published in the Philosophical Transactions<sup>(1)</sup>. Hargreave's "original and most ingenious" analysis motivated Boole to take the next step and apply his general method effecting the integration of the LE in finite symbolic form [1846,10-11]. Boole produced two more papers on this subject [1847b,c] followed by Hargreave [1848] and Donkin [1857] who provided symbolic solutions slightly differing in form from Boole's solution. A totally different approach was taken by Carmichael in 1852 based on Hamilton's calculus of imaginaries. In the mid 1850's, Hamilton, Graves, Carmichael and Boole were to delve further into the inaccurate but interesting method employed by Carmichael.

All these attempts to solve the LE, commonly assumed in the form (13.6)

$$(58.1) \quad \frac{d}{d\mu} (1-\mu^2) \frac{du}{d\mu} + \frac{1}{1-\mu^2} \frac{d^2 u}{d\phi^2} + n(n+1)u = 0^{(2)},$$

are ignored nowadays. In contrast with the EFE, the LE involved a long and complicated treatment for its solution and the interpretation of the final result was often omitted as it presented with difficulties. Despite these inconveniences, the study of this important equation deserves attention and in what follows we provide the various forms obtained for its solution, hinting at the techniques involved. We will thus have a more complete view of the range of applications of symbolical methods which had primarily focused on another product of Laplace's work, the EFE [chapter 4;5.2-5.4,5.6].

Boole integrated equation (58.1) soon after producing his [1844] as a brief note published in [1845e] reveals<sup>(3)</sup>. His method was incorporated in full in his [1846], published like his next two papers [1847b,c] in the Cambridge and Dublin Mathematical Journal. His first paper, entitled "On the equation of Laplace's functions" [1846], opened with a commentary upon equation (58.1) and its solution as provided in Hargreave [1841]

$$(58.2) \quad u = \dots \int \cos^{-2} \frac{y-x}{2} \int \cos^{-2} \frac{y-x}{2} \left[ \cos^{2n} \frac{x-y}{2} \chi(y) dy + \psi(x) \right] dy dy \dots$$

with  $n$  integrals, where  $x$  and  $y$  new variables connected with the former  $\phi$  and  $\mu$  by the relations

$$(58.3) \quad x = \phi + \frac{1}{2} \sqrt{-1} \log \frac{1+\mu}{1-\mu}, \quad y = \phi - \frac{1}{2} \sqrt{-1} \log \frac{1+\mu}{1-\mu}$$

and where  $\chi(y), \psi(x)$  "denote arbitrary functions of  $y$  and  $x$ " [1846,10-11].

Boole observed that (58.1) is an equation "not more remarkable for the importance of its physical applications, than for the difficulties which it presents in a purely mathematical point of view". Claiming to have verified the correctness of Hargreave's solution (58.2) by a "different analysis", he stated that certain of Hargreave's deductions are erroneous, his result is "so complicated by signs of integration" and that the determination of the arbitrary functions "extremely difficult". Based on his [1844] Boole would show "that the complete integral may be expressed in a form at once symmetrical and free from signs of integration" and that by a proper determination of the

arbitrary functions one can deduce from it "the actual forms of Laplace's coefficients" [1846,10-11].

His first step was to reproduce the theorems applied to the binomial equation (46.1) in his [1844;see 4.5, stage 4; 4.6 up to (46.14);1846,11-14]. The next step was to reduce the partial differential equation (58.1) to an ordinary one by putting

$$(58.4) \quad \frac{d}{d\varphi} \sqrt{-1} = a^{(4)}.$$

Finally, assuming the transform

$$(58.5) \quad u = (1-\mu^2)^r v$$

and setting

$$(58.6) \quad r = -a/2, \quad \mu = e^{\theta}, \quad D = d/d\theta,$$

he cast by his known method [see 4.5, stage 2] the initial equation (58.1) in  $u(\varphi, \mu)$  to the symbolical form

$$(58.7) \quad v \frac{(D-a+n-1)(D-a-n-2)}{D(D-1)} e^{2\theta} v = 0$$

in  $v(\theta, a)$ , "a" regarded as a constant. The integral of (58.7) will readily yield that of (58.1) if we take under consideration (58.4)-(58.6) [1846,14-15].

Now, equation (58.7) is of the binomial form (46.1) susceptible to solution by theorem 2: (46.8)-(46.10) of Boole's general method. Skipping the procedure for the solution of (58.7) -illustrated via the EFE (46.33)- we present the final result given by Boole in the form

$$(58.8) \quad u = F(\mu, \frac{(1-\mu^2)^{1/2}}{\mu} e^{\varphi\sqrt{-1}})$$

where

$$(58.9) \quad F(\mu, e^{\varphi\sqrt{-1}}) = \mu^n \left\{ \frac{d}{d\mu} \frac{1}{\mu} \right\}^n \left\{ (\mu+\mu^2)^n \psi \left( \frac{\mu e^{\varphi\sqrt{-1}}}{1+\mu} \right) + (\mu-\mu^2)^n \chi \left( \frac{\mu e^{\varphi\sqrt{-1}}}{1-\mu} \right) \right\},$$

$\psi, \chi$  standing for the arbitrary functions [1846, 16-18] (5).

Boole went on to discuss the integral (58.9); substituting a series form for  $u$ , he found that "the most general form, of the kind, which the integral can assume", is

$$(58.10) \quad u = \Sigma [c f(\mu) + c' f(-\mu)] \cos r \varphi,$$

$r$  any number "integral or not",  $c, c'$  constants "differing for different values of  $r$ " and where the coefficient of  $\cos r \varphi$  "will be always a finite algebraic function of  $\mu$ ".

$$(58.11) \quad f(\mu) = (1-\mu^2)^{r/2} \mu^{n-r} \left[ \frac{d}{d\mu} \frac{1}{\mu} \right] n \mu^{n+r} (1+\mu)^{n-r}$$

[1846,18-20] (6).

The paper concluded with a survey on the form of Laplace's coefficients [see (13.7); 1.3,(5)] and the following statement [1846,20-22]

I have entered with more particularity into the details of the above solution, than to some might have appeared necessary; but it was my object in this paper, not only to integrate the Equation of Laplace, but also to illustrate, and in so doing, if it might be, to recommend a method in Analysis (7).

The part of this paper, as sketched from (58.4) up to (58.9) was briefly reproduced in his textbook [1877,433-5].

In his paper "On the attraction of a solid of revolution on an external point" [1847b], he considered the Laplace form (13.2)

$$(58.12) \quad \frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2} = 0.$$

Denoting by  $u$  the potential of the solid,  $z$  its axis of revolution and  $r=(x^2+y^2)^{1/2}$  the distance of the attracted point from that axis, he transformed (58.10) to an equation in two variables

$$(58.13) \quad \frac{d^2u}{dr^2} + \frac{1}{r} \frac{du}{dr} + \frac{d^2u}{dz^2} = 0.$$

Further, letting

$$(58.14) \quad r = e^\theta, \quad D = \frac{d}{d\theta},$$

he cast equation (58.13) into the symbolic form

$$(58.15) \quad D^2u + \frac{d^2}{dz^2} e^{2\theta} u = 0$$

solved in series form by his general method [see 4.5, stage 3; 1847b,1-3].

In the opening of the paper Boole stated his intention to deal with the LE (58.12) in the realms of the theory of elastic fluids, pointing out that, with perhaps the exception of Poisson ("Allusion is sometimes made to a complete solution of the equation obtained by Poisson, but I have not been so fortunate

as to meet with it"), noone had applied so far a "more general form of the integral except in Laplace's series". Acknowledging the form earlier obtained by himself as "too complex for physical applications", he deduced a particular solution of (58.15) -hence of (58.12)- in definite integral form

$$(58.16) \quad u = \int_0^\pi \varphi(z+r\cos\theta\sqrt{-1})d\theta,$$

by means of which interesting physical applications were hinted at [1847b,1,4-7] (8).

But while that paper was physically orientated, the next one published consecutively to it, was purely abstract as it dealt with "a certain symbolical equation" [1847c] a particular case of which is the LE (58.1). In brief, the general method in analysis [1844] gave rise to a first study of the LE in [1846], and consequently invention worked the other way round (9); that is [1846] yielded a far more general and abstract study of symbolical equations of the form

$$(58.17) \quad (\pi_m + \alpha)(\pi_n + \beta)u + qu = 0.$$

In the case of  $\alpha = \beta = 0$ , equation (58.17) was partially studied above as (54.29),  $\pi_m$ ,  $\pi_n$ ,  $p$  being operators which obey the laws (52.54) and (54.28) and  $q$  a constant. In this case we have for solution the "conjugate" forms

$$(58.18) \quad u = \pi_{m+1}\pi_{m+2}\dots\pi_{m+r}\pi_n\pi_{m+1}^{-1}0 = \pi_m^{-1}\dots\pi_{m-r+1}^{-1}\pi_n\pi_{m-r}^{-1}0,$$

the values of  $r$  being respectively determined by the equations

$$(58.19) \quad q + (\pm m \mp n)r + a \frac{r(r+1)}{2} = 0,$$

with  $a$  the constant involved in (52.54) or (54.28) (10).

In analogy with (58.18) Boole claimed that the solution of (58.17) will be exhibited in either the form

$$(58.20) \quad u = (\pi_{m+1} + \alpha)\dots(\pi_{m+r} + \alpha)(\pi_n + \beta)^{-1}(\pi_{m+r} + \alpha)^{-1}0$$

or its conjugate one, the value of  $r$  determined by (58.19). Letting next the operators

$$(58.21) \quad \pi_m = \varphi(\mu) \frac{d}{d\mu} + n\varphi'(\mu), \quad p = \varphi(\mu),$$

which proved to satisfy the laws (52.54), Boole showed that equation (58.17) is reduced to (58.1) under the assumptions

$$(58.22) \quad m = n = 0, \quad a = -2, \quad \alpha, \beta = \pm \frac{d}{d\varphi} \sqrt{-1}, \quad q = (n+1)n$$



[1847c,10-12].

Omitting a detailed study of his results, Boole confined to remarking that (58.1) is susceptible to the solution (58.20) and its conjugate, neither of which "can be freed from integral signs" and that the second solution is equivalent to Hargreave's result (58.2). He concluded by saying [1847c,12]

The investigation we have entered upon is chiefly valuable as presenting to us what will be thought a very curious chapter in symbolical algebra, and introducing us to the family [(58.17)] of which Laplace's equation is a member. But it must be confessed that they are an interesting rather than an amiable group<sup>(11)</sup>.

Thus, Boole studied the LE in [1846; 1847c] from a strictly mathematical point of view, grasping the opportunity not only to show the efficacy of his general method but also to introduce a new approach. This approach had a direct effect on Bronwin [1850a], as we saw in 5.4, and a less direct one on Donkin [1857]. But before we proceed to Donkin's paper, let us see Hargreave's symbolical treatment of the LE as in his [1848; 5.3].

Hargreave studied the equation

$$(58.23) \quad D^2 + bDu + c^2u - n(n+1)\frac{u}{\cos^2x} = X,$$

where  $X=X(x)$ ,  $u=u(x)$ ,  $D=d/dx$  and  $b, c$  constants. He let  $\alpha, \beta$  be the roots of the equation

$$(58.24) \quad z^2 + bz + c^2 = 0,$$

and assumed next the transforms

$$Du - \alpha u = u_1$$

$$(58.25) \quad \dots\dots\dots$$

$$Du_r - \alpha u_r - 2r \tan x u_r = u_{r+1}$$

for  $r=0,1,\dots,n$ . Successively reducing thus (58.23), he eventually integrated it, obtaining for the case of

$$(58.26) \quad \alpha = c, \beta = -c, X = 0,$$

which renders (58.23) in the form

$$(58.27) \quad \frac{d^2u}{dx^2} - c^2u - n(n+1)\frac{u}{\cos^2x} = 0,$$

the solution of (58.27) as

$$(58.28) \quad u = k e^{\alpha x} \left[ \frac{d}{d(\tan x)} \right]^{n+1} \left\{ e^{-2cx} (\cos x)^{-2n+1} \int e^{2cx} (\cos x)^{2n} dx \right\}$$

[1848,52-53).

He then assumed the LE in the form (58.1) where  $\varphi$  is now replaced by  $y$ . Putting

$$(58.29) \quad x = \tan^{-1}(\mu\sqrt{-1}),$$

equation (58.1) was cast into the form

$$(58.30) \quad \frac{d^2u}{dx^2} - \frac{d^2u}{dy^2} - n(n+1)\frac{u}{\cos^2x} = 0,$$

readily reduced to (58.27) if we replace  $d/dy$  by  $c$ . Accordingly, (58.28) will give the solution of (58.30), and eventually, by considering (58.29), the solution of the LE (58.1) in the form

$$(58.31) \quad u = e^{\tan^{-1}(\omega)\frac{d}{dy}} \left[ \frac{d}{d\mu} \right]^n [(1-\mu^2)^n \varphi(y-2\tan^{-1}(\omega))] + \\ + e^{-\tan^{-1}(\omega)\frac{d}{dy}} \left[ \frac{d}{d\mu} \right]^n [(1-\mu^2)^n \chi(y+2\tan^{-1}(\omega))],$$

where  $\omega=\mu\sqrt{-1}$ ,  $\varphi, \chi$  arbitrary functions [1848,53-4].

The sole remark Hargreave made upon his procedure was that "The only important result which I obtained [by means of successive operations] is the following [(58.23)-(58.31)], being a slight generalization of the method originally employed by me in effecting the solution of the equation of LAPLACE'S coefficients" [1848,52]. Apparently, Hargreave referred by this cryptic remark to his [1841] and to the fact that (58.31) is equivalent to his former solution (58.2). But no further discussion ever appeared about the form (58.31) and its potential equivalence with Boole's form (58.9). The only comment to be made upon Hargreave's procedure was by C.Graves [1857a; 5.6]. Assuming in (56.30)  $\psi=(\cos x)^{2m}$ , the general equation under study in his paper (56.29) could be reduced to Hargreave's equation (58.27); in other words Graves's study was more general but less practical than Hargreave's own [see (56.29)-(56.38)].

A very different approach was taken by Donkin [1857; see 5.6]. Assuming initially the LE in the form (58.12) in rectangular coordinates he cast it into the form

$$(58.32) \quad \left[ \left( \sin\theta \frac{d}{d\theta} \right)^2 + \left( \frac{d}{d\varphi} \right)^2 + n(n+1)(\sin\theta)^2 \right] u_n = 0,$$

first by using the polar coordinates

$$(58.33) \quad x = r\sin\theta\cos\varphi, \quad y = r\sin\theta\sin\varphi, \quad z = r\cos\theta,$$

and next by assuming the function  $u$  in the form

$$(58.34) \quad u = u_0 + u_1 r + u_2 r^2 + \dots$$

[1857,43]. Apart from omitting

Hargreave [1848], Donkin revealed a full knowledge of the work of his predecessors on the LE. He observed that Boole's form (58.9) was definitely an improvement over (58.2), adding that Boole's second form of solution (58.20) "is still for most purposes probably less useful" than the earlier form (58.9). Under the influence of Boole [1846, 1847c] and his own [1850; see 5.5] Donkin went on to provide a solution of (58.1) - or (58.32) - which bore "a general resemblance" to Boole's (58.9) "by a very simple method" [1857,43-44] sketched below.

First he let  $d/d\varphi = k$  in (58.32) and solved the reduced equation for the case of  $n=0$

$$(58.35) \quad \frac{d^2 u_0}{dt^2} + k^2 u_0 = 0, \quad t = \log \tan \frac{\vartheta}{2}.$$

Replacing  $k$  by  $d/d\varphi$  again and substituting arbitrary functions of  $\varphi$  for the arbitrary constants involved in the solution of (58.35), he obtained the complete integral of (58.32) for  $n=0$  in the form

$$(58.36) \quad u_0 = f(e^{\varphi} \tan \frac{\vartheta}{2}) + F(e^{-\varphi} \tan \frac{\vartheta}{2})$$

[1857,44-5].

Returning to the general case (58.32), he put

$$(58.37) \quad \left[ \sin \vartheta \frac{d}{d\vartheta} \right]^2 + n(n+1)(\sin \vartheta)^2 = \rho_n$$

and

$$(58.38) \quad \sin \vartheta \frac{d}{d\vartheta} + n \cos \vartheta = \pi_n.$$

Evidently the operators  $\rho_n$ ,  $\pi_n$  thus defined follow the laws

$$(58.39) \quad \rho_{-n} = \rho_{n-1} \quad \text{and} \quad \pi_n \pi_{-n} + n^2 = \rho_n.$$

By combining these results it follows in the reduced symbolic form of (58.33)

$$(58.40) \quad (\pi_n \pi_{-n} + n^2 + k^2) u_n = 0, \quad k = d/d\varphi,$$

that the operators  $\pi_n, \pi_{-n}$  combine according to the law

$$(58.41) \quad \pi_{-n} \pi_n + n^2 = \pi_{n-1} \pi_{-(n-1)} + (n-1)^2$$

[1857,45].

Setting next

$$(58.42) \quad u_n = n_n v,$$

Donkin arrived after substitution in (58.40) at

$$(58.43) \quad u_n = \pi_n u_{n-1}$$

in virtue of the property (58.41). By iteration it follows from (58.43) that

$$(58.44) \quad u_n = \pi_n \pi_{n-1} \dots \pi_2 \pi_1 u_0,$$

$\pi_1$  defined by (58.38) and  $u_0$  given in (58.36). Substituting these values in (58.44) we arrive at the complete integral of (58.32) in the form

$$(58.45) \quad u_n = (\sin \theta)^{-n} \left[ \sin \theta \frac{d}{d\theta} \sin \theta \right]^n \left[ f(e^{\varphi - 1} \tan \frac{\theta}{2}) + F(e^{-\varphi - 1} \tan \frac{\theta}{2}) \right],$$

where  $f, F$  arbitrary functions [1857,45-46].

Like Boole [1846], Donkin devoted the rest of his lengthy paper to a further investigation of the results obtained by his method. For example he applied his method to an inquiry on Laplace's function  $T$  [see(13.3),(13.7)] claiming in a "Postscript" that his investigations on  $T$  "may be compared with some of those" in Murphy [1833c; see 3.2 below (32.25); Donkin 1857,47-53,57]. Donkin felt incapable to prove the equivalence between his form (58.45) and Boole's (58.9) [1857,46-7]. In response, Boole showed that Donkin's "singularly elegant form" (58.45) could be obtained by his method of [1846], implicitly thus proving the desired equivalence [Boole 1877,435-6].

All the methods applied so far to the LE, directly or indirectly stemmed out of Boole's general method [1844]. Moreover, instances of these methods were applied to the EFE<sup>(12)</sup>; only now the symbolical procedures were more complicated and sophisticated. Both these equations were gradually isolated from their physical environment and studied strictly mathematically. The first to call attention to this necessity was Murphy [1833c;3.2]. Nowadays Murphy's prophecy is verified as the study of the Laplace's coefficients belongs strictly in the realms of analysis. But what about the first bold attempts to render the integral of the LE in finite form? Did the results obtained prove useful in physical or mathematical context?

These questions will seek an answer in our concluding chapter 9. In the meanwhile the only acknowledgment to the three analysts mentioned above to be found in connection with the LE are Pratt [1871,20,fn] and Todhunter [1875,154; see 3.2]. The

former gave a brief reference to Hargreave [1841] and Donkin [1857], while the latter a more general reference to "chapter 17" of Boole's textbook [1859;1877;see 4.5,(5)]. But even less fortunate was Carmichael's novel approach based on Hamilton's calculus of imaginaries for reasons which will be evident in the brief overview of his work with which we conclude our account.

Carmichael first studied the LE in the symmetrical form (58.12) in 1852. The material of his study was published in the Philosophical Magazine under the title "Laplace's equation, its analogues, and the calculus of imaginaries" [1853c]<sup>(13)</sup>. His object was to "exhibit the applicability of imaginary symbols of operation to integration" and to show that "both the methods of deduction and the solutions thus derived are symmetrical and not devoid of elegance" while the process of verification in all cases is "simple and obvious" [1853c,273].

He studied first the equation

$$(58.46) \quad \frac{d^2U}{dx^2} + \frac{d^2U}{dy^2} = 0,$$

providing three distinct forms of solution; one in series, one in integral and one in functional form. To achieve the latter he assumed (58.46) to be written in the symbolical form

$$(58.47) \quad \left(\frac{d}{dy} - i\frac{d}{dx}\right)\left(\frac{d}{dy} + i\frac{d}{dx}\right)U = 0, \quad i^2 = -1.$$

Omitting any derivations, he simply stated that after dividing both sides of (58.47) by the two factors  $(d/dy \pm id/dx)$  the result obtained would evidently be

$$(58.48) \quad U = \varphi(x+iy) + \psi(x-iy),$$

where  $\varphi, \psi$  are arbitrary functions [1853c,274-5].

On similar lines he applied the "calculus of Triplets", that is of operations  $i, j$  connected by

$$(58.49) \quad i^2 = j^2 = -1, \quad ij = -ji,$$

to the LE in the form (58.12). In analogy with (58.47), he cast (58.12) in virtue of  $i, j$  to the symbolical form

$$(58.50) \quad \left(\frac{d}{dz} - i\frac{d}{dx} - j\frac{d}{dy}\right)\left(\frac{d}{dz} + i\frac{d}{dx} + j\frac{d}{dy}\right)V = 0$$

assuming for its solution the functional form

$$(58.51) \quad V = \varphi(x+iz, y+jz) + \psi(x-iz, y-jz)$$

in analogy with (58.48). However he admitted his "inability" to

offer an interpretation of the form (58.51) upon which the actual practical value of the "analytical complete" results of his study would depend [1853c,275-277] <sup>(14)</sup>.

As Carmichael stated few years later, "Unable to interpret this form [(58.51)], and impressed with the conviction that, to render the solution, if true, of any value, such interpretation was absolutely necessary, I took the liberty of soliciting the attention of mathematicians to this point" <sup>(15)</sup>. Indeed, in the mid 1850's, various analysts responded to Carmichael's appeal, particularly Hamilton and C.Graves. All the quotations to follow are taken from papers and notes published in the Proceedings of the Royal Irish Academy for the years 1853-1857 <sup>(16)</sup>. But before we proceed to the most important amendments effected upon Carmichael's original procedure, let us first clarify its principal weak point.

In order to deduce (58.48) from (58.46) we need as intermediate step

$$(58.52) \quad \left( \frac{d}{dy} + i \frac{d}{dx} \right)^{-1} 0 = e^{-i \frac{d}{dx} y} \left( \frac{d}{dy} \right)^{-1} 0 = e^{-i \frac{d}{dx} y} \psi(x) = \psi(x-iy),$$

omitted by Carmichael, apparently on the grounds that it is evident. In fact such a symbolical procedure was used by Gregory in his Examples [1846,351]. In analogy with (58.52), in the case of three variables we would have

$$(58.53) \quad \left[ \frac{d}{dz} + i \frac{d}{dx} + j \frac{d}{dy} \right]^{-1} 0 = e^{-\left( i \frac{d}{dx} + j \frac{d}{dy} \right) z} \psi(x,y).$$

For Carmichael's result (58.51) to be correct it would suffice that

$$(58.54) \quad e^{i \frac{d}{dx} j \frac{d}{dy}} = e^{i \frac{d}{dx}} \cdot e^{j \frac{d}{dy}}$$

holds true. But according to Murphy's theorem (33.22), equation (58.54) would be true only if the exponents of e in its right-hand side are commutative. So, in virtue of the definition of i,j (58.49), (58.54) is false, therefore the solution (58.51) erroneous.

Hamilton was the first to hint at the non-commutativity of i,j and thus object to Carmichael's solution of the LE. However, he acknowledged Carmichael for being the first to apply triplets to the LE, <sup>the</sup> naming after him <sub>an</sub> equation

$$(58.55) \quad \left( \frac{d}{dz} - i \frac{d}{dx} - j \frac{d}{dy} \right) V = 0$$

[1857,62-3] (17).

Hamilton's brief note stimulated Graves to study the effect of the symbol

$$(58.56) \quad e^{(i \frac{d}{dy} + j \frac{d}{dz})x}$$

upon functions of the finite form

$$(58.57) \quad f(y,z) = \sum A y^m z^n.$$

[1857c,163-166]. Omitting the complicated combinatorial details involved, we give the result written by Graves as

$$(58.58) \quad e^{(i \frac{d}{dy} + j \frac{d}{dz})x} f(y,z) = M f(y+ix, z+jx);$$

the symbol  $M$  stands for the mean value of the  $(m+n)!/m!n!$  products of  $m+n$  factors, of which  $m$  are equal to  $y+ix$ , and  $n$  to  $z+jx$  [1857c,166-7].

Before concluding his account, Graves stressed that:

The boundaries of algebra having been of late extended so as to include symbols which are not commutative with each other, it becomes absolutely necessary to have the means of denoting certain standard and constantly occurring combinations in brief and unambiguous ways. The symbol  $M$ , proposed in this paper, may perhaps be a useful contribution to mathematical language. It has the recommendation of having been already used in a similar, though less extensive, meaning by M. Cauchy<sup>(18)</sup>.

Carmichael acknowledged Hamilton's "valuable assistance" and admitted his mistake in assuming (58.54) to hold true [1857a,317; see (15) above]. However, he expressed several objections towards Graves's treatment of the LE; among them was his typical objection towards the form (58.57) —used initially in his [1853c] and in Graves [1857c]— and the solution which involved Graves's formula (58.58) on the grounds of non-symmetry. However, he took under consideration both Graves's study of the operator (58.56) and Hamilton's initial symmetrical form of the LE [see (17) above]

$$(58.59) \quad (iD_x + jD_y + kD_z)^2 V = 0,$$

where  $i, j, k$  Hamilton's quaternions defined by

$$(58.60) \quad i^2 = j^2 = k^2 = -1, \quad jk = -kj = i, \quad ki = -ik = j, \quad ij = -ji = k.$$

The solution to be produced now, satisfying Carmichael's stand-

ards by being in a "purely symmetrical" form, was

$$(58.61) \quad V = \left( \frac{x}{i} + \frac{y}{j} + \frac{z}{k} \right) \varphi \left( \frac{y}{j} - \frac{z}{k}, \frac{z}{k} - \frac{x}{i}, \frac{x}{i} - \frac{y}{j} \right) + \psi \left( \frac{y}{j} - \frac{z}{k}, \frac{z}{k} - \frac{x}{i}, \frac{x}{i} - \frac{y}{j} \right)$$

[1857a,218].

Graves was to observe that Carmichael's solution (58.61) was once more "not a true solution of Laplace's equation" by saying that "This becomes at once apparent on trying the case in which the function [ $\psi$  in (58.61)] reduces to  $(y/j - z/k)^2$ , or  $-(y^2 + z^2)$  [1857d,220]. He further contributed a paper on the LE [1857e,221-223] to which Boole replied in a letter published in the same journal [see (15) above] as [1857].

Motivated by Graves [1857c,e] Boole wrote

It appears to me, therefore, that the operating symbols [(58.56)] being functions of symbolical quaternions, the most natural method of seeking their interpretation is to refer them to the most general problem of the development of a function of a quaternion<sup>(19)</sup>.

He next displayed his "fundamental theorem" which gives the expansion of the function

$$(58.62) \quad f(\omega + ix + jy + kz)$$

in the form of a simple quaternion—given the laws (58.60) and that  $f(\omega)$  is capable of being expanded in ascending powers of  $\omega$  by Maclaurin's theorem [1857,376-379].

Boole presented next particular deductions of his theorem; among them the expansion of the operator (58.56) in a finite trigonometrical form, devoid of the problem of interpretation and suitably applied to the solution of the LE [1857,380-384]. Boole's letter to Graves ended as follows:

I offer no apology for making these observations. I am sure that your object, like mine, is the discovery of truth alone. The application of quaternions to the solution of partial differential equations is a subject deserving of being thoroughly investigated; partly because of the analytical interest attaching to the inquiry, and partly because the possibility of resolving the function

$$\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2} \quad \text{in two linear factors, seems at first sight to}$$

promise material aid in the solution of a problem of peculiar



physical importance. The latter consideration appears to have been present to your own mind. I have now stated to you the reasons which have led me to entertain a different opinion<sup>(20)</sup>.

The series of these mild disputes on the amendent of Carmichael's solution of the LE ended with a brief response of Graves [1857f,385-386;see (15) above] to Boole [1857]. And here ends our overview of mid 19<sup>th</sup> century British attempts to integrate the LE in finite symbolical form. All these attempts are for the first time in our century saved from oblivion; attempts which, no matter how far they proved useful in physics, definitely contributed in the development of symbolical algebra after Boole's general method and Hamilton's quaternions [see (9),(12),(18)-(20) above].

#### 5.9 The impact of Carmichael's and Boole's operator calculi: 1853-1863; the work of Spottiswoode and Russell.

This section covers the most important applications of Carmichael's and Boole's symbolical methods in the realms of differential equations in mid-19<sup>th</sup>-century Britain. The main figures under study are W.Spottiswoode and W.H.L.Russell; the former offering an interesting sample of the obsession with symmetry in symbolical notation which characterized the new English algebra of that time [see 5.7,5.10], the latter being the main defender of Boolean operator calculus in the 1860's. Besides we refer to Curtis [1854; see 5.6], concluding with two unknown figures, S.Roberts and R.Greer. We follow a chronological order, avoiding to enter into technical details which in the case of our principal figures would demand complex symbolical expressions which afforded little scope for useful application.

Born in London in 1825, Spottiswoode graduated from Oxford in 1842 with a first class degree in mathematics. Between 1847 and 1871 he contributed an abundance of original work in a variety of mathematical topics, such as the calculus of variations, quaternions, and the theory of curves, switching next his interests towards experimental physics. He is remembered as a devoted teacher and his lectures were characterized by "a remarkable clearness of exposition" and a "depth of poetic feeling".

Like Sylvester, Spottiswoode was an accomplished linguist. The mastery he had gained over the mathematical symbols "was so complete that he never shrank from the use of expressions, however complicated, nay, the more complicated they were the more he seemed to revel in them, provided they did not sin against the ruling spirit of all his work—symmetry". Thus it is not a surprise that he delved into a study of determinants, for "To a mind imbued with the love of mathematical symmetry the study of determinants had naturally every attraction". Moreover, "the rapid development of the subject" was largely due to his elementary treatise on determinants [1851] (1).

Spottiswoode died in 1883 and his remains rest in Westminster Abbey. In his memorial speech, Spottiswoode was praised for the exceptional sweetness of his character, <sup>for</sup> his wide fields of knowledge and his successful career, <sup>and</sup> ranked nearly as high as a Newton or Darwin [see (1) above]. The most interesting aspect of his work, shared in common with Sylvester, was that he viewed mathematics in terms of "morphology". This term, according to Sylvester, applied to the most diverse branches of culture, such as grammar, botany, comparative anatomy, music and algebra, most particularly to the calculus of forms or invariants [Sylvester 1869a, 2, fn]. Both mathematicians studied under the influential biologist T.H. Huxley who was an ardent admirer of Darwin. Introduced by J.W. von Goethe in the 1810's, the term "Morphology", defined as "The law of form or structure independent of function", was a central feature of Darwin's book The origin of species [1859]. Sylvester mentioned Goethe, Darwin and Huxley in his [1869a], and apparently he picked out this term from natural history whose "very soul" in the 1860's was morphology (2).

Spottiswoode's "great skill in, what might be called the morphology of mathematics" [Rix 1898, 826] (3) is evident in his paper "On certain theorems in the calculus of operations" [1853] published in the Cambridge and Dublin Mathematical Journal. Acknowledging Boole's D-operator calculus [1844] and Carmichael's  $\nabla$ -calculus [1851], Spottiswoode proposed "still further extensions" of the latter calculus applicable to wider classes of equations than those treated by Carmichael [Spottiswoode 1853, 25].

New symbols  $\nabla_i, \nabla_{j,i}, \nabla_{j+i}$  were defined [1853,25-26] as follows. If the indices  $i_1, i_2, \dots, j_1, j_2, \dots$  and so on denote any permutations of  $1, 2, 3, \dots$ , then  $\nabla_i$  stands for

$$(59.1) \quad \nabla_i = x_{i_1} \frac{d}{dx_1} + x_{i_2} \frac{d}{dx_2} + \dots,$$

and symmetrically for  $\nabla_j$  and so on. Now, if the series  $j_1, j_2, \dots$  is a permutation of  $i_1, i_2, \dots$ , it is accordingly denoted by  $j_{i_1}, j_{i_2}, \dots$ ; hence the new definition

$$(59.2) \quad \nabla_{j,i} = x_{j_{i_1}} \frac{d}{dx_1} + x_{j_{i_2}} \frac{d}{dx_2} + \dots$$

Finally he defined

$$(59.3) \quad \nabla_{j+i} = x_{j_1} x_{i_1} \frac{d^2}{dx_1^2} + x_{j_2} x_{i_2} \frac{d^2}{dx_2^2} + \dots + (x_{j_1} x_{i_2} + x_{j_2} x_{i_1}) \frac{d^2}{dx_1 dx_2} + \dots$$

In a similar manner he defined (59.2)-(59.3) for more than two indices and pointed out the principal relations holding between these new symbols (without proof). Among them <sup>was</sup> the formula

$$(59.4) \quad \nabla_{j+i} = \nabla_j \nabla_i - \nabla_{j,i},$$

which easily follows from (59.1) and (59.2) if we take under consideration the identity

$$(59.5) \quad (x_{j_1} \frac{d}{dx_1}) (x_{i_1} \frac{d}{dx_1}) = x_{j_1} x_{i_1} \frac{d^2}{dx_1^2} + x_{j_1} \frac{d}{dx_1},$$

and compare the result with (59.3). Formula (59.4) is an extension of Carmichael's theorem (57.11) for the case  $n=2$ .

$$(59.6) \quad \nabla_2 = \nabla(\nabla - 1)$$

where  $\nabla_2$  is defined by (57.10) in analogy with (59.3).

Based on the theory of permutations, Spottiswoode proposed a symmetrical notation for  $\nabla_{j+i}$  in the form of a determinant. However, he finally adopted "Sylvester's notation"

$$(59.7) \quad \nabla_{j+i} = \left\{ \begin{matrix} i, j \\ i, j \end{matrix} \right\}, \quad \nabla_{k+j+i} = \left\{ \begin{matrix} i, j, k \\ i, j, k \end{matrix} \right\}, \dots$$

[1853,26-27]. All these new devices served him to show that a complex class of a linear partial differential equation of the  $n^{\text{th}}$  order in  $u(x_1, x_2, \dots)$  can be cast into the symbolical form

$$(59.8) \quad A \nabla_{j_1+1+\dots+1} + B \nabla_{j_2+1+\dots+1} + \dots = \Theta$$

where the number of the quantities  $i, j, \dots, l$  is  $n$ ;  $A, B, \dots$  are constants and  $\Theta$  any function of  $x_1, x_2, \dots$ . The solution of (59.8)

would be given accordingly as

$$(59.9) \quad u = F(\nabla)\theta + F(\nabla)0$$

where  $F(\nabla)$  stands for

$$(59.10) \quad [A\nabla_{1+j+\dots+1} + B\nabla_{j+1+\dots+1} + \dots]^{-1}$$

[1853,25-28] (4).

Spottiswoode omitted any illustration whatsoever of the process (59.8)-(59.9) via an example. What he aimed at was to show how Carmichael's  $\nabla$ -calculus could be applied to cases of partial differential equations "in which the order of the variables by which the symbols of differentiation are multiplied is not the same as that of the variables with respect to which the differentiations are to be performed" [1853,25]. Thus, such an equation was susceptible to the solution (59.9) by being previously cast into the form (59.8) in virtue of his symbolical extensions. In this respect he drew on his background in permutation theory which forms the basis for that of determinants, revealing thus his concerns for symmetry of forms [which was also the characteristic of Carmichael's work: see 5.7,(9)]. The forms (59.8)-(59.9) are extensions of Carmichael's (57.17), (57.39) respectively. Spottiswoode's brief but condensed paper concluded with a similar example of an equation "in which the variables by which the symbols of differentiation are multiplied are any linear functions of the given variables" [1853,25,29-33].

A less general extension of Carmichael's  $\nabla$ -calculus was invented by Curtis [1854]. While Carmichael's method was based on the possibility to perform operations of the form

$$(59.11) \quad (\nabla - \alpha)^{-1} \quad \text{or} \quad \left( x \frac{d}{dx} + y \frac{d}{dy} + \dots + \alpha \right)^{-1},$$

now Curtis proposed for solution equations which involved the extension of (59.11),

$$(59.12) \quad \left( ax \frac{d}{dx} + by \frac{d}{dy} + \dots + \alpha \right)^{-1},$$

where  $a, b, \dots$  are any positive or negative integers. This extension was based upon the consideration of the formula

$$(59.13) \quad ax \frac{d}{dx} + by \frac{d}{dy} + \dots + \alpha = x^{1/a} \frac{d}{dx^{1/a}} + y^{1/b} \frac{d}{dy^{1/b}} + \dots + \alpha$$

given without proof [1854,272].

Curtis's formula (59.13) can be proved if we show that

$$(59.14) \quad \frac{1}{n} \frac{d}{dx} = x^n \frac{d}{dx^n}.$$

Putting  $x=e^\theta$  we have  $\frac{d}{dx} = \frac{1}{x} \frac{d}{d\theta}$ . Also since  $x^n=e^{n\theta}$  it fol-

lows  $dx^n=e^{n\theta}d\theta$  or,  $\frac{d}{dx^n} = \frac{1}{n x^n} \frac{d}{d\theta}$ . Substituting the values of  $\frac{d}{dx}$ ,  $\frac{d}{dx^n}$

in (59.14), we find that both sides of it equal  $(1/n)d/d\theta$ . Therefore (59.13) holds true, and thus in place of the known formula

$$(59.15) \quad (x \frac{d}{dx} + y \frac{d}{dy} + \alpha)^{-m} 0 = u_{-\alpha} (\log x + \log y)^{m-1} + v_{-\alpha} (\log x + \log y)^{m-2} + \dots + \omega_{-\alpha}$$

we can now have its extension

$$(59.16) \quad (a x \frac{d}{dx} + b y \frac{d}{dy} + \alpha)^{-m} 0 = u_{-\alpha} (\log x^{1/a} + \log y^{1/b})^{m-1} + \dots + \omega_{-\alpha},$$

the index  $-\alpha$  indicating an arbitrary homogeneous function of  $x, y$  of the  $-\alpha$  degree [Curtis 1854, 272-3; (57.26), (57.27), (57.33)] (5).

Curtis offered similar extensions of other key theorems used in Carmichael's solution of partial differential equations, such as (57.5)

$$(59.17) \quad f(\nabla) \cdot u_m = f(m) u_m,$$

now generalized into

$$(59.18) \quad \varphi(x \frac{d}{dx}, y \frac{d}{dy}, \dots) A x^m y^n z^r = \varphi(m, n, \dots) A x^m y^n \dots$$

where  $A$  a constant,  $f, \varphi$  polynomial functions. Let us illustrate Curtis's method with an example.

Assume the equation

$$(59.19) \quad x \frac{dz}{dx} - y \frac{dz}{dy} = \frac{x^2}{y}$$

solved symbolically in the form

$$(59.20) \quad z = (x \frac{d}{dx} - y \frac{d}{dy})^{-1} \frac{x^2}{y} + (x \frac{d}{dx} - y \frac{d}{dy})^{-1} 0.$$

Carmichael's method would had been valid here if the sign of  $y$  was positive. Now, in virtue of (59.13) and its consequences, such as (59.16), for  $m=1$ , and (59.18), we have

$$z = \frac{1}{2-(-1)} \frac{x^2}{y} + f_0(x, y_{-1}), \text{ hence}$$

$$(59.21) \quad z = \frac{x^2}{3y} + f(xy)$$

[1854,274].

Like all the equations solved in Curtis [1854], (59.19) was drawn from Gregory's Examples [1846,361]. Gregory had integrated this equation by means of the substitutions (44.37) which were applied in our study for his solution of Greatheed's equation (42.10) or (44.36). While Gregory's method was a simplification of Greatheed's early symbolical method, now Curtis's own improved over all previous studies of elementary partial differential equations with homogeneous coefficients. Symmetry, elegance and brevity in method and notation are the characteristic features of Curtis's method after Carmichael [1851]('6).

The next to contribute in the symbolic solution of ordinary and partial differential equations was W.H.L.Russell. Born in Shepperton in 1823, Russell graduated from King's College London in 1844, obtaining next his degree in mathematics from St.John's College Cambridge in 1851. Unfortunately no biography exists about him, our only knowledge so far being his review [1857b] of Carmichael's treatise [1855; 5.7]. Both his review and the papers under discussion below give evidence of his defence of Boole's work which was implicitly refuted by Carmichael on the grounds of its "artificial" character [see 5.7,(8)].

Russell's early work consisted of two papers on the integration of linear ordinary and partial differential equations consecutively published as [1854a,b] in the Cambridge and Dublin Mathematical Journal. Drawing on Boole [1844; 4.5,stage 3], Russell cast the ordinary equations under consideration into the form (45.33)

$$(59.22) \quad u + \phi(D) e^{r\omega} du = 0, \quad D = d/d\omega,$$

solved them in series form, and finally -by a method devised by him for the summation of series- he obtained the final solution in definite integral form [1854a,104-110; see also 4.5, stage 7]. A similar method was applied to partial equations [1854b,112-115], the results in both papers being extremely long and complex in form, and apparently of no impact.

Three years later Russell contributed a paper "On the application of the calculus of finite differences to the solution of linear differential equations" [1857a] in the Quarterly Jour-

nal. Based upon Boole [1844;4.5, stage 6 and 8] and Bronwin's "ingenious paper" [1847a;5.2], Russell set off to show how the methods invented for the integration of finite difference equations with variable coefficients would serve to effect the solution of differential equations in definite integral form [1857a, 23-25,29]. Like Hargreave [1850;5.4,(4)], Russell [1857a] was within the spirit of Boole's method when he claimed that "improvements in the knowledge of finite difference equations will lead to corresponding improvements in our knowledge of differential equations" [1857a,31-2].

Another contribution in the spirit of Boole's general method was effected by Russell in a series of papers published in the Philosophical Transactions [see 5.8,(1)] on the calculus of non-commutative operators. We confine <sup>ourselves</sup> here to a partial discussion of his first paper "On the calculus of symbols, with applications to the theory of differential equations" [1861]. The paper opened with a brief review of the history of the subject with focus on Laplace's calculus of generating functions [1.5,1.7] and Boole's "well-known and beautiful memoir" [1844]. His object being to "perfect and develope [sic] the methods" employed by Boole, Russell constructed "systems of multiplication and division for functions of non-commutative symbols" subject to Boole's laws of combination (45.11), arriving thus "at equations of great utility in the integration of linear differential equations with variable coefficients" [1861,69].

Russell assumed two operators  $p, n$  which combine according to Boole's law

$$(59.23) \quad p^n f(n)u = f(n-n)p^n u,$$

$f$  a polynomial function, focusing on applications where  $p=x$  and  $n=xd/dx$ . These values of  $p$  and  $n$  had featured in the greatest part of Boole [1844;see (45.18) and text below (45.20)]. Russell defined "external" and "internal" multiplication between two polynomials of  $n$  and  $p$  in the sense of left and right multiplication respectively. Thus the external multiplication of  $p - n$  upon  $p + n$  will give  $(p - n)(p + n) = p^2 + pn - p(n+1) - n^2 = p^2 - p - n^2$  in virtue of (59.23) which implies that  $np=p(n+1)$ . Similarly, the internal multiplication will give  $(p+n)(p-n) = p^2 - pn + np - n^2 = p^2 - pn + p(n+1) - n^2 = p^2 - pn + pn + p - n^2 = p^2 + p - n^2$  [1861,70-71] ('7).

Under the definition of internal and external division on similar lines, he studied the conditions according to which the

binomials in  $p$  and  $n$

$$(59.24) \quad p\psi_1(n)+\psi_0(n) \text{ and } \psi_1(p)n+\psi_0(p)$$

divide the respective polynomials internally without a remainder [1861,72-75]. He next introduced theorems according to which the binomial

$$(59.25) \quad (p^2 + p\theta(n))^n$$

can be expanded in powers of  $n$ , where  $n>0$  and  $\psi_1, \psi_0, \theta$  all polynomial functions [1861,76-79]. As the conditions and the expansions provided are extremely long, we will only hint at the application of Russell's results to the solution of differential equations via an example.

Let the equation

$$(59.26) \quad x^2(x+1)^3 \frac{d^3u}{dx^3} + 3x(x+1)^3 \frac{d^2u}{dx^2} + (x^3+4x^2+3x) \frac{du}{dx} - (2x-3)u = X,$$

$X$  a function of  $x$ . Equation (59.26) has the symbolical form

$$(59.27) \quad p^3n^3u + p^2(3n^3+n-1)u + 3p(n^3+1)u + n(n^2-1)u = X.x,$$

where

$$(59.28) \quad p = x, \quad n = x \frac{d}{dx}.$$

Effecting the internal division of the coefficient of  $u$  in (59.27) by  $pn+(n-1)$ , equation (59.27) was reduced to

$$(59.29) \quad [p^2(n-1)^2 + p(n+1)(2n-3) + n(n+1)]u_1 = X.x$$

where

$$(59.30) \quad u_1 = pn + (n-1)u.$$

Effecting once more internal division-by the factor  $p(n-1)+(n+1)$ -the final reduction of the initial equation under consideration (59.26) obtained the form

$$(59.31) \quad [p(n-2)+n][p(n-1)+(n+1)][pn+(n-1)]u = X.x.$$

Taking now under account the values of  $p, n$  by (59.28) we find by performing the inverse operations the complete integral

$$(59.32) \quad u = \frac{x}{x+1} \int \frac{dx(x+1)^2}{x^3} \int \frac{dx}{x+1} \int \frac{Xdx}{(x+1)^3}.$$

"the three arbitrary constants being included under the signs of integration" [1861,80].

Russell's extension of Boole's work on non-commutative symbols of operation attracted the attention of Spottiswoode who had a sequel to Russell [1861] published in the same journal the following year. Under the title "On the calculus of symbols" [1862],



Spottiswoode proposed some additions to Russell's investigations for internal division. In the same year and journal we also see Russell's paper "On the calculus of functions" [1862], a subject neglected by mid-19<sup>th</sup>-century analysts. Acknowledging Babbage for his systematic treatment of this subject, Russell applied analytical methods to functional equations. Finally, he had two more papers published in 1862 and 1863 respectively as sequels to his [1861] (e).

In the 1860's the only journal to see numerous, though brief contributions in the realms of the calculus of operations and related subjects was the Quarterly Journal of Pure and Applied Mathematics [see 5.1]. We conclude our account with some early contributions as indicative of Carmichael's and Boole's wider influence, such as Greer [1860] and Roberts [1860a].

Greer's paper was devoted to "A theorem in the calculus of operations, with some applications" [1860]. The theorem proposed is in his words:

"Let  $\psi$  be a symbol of operation obeying the two distributive laws

$$(1) \quad \psi.(u+v) = \psi.u + \psi.v$$

$$(2) \quad \psi.uv = v\psi.u + u\psi.v$$

(59.33) and let  $M$  be such a quantity that

(3)  $\psi M$  shall = 1 identically; then will the solution of the equation  $\psi.\Omega = u$ , be

$$(4) \quad \Omega = M.u - \frac{M^2}{1.2}\psi.u + \frac{M^2}{1.2.3}\psi^2.u - \dots (9).$$

This theorem was suitably applied to equations already solved by Gregory and Carmichael, such as to equation

$$(59.34) \quad \left(\frac{d}{dx} + \frac{d}{dy} + \frac{d}{dz}\right)\Omega = xyz$$

[see Greer 1860<sup>a</sup>, 149-150; Gregory 1841, 350; Carmichael 1855, 65 and (9) above].

Greer went on to discuss Carmichael's symbol  $\nabla$  in correlation with  $D$  instead of  $x D$ ,  $D = d/dx$ . Letting

$$(59.35) \quad L = \log u_1,$$

in other words  $L$  denotes the logarithm of a homogeneous function of the degree unity, it followed that

$$(59.36) \quad \nabla L = L \nabla + 1;$$

the latter equation can be compared with the known one (55.44),

$$(59.37) \quad Dx = xD + 1,$$

thus (59.36) enables us to compare  $\nabla$  not with  $xD$ , but with  $D$  [1860,152,fn]. Greer further remarked that similarly the analogue of the "well-known" equation (47.2),

$$(59.38) \quad f(D+\phi'x) = e^{-\phi(x)}f(D)e^{\phi(x)}$$

would be

$$(59.39) \quad f(\nabla + \phi') = e^{-\phi}f(\nabla)e^{\phi},$$

$\phi$  any function of  $\log u$ , and  $\phi' = \nabla \cdot \phi$  [1860,153]. The theorem (59.39) was given by Carmichael [1851,283]. The analogy between (59.38)–(59.39) justifies Greer's correlation between  $\nabla$  and  $D$ .

We conclude with S.Roberts, a prolific mathematician who contributed up to the year 1883 over sixty papers. Born in Hackney in 1827, he matriculated<sup>at</sup> London University<sup>and graduated</sup> in 1845 with honours in classics and mathematics. He died in 1913 after obtaining several academic distinctions, leaving behind a considerable mathematical correspondence with De Morgan, Salmon, Cayley and Sylvester; the latter referred to him as his "distinguished mathematical friend" [Sylvester 1867,609,fn]. Besides mathematics he was also<sup>active in</sup> chess, philosophy and geology.

Roberts commenced his research in 1848, devoting the first fifteen years to the study of geometry and the calculus of operations. In a "Note" [1860b] he provided a "slightly more general form" of Greer's theorem (59.33). In his paper "On the solution of differential equations" [1860a] he inquired into the circumstances under which an equation in symbolical form is regarded as solved. In this respect he was motivated by Hargreave [1848; see (53.8)] referring additionally to Boole [1844;4.5, stage 4] and to the scarcely ever cited Bronwin [1847b;5.2] (10). Like De Morgan and Cayley, he contributed on Arbogast's theory of derivations [Roberts 1861;1866;see also 5.10].

## 5.10 Symbolical methods: impact and utility within mathematical sciences in mid-19<sup>th</sup>-century England.

In the previous chapter we studied the genesis and the peculiar features of Boole's general method in analysis [1844; 1845d] and showed that the role of the EFE was a major stimulus [4.6]. Our conjectures are reinforced by Kelland's speech

delivered at the Royal Society of Edinburgh in 1858 on the merits of Boole's work. Focusing on Boole [1844] Kelland commented as follows:

Many problems of no very great apparent complication had baffled the ingenuity of mathematicians. Solutions were, it is true, obtained, but the processes were so indirect and unsatisfactory, that they were something like excrescences on the smooth face of science. Of this class of problems is an equation which occurs in the theory of the figure of the earth. Mr Airy, in his "Tracts", gives simply the result, without the slightest indication of a process [3.2]. Mr Gaskin and Mr Leslie Ellis had attacked this individual problem with partial success [4.2-4.3]. But Mr Boole's "New Method" not only set the logical question of dealing with separation of symbols in a clear light, but completely effected the solution of all that class of problems of which this was a particular example<sup>(1)</sup>.

This chapter has focused so far on the effects of Boole [1844; 1845d] on the symbolical treatment of differential equations. We conclude with a critical overview of his impact, hinting at the wider effect of separation of symbols in the development of mathematical sciences in the period from the 1840's to 1860's. Our point of departure is the examination of the three distinct ways in which Boole's method was developed by his contemporaries. We discuss next the impact that operator algebras had in general on the shaping of mathematical views and theories, concluding by connecting our study of operator methods with the rise of invariant theory by Cayley and Sylvester in the 1850's and 1860's.

Boole laid the foundations of his general method in [1844] including further extensions and applications in [1845d; 1847c]. With modesty he declared in his initial paper that he viewed his incomplete method as a potential tool of unity in analysis [4.6,(10)]. The fact that both he and Bronwin delved further into a study of [1844] broadening its boundaries, reinforced his belief that the germs of his method could give rise to one that would be both "genuine" and "complete" [5.2,(12)]. The essence of Boole's method lay in his FTD (45.26) which reduced problems in differential equations to ones in finite difference equations and vice versa; the language of the calculus of operations

merely a procedural device which enabled quick and elegant proofs. This fundamental aspect of Boole's theory had very few followers, namely Bronwin [1847a;5.2], Hargreave [1850;5.4] and Russell [1857a;5.9]. Avoiding complex symbolic theorems, Hargreave expressed his belief in the existence of a universal method [5.4,(1),(4),(5)], unable though to effect its realization. Boole dropped his project, switching to logic<sup>(2)</sup>. The search for a universal method terminated by 1851 when Bronwin implicitly proved it as groundless. For, following closely on Boole's steps for five years, Bronwin perceived such a search to be fruitless since distinct classes of equations demand of radically different treatments [5.4,(6)]. No matter the limited impact that Bronwin [1851a] might have had on his contemporaries, the fact is that all attempts from then onwards followed on the path of "partial" [5.4,(1)] and not of "general" methods -further proving the failure of Boole's ambition, though not of his actual method.

The second issue is the language of Boole's method and its related symbolic theorems in terms of non-commutative operations, their germs founded in Murphy [1837;3.3]. Exploring the properties of the operations  $x$  and  $d/dx$  in the dual theorems (47.1)-(47.2), Boole was impressed by the similarity in the development of binomials of these operations with Taylor's theorem [4.7,(2)]. The study of such operations attracted the interest of numerous researchers who were charmed by the property of the calculus of symbols to effect abstraction, generalization and to display analogy with the common theorems of the calculus. Bronwin and Donkin extended Boole's dual theorems by redemonstrating them via more general ones [5.2;5.5,(3)], while Hargreave -avoiding their use- introduced a new method based on his empirical principle (53.8) on the interchange between  $x, D$

(510.1)  $x \longrightarrow D, D \longrightarrow -x$ , where  $D=d/dx$

motivated by the application of (47.1)-(47.2) by Boole [4.7, text above (3);5.3,(6)].

With Graves, Hargreave's principle and a variety of dual theorems were rigorously established as derived from a unique law (55.45)

$$(510.2) \quad pn - np = 1$$

[5.5,(3),(6),(9)]. Graves further studied exponential operators with non-commutative exponents, thus improving and extending Murphy's pertinent work (33.22), his theory suitably extended to

non-commutative partial exponents by Carmichael [(55.34); 5.7, (7); 5.8, (18)]. Greer argued that the analogy between  $xD$  and  $\nabla$  which featured in Carmichael's work [5.7] could be modified to one between  $D$  and  $\nabla$  [(59.36)]. Finally Russell, and after him Spottiswoode, delved in the 1860's into the most general study possible of complex binomials and polynomials of the operators  $x$  and  $d/dx$  [5.9 text and (8)].

The third and last issue concerning Boole's method is its actual utility in practice, for with few exceptions, neither his FTD, nor the complicated theory of non-commutative operations were of any sustained success in applications<sup>(3)</sup>. It was the part of his general method as based principally on the known theorems (45.19)-(45.20) and on the technique (45.21)-(45.23) -via which an equation was cast into symbolic form- which inspired a variety of slightly different methods suitably applied to differential equations. As we saw, Boole's own approach as based on theorems 1,2 [4.6] had hardly any follower [4.6, (11); 4.8, (5); 5.6, (2)]. However, via his theory of binomial equations [4.5, stage 3; 4.6] -the only part of his method to be satisfactorily developed and widely applied by him- he solved both the EFE and the LE in finite symbolic form, challenging his contemporaries to inquire further into a study of equations of a more general nature or to invent partial treatments which would effect the solution of these two equations in a different form [5.2-5.4; 5.6; 5.8, (12)]. The interesting by-products of this obsessional inquiry were a considerable growth of symbolic algebra, a concern for the interpretation of complex symbolic forms and interesting disputes on the role of symbolic language, its utility and tendency to invoke fallacies if not carefully handled [5.2, (16); 5.3, (7); 5.4, (5); 5.8, (9), (11), (17)-(20)].

What was the wider impact of this growth of operator algebras in mid-19<sup>th</sup>-century in the realms of mathematical sciences? First of all, these inquiries culminated in three textbooks, Carmichael [1855] and Boole [1859; 1860] which together reflect best the achievements of most of the British researchers in the field of analysis and of the authors' individual epistemological and educational views [5.7, (8), (9); 4.5, (5), (8)]. However, though not overlooked in the second half of the century [4.6, (11)], they did not serve as manuals of abstract symbolical studies in that direction. Postponing a further in-

quiry on this matter to chapter 9, we say here that what prevailed from the 1860's onwards in the realms of differential equations was the spirit of the Lagrangian algebraic calculus and of Gregory's elementary application of the separation of symbols [4.1,(1); 4.2,(2)].

Gregory's Examples [1841] was to survive much longer than any of the textbooks mentioned so far, well up to the 1900's [7.1,(25)]. It proved useful to certain figures, such as Curtis and Greer [5.6,(1); 5.9] and formed an invaluable source of examples and methods for Boole [1844;1859;4.5] and Carmichael [1855;5.7]. This fact shows that neither Gregory's own methodology, nor the variety of other methods displayed in his Examples were superseded by the sophisticated but complicated methods suggested after Boole [1844]. Carmichael's preference for Gregory's over Boole's style further proves that simple procedures win over "artificial" ones [5.4,(1);5.7,(8)]. Finally, contrary to Boole's early work which was of an advanced level with little inclination for foundational speculations, Gregory's delved into the nature of symbolic algebra offering the grounds for a novel conceptual approach in the realms of mathematical sciences which involved a minimum of symbolical language easily assimilated and applied by the more conservative mathematician.

These assertions will be illustrated with two cases, their common point being Gregory's view "that Symbolical Algebra is a Calculus of Operations", reformulated also as "Symbolical Algebra must be considered as a science of operations represented symbolically" [1843a,242]. We first examine O'Brien's views on symbolical innovations as in his paper "On symbolic forms derived from the conception of the translation of directed magnitude" [1852]. It opened with the following remarks:

There can be no doubt, that time and ingenuity have been often wasted in devising systems of notation, and new methods of algebraical representation, which have never proved of any service in advancing the cause of science. It is not surprising, therefore, that symbolical innovations, if they have not the strongest and most obvious reasons to recommend them, are generally received with little favour by mathematicians<sup>(4)</sup>.

He further argued why on these grounds he chose to adopt Gregory's views on symbolical algebra "to a certain extent" as

these views "appear to my mind to form the most satisfactory theory of Symbolical Algebra" [1852,163]. However, he proposed a much simpler notation and introduced the notions of commutative and distributive functions as necessary for the algebra of vectors he was to discuss [1852,164-5]<sup>(5)</sup>.

Thus the terms "symbolic form", "formula" or "geometry" appeared in the title and text of O'Brien's papers [1852;1847] and others published by him and several mathematicians of that time<sup>(6)</sup>. These terms were used loosely, recalling De Morgan's [3.9] rather, than Boole's application [1847c;5.8]. The spirit of O'Brien's conceptual assimilation of "function" or "operation" in Gregory's and De Morgan's wider terms is raised in a curious paper written by A.J.Ellis, the first English mathematician to solve the EFE [3.2].

Entitled "On the laws of operation, and the systematization of mathematics" [1860], Ellis's paper had as object "to give a firmer basis to the calculus of operations, to assign the strict limits and connexion of the mathematical sciences, and to found them upon purely inductive considerations, without any metaphysical or a priori reasoning" [1860,85]. Avoiding the use of symbols, he defined the "product" of two operations, and instead of inverse, he called two operations "reciprocal when their product is unity", claiming that "The above general conceptions and laws of combined operations hold for any operations whatsoever with their appropriate operand objects; but the nature of operations and operands requires especial study" [1860,86]. Thus the actual calculus of operations was excluded from his discussion.

Ellis used the concept of operation to link a multiplicity of branches of mathematics culminating in successively defining the "algebra" of "integers", "fractions", "tensors", "scalars", "differences", "differentials", "derivatives" concluding with "the algebra of quaternions". This tendency for unity by an inductive process echoes of De Morgan's exposition in [1836]; particularly when we come across statements such as "The inversion of formations [in the algebra of fractions] is less limited than [in the algebra of integers]" [1860,87] or that "the algebra of differentials [...] is, in fact a mere simplification of that of differences" [1860,91]. Despite its interesting objective, Ellis's paper is hardly satisfactory for the standards of its time, being vague in definitions, lacking illustrations or references and

hardly focusing on the intricate problem of inverse operations. However, it nicely reflects the wider tendency for "algebras" of all kinds and it should be mentioned that he refers not only to Gregory's [Servois's] three widely diffused laws (44.7) but also to the less popular associative law [1860, 86]('7).

Ellis's tendency for unity in the mathematical sciences reminds of the similar attempts by Herschel and Babbage [2.9], De Morgan [3.6;3.9], and above all by Boole [5.10,7.1;8.7-8.9]. In a loose sense the calculi of operations and functions formed the basis for this motivation which in the two latter cases had interesting results in their study of logic [chapters 6-8]. Unity of the most diverse branches of culture was proposed by Sylvester through the notion of "morphology", a term suggested after Darwin [5.9,(2) and text]. In an almost poetic way, this trend of research, followed as we saw by Spottiswoode [5.9], explored similarity between forms and cared for beauty and symmetry within algebraic forms. Sylvester's work on the "calculus of forms" -carried out in the realms of invariant theory- reflects this attitude as much<sup>a3</sup> as Spottiswoode's own on determinants [5.9,(1), (3)]. Together with Cayley, these new algebraists made use of excessive symbolism, and though indifferent to the current emphasis on differential equations, they did borrow tools from the calculus of operations which they combined with the new concerns of research. The study of these links within the work of Cayley and Sylvester forms the last part of this section.

A. Cayley was the senior wrangler of Cambridge University in 1842, and together with Sylvester the most prolific algebraists of their time. Devoted to conveyancing from 1849 to 1863, he jealously devoted his leisure time to the pursuit of mathematics, producing a total of over <sup>vins</sup> hundred papers on pure mathematics in his lifetime; he is best known for his contributions in invariants, matrices and quintic equations. In 1863, Cayley was elected professor at Cambridge, a post he kept to his death in 1895. An exceptionally eminent mathematician, he was comparably ineffective as a teacher, his audience often consisting of one pupil. But "that pupil was Andrew Russell Forsyth" [Roth 1971,230], the known textbook writer who succeeded Cayley in 1895('8).

Cayley was an admirer of Boole whose work on invariants in 1841 motivated his own first work in 1845. In his turn Cayley



stimulated Sylvester to direct his interests to that subject, called by him "the Darwin of the English school of mathematicians" [Sylvester 1869a,5]. A correspondence of mutual substantial influence started in 1850, but unlike the friendly collaboration of Babbage and Herschel [2.3-2.9], the uneasy temperaments and common pursuits of Cayley and Sylvester made them often behave like rivals, worried immensely about priority, especially the latter. J.J.Sylvester studied for a while in London under De Morgan but was expelled from the University in 1828 due to his aggressive temper. He graduated from St. John's College in Cambridge in 1837 as a second wrangler and held since then <sup>different</sup> academic posts. A keen linguist and poet, Sylvester invented his own mathematical vocabulary borrowing terms from astronomy, grammar and biology. After T.H.Huxley's assimilation of Darwin's evolutionary theory he used the term 'morphology' to link all branches of art and science, focusing on linear associative algebras<sup>(9)</sup>.

Both algebraists were concerned with the symmetry of algebraic forms; Sylvester in particular could see and appreciate the beauty of complexity of their inner structure, as for example in the case of the binary quintic

$$(510.3) \quad a_0x^5 + a_1\binom{5}{1}x^4y + \dots + a_5y^5 \quad (10).$$

He constantly used linear partial differential operators

$$(510.4) \quad a_1 \frac{d}{dx_1} + \dots + a_n \frac{d}{dx_n}$$

from 1851 onwards, in connection with his study of homogeneous functions and the theory of forms [1851 -see 5.7 text below (12); 1852a,b]. As his studies developed, Sylvester made use of more complicated operators, devoting in fact a few papers exclusively on their properties, such as his "On the multiplication of partial differential operators" [1867].

Cayley applied the calculus of operations in a diversity of papers prior to a more systematic use in the 1850's in invariant theory. In his paper "On Lagrange's theorem" [1843] he was based on "a well-known form" of this theorem,

$$(510.5) \quad \frac{Fx}{1-hf'x} = \left( \frac{d}{du} \right)^h \left( F(u+hf(x)) \right),$$

in order to determine the expansion of  $F(x)$  where  $x=u+hf(x)$

[1843,283-285]. Further, in a paper on Laplace's coefficients [1848], published as his [1843] in the Cambridge and Dublin Mathematical Journal, he applied the operator  $\Delta$ , commenting in the end: "The mode of employing the general theory of the separation of symbols made use of in the preceding example, may easily be applied to the solution of analogous questions" [1848, 12].

Both Cayley and Sylvester ~~did not~~<sup>did not</sup> delve into Boole's general method or in<sup>t</sup>he solution of differential equations by operator methods. They did, however, employ ardently operators of the form (510.4), and <sup>the</sup> few references or analogies perceived show that they were in some distant mutual touch with the current symbolical methods. For example, Carmichael was aware of Sylvester [1851] before he introduced his  $\nabla$ -calculus [5.7, text below (12)]. Cayley's notation in the early 1850's [Crilly 1986,245]

$$(510.6) \quad \square = a_0 \frac{\partial}{\partial a_1} + 2a_1 \frac{\partial}{\partial a_2} + \dots + na_{n-1} \frac{\partial}{\partial a_n}$$

resembles Carmichael's introduction of  $\square$ , via

$$(510.7) \quad \square = a \frac{d}{da} + b \frac{d}{db} + \dots$$

in [1852,131]. Both Cayley and Sylvester used operators  $P, Q$  which combined according to the law

$$(510.8) \quad P.Q = PQ + P(Q)$$

in the 1870's [Crilly 1988,334]; the property (510.8) was first formulated by Murphy (33.45) in 1837. Donkin drew on Cayley [1848] for his own study of Laplace's coefficients in [1857,5.7;5.8] and finally both algebraists showed a great interest in the still fashionable theory of Arbogast's derivations after De Morgan's pertinent research<sup>(11)</sup>.

Thus the language of the calculus of operations found a new field of useful applications in the algebras of Cayley and Sylvester, but despite the links noticed above, Boole's own considerations seem absent. However, our limited investigation into the concerns of these algebraists reveals a deeper proximity in epistemological interests and hints at an apparently greater influence than that effected by Boole on Cayley's early work on invariants. The general knowledge and appreciation of Boole's work can, for example, be seen in Sylvester paper "The story of an equation in differences" [1869b,227,fn] where Boole [1860] was

called a "valuable treatise on finite differences". We shall also see that a most fruitful mutual influence was effected through Cayley's and Boole's correspondence in 1847 on the application of symbolical procedures to logic [7.5,(3),(4);8.2].

While Cayley directed his own researches strictly to mathematics, borrowing operators as mathematical instruments, Sylvester combined his linguistic knowledge and skill in morphology, ascending <sup>above</sup> De Morgan's and Boole's speculations on the form-matter distinction to a remarkably higher degree. We close with a quotation from Sylvester [1867] devoted solely to a study of operators which is highly indicative of the continuation of development of notation from Brinkley until after Boole's death. Having suggested in an earlier paper the notation " $\psi^*$ " to signify "the process of operating with  $\psi$  upon all that follows" [1866,461], Sylvester delved deeply into the nature of symbols of operation and quantity and further applied his investigations in [1867], appending to a footnote the following interesting observations:

With the star sign [...] algebra, as far as yet developed, may revel in unbounded freedom of utterance. The rise of this star above the mathematical horizon marks one of the epochs of algebra. It is worth remarking how already it is beginning in its turn to assume the attributes of quantity [...]; so that apparently it is destined to run the same course as Newton's fluxional symbol, which is, and of fatal necessity must have been, superseded by the lettered symbols of Leibniz, which have now long ago, to all intents and purposes, become converted into a new species of algebraical quantity. As soon as it becomes necessary (as will probably before long be the case) to express the specific relation of the star to something which limits and discriminates its mode of application, it must in its turn develope into a third species of symbolical quantity; and so there may be in store for the future of algebra an endless procession of more and more abstract symbols of operation, each successively developing into a more and more subtle species of quantity, suggesting the analogy of successive stages of so-called imponderality in the material world<sup>(12)</sup>.

### Mathematical aspects of De Morgan's logic: 1831-1860.

#### 6.1 Introduction.

In the previous chapters we covered the genesis of the calculi of functions and operations in late-18<sup>th</sup>-century France and their development in England up to mid-19<sup>th</sup>-century. In the late 1810's Babbage viewed his calculus of functions as a powerful analytical tool and as a mathematical language par excellence [2.9]. Two decades later De Morgan perceived in the foundations of this calculus a methodology applicable to symbolic algebra and the calculus of operations [3.5-3.9]. On fairly parallel lines, Herschel's calculus of finite differences, enriched with Servois's laws and rigorously established by Murphy and Gregory, gained a new power in Boole's hands in 1844 [2.3;2.9;3.3;4.4-4.5]. Herschel's favourite method of separation of symbols saw a most significant utility in Boole's general symbolical method, by means of which most of the prevailing problems in analysis were now reduced to a simple one: the solution of a differential equation [4.5;5.10].

The diffusion and development of symbolic algebras raised questions about rigour in mathematics. De Morgan was the first to notice this in the early 1830's in connection with the instruction of algebra. He stressed that a student should be acquainted with logic before becoming a mathematician, for "The art of reasoning is exercised by mathematics, not taught by it" [3.4 (14)]. Fallacies arising from a misuse of symbolical procedures urged Boole to perceive in the late 1840's "the almost entire absence of any direct study of the laws of correct reasoning in connexion with the practical discipline of modern science" [5.2, (16)]. These fundamental laws of sound deductive reasoning formed the subject of a separate study for both mathematicians, their background experience in symbolical procedures inevitably influencing the shaping of their work on logic.

The scope of this and the next two chapters is to bring to the surface those mathematical issues and technicalities which

underlay their logical contributions, most particularly the issues connected with the key notions of De Morgan's and Boole's unifying methods: respectively the functional and differential equations. Though their first major contributions in logic appeared in the same day in 1847, De Morgan preceded Boole and deserves to be discussed first. So, before giving way to Boole in the next two chapters, the present chapter is devoted to De Morgan.

Here we first give a brief review of English logic as it prevailed early in the 19<sup>th</sup> century. In 6.2 we hint at Kirwan's and Whately's role in the revival of Aristotelian logic during the period 1807-1826, noticing some early vague links between logic and mathematics. We proceed in 6.3 to the issue of the quantification of the predicate as it appeared in Bentham's work in 1827 and as independently discussed by Solly and Hamilton in the late 1830's. Though the work of these logicians was not of an immediate or direct impact on De Morgan and Boole, still the knowledge of the state of logic prior to their major contributions in 1847 is indispensable in our study.

Section 6.4 contains a general outline of De Morgan's contributions in logic in chronological order, from 1831 to 1860, picking out the main novelties in De Morgan's logic and their mathematical underlying issues, and stressing those which played a most significant role in the shaping of his logic of relations in 1860.

We start at 6.5 with the issue of the abstract copula as developed from 1831 to 1850, hinting at the first instances of composition of relations. Then 6.6 contains De Morgan's syllogistic system, initially introduced in 1847, followed by its elaboration and modification in 1850. In so doing we clarify some obscure points concerning Hamilton's and De Morgan's dispute on the quantification of the predicate, illustrating the former's subtle role in the evolution of De Morgan's syllogistic.

After Hamilton, Mansel attacked De Morgan in 1851 for having subordinated logic to mathematics. The latter's answer to this attack in 1858 forms part of 6.7, where we will see also instances where algebraic processes influenced logical ones, and his views on the links between mathematics and logic. Treating in

6.8 his calculus of relations as shaped by 1860, we proceed in 6.9 to a comparative study between this calculus and that of functions [1836]. Instances from De Morgan [1836] on functional equations will be evident in the course of our study as it was there for the first time that reasoning by signs got thoroughly examined by him and applied with effect in his later mathematical work [3.5-3.9].

6.2 The revival of logic in England; the work of Kirwan and Whately: 1807-1826:

Aristotelian logic was at a low ebb in English universities at the turn of the century. This fact is hardly surprising given the harsh attacks on syllogistic logic by Descartes and empirical, semiotic and common-sense philosophers in the 17<sup>th</sup> and 18<sup>th</sup> centuries [Van Evra 1984,3-9; 1.8]. It has been argued that the epistemology of Locke, D.Stewart and Condillac had a subtle, though rather marginal, influence on English mathematicians of early-19<sup>th</sup>-century<sup>(1)</sup>. As we shall see, these philosophers were not totally ignored by writers on logic of that time either. However, just as Condillac's La langue des calculs [1798] could not serve as a methodology in higher algebra [1.8,(12)], the epistemological theories put forward by Locke or the Idéologues could not form a satisfactory alternative for syllogistic logic. According to Kirwan, Whately and Bentham, Aristotelian syllogistic was indispensable for the study of law, ethics, theology, mathematics and botany..

By calling logic a "science" in his Logick [1807], Kirwan was the first to take a step towards restoring its neglected role and stimulating interest in its study. We have no information about the popularity and impact of Kirwan's book. It is, however, highly probable that it influenced Whately, whose Elements of logic [1826] not only insisted that logic is a science, but established the proper grounds for a general revival of logical studies in England. Absent from many classical histories of logic in our century, Whately recently gained attention; in fact his Elements [1826] saw a reprint in Dessi's edition dated [1988]<sup>(2)</sup>. As Kirwan's less-known role deserves some attention, we open our

account with a biographical note on him.

R. Kirwan was born in Galway, Ireland in 1733, but his family was of English descent. At seventeen he went to the University of Poitiers where he studied chemistry, Latin and French for four years. He next studied law in London, obtaining in his life time several academic distinctions for his eminence as a chemist and philosopher. Until his death in 1812 he resided alternately in Dublin and London, being in frequent contact with distinguished Irish, English and Continental scientists and philosophers of his time. Between 1781 and 1805 he published over forty papers on geology, chemistry, mineralogy and meteorology, and was equally outstanding for his contributions in metaphysics, philology, theology and baroque music. Beside his vast range of knowledge, Kirwan was remembered for the simplicity and purity of manners which at old age developed eccentricities making him a subject of anecdotes<sup>(3)</sup>.

Like many of the intellectuals discussed in our thesis, Kirwan was very keen on linguistics. On this matter he exchanged long conversations with H.T. Hooke, known for his interests in universal language and his study of Locke<sup>(4)</sup>. Widely read in the prevailing epistemological theories of his time, Kirwan put aside his scientific studies in the last years of his life in order to pursue his earlier philosophical concerns. For example, in 1805 he wrote an essay in which he tried to prove that Greek was man's first language. But the crown of his epistemological inquiries was his two-volume treatise on Logic; or an essay on the elements, principles and different modes of reasoning [1807], intended for the use of students of law [see (3) above].

Though not a study of formal logic in the strict sense, Kirwan's textbook was a first attempt to call attention to the necessity of "the detail of the varieties of propositions" which was overlooked by the "excellent metaphysician" Condillac when he rejected Aristotelian syllogistic as utterly useless. Perhaps, Kirwan wrote, this was due to the fact that Condillac had never speculated cases of law or theology where syllogism is indispensable [1807, xi].

Kirwan drew heavily on Condillac's Grammaire 1775 and on Condorcet's work on probability [Kirwan 1807, vol. 2, chap. 7]. Addi-

tionally he drew on the logic of Watt 1725, and Dunker 1748, as well as on Locke's Essay concerning human understanding 1690. Another important influence was Arnauld's Logique ou l'art de penser 1660, commonly known as "the Port-Royal logic". This cartesian approach aimed at establishing a theory of clear and distinct ideas. Its most influential doctrine was the distinction between the "comprehension" and "extension" of a term, the former renamed by Hamilton as "intension"<sup>(5)</sup>.

For Kirwan the proper object of logic "is to determine with precision, the exact signification of words, in what relation so ever they may stand, the general and particular properties and varieties of propositions, the nature of ratiocination, the validity of the grounds on which it rests, and lastly, the means of investigating truths" [1807, i]. By calling logic a "science", he implied "a system -an arranged collection of truths immediately or mediately deduced from first principles" [1807, 3].

After semiotics and the Port-Royal logic, Kirwan saw the significance of mathematics whose chief advantage lay in the clarity of its definitions and the simplicity of its signs. Foreshadowing De Morgan's views some thirty years later [3.4, (14)], Kirwan remarked that "in no science have the rules of logic been so well observed, as in that of mathematics, for, in none is their application so obvious and easy" [1807, iii-v]. He further added that it is wrong to believe that by exercising ourselves in mathematical demonstration we acquire a habit of "reasoning closely"; "If logick has had its sophisms, mathematics has had its paralogisms, and algebra, in particular, many absurdities". Therefore, and here the similarity with De Morgan's claims [see 3.4, (14), (15), (22)] is striking, "Logick is frequently necessary for detecting the errors of a mathematician" [1807, vi].

The analogy between logic and mathematics was further contemplated by R. Whately. He graduated in 1808 from Oxford University in classics and mathematics. After serving in a rural parish from 1821 until 1825, he returned to Oxford for a few years, becoming in 1831 Archbishop of Dublin where he remained until his death in 1867. He was known as a talented teacher and as one of the best conversationalists of his days. Unlike Kirwan, Bentham



and Hamilton. Whately was not a scholar of historical detail, a fact illustrated in his Elements [1826] which lacks the abundance of references found in the works of his contemporaries. His work on logic first appeared as a two-part article in 1823 in the Encyclopedia Metropolitana, and three years later as a book [1826] in a slightly modified and extended form<sup>4</sup>.

More emphatically than Kirwan, Whately stressed that the role of logic "as instituting an analysis of the process of the mind in Reasoning" is strictly that of a "science"; he further distinguished its role as an "Art" when he considered it with reference to its practical rules [1826,i]<sup>5</sup>. He regarded as wrong the view shared by earlier logicians about the role of logic as furnishing a "peculiar" method of reasoning instead of a method of analysing that mental process "which must invariably take place in all correct reasoning" [1826,ii].

On the proximity between logic and mathematics he wrote: There is in fact a striking analogy in this respect between the two sciences. All numbers (which are symbols of Arithmetic) must be numbers of some things [...]; but to introduce into the science any notice of the things respecting which calculations are made, would be evidently irrelevant, and would destroy its scientific character: we proceed therefore with arbitrary signs representing numbers in the abstract. So also does Logic pronounce the validity of a regularly -constructed argument, equally well though arbitrary symbols may have been substituted for the terms; and consequently, without any regard to the things, signified by those terms. And the possibility of doing this (though the employment of such arbitrary symbols has been absurdly objected to, even by writers, who understand not only Arithmetic but Algebra) is a proof of the strictly scientific character of the system<sup>6</sup>.

Another characteristic statement of Whately concerns the connection between logic and language. He concluded that all arguments can be reduced to the form of Aristotelian syllogism; ...and since Logic is wholly concerned in the use of language, it follows that a Syllogism (which is an argument stated in a regular logical form) must be "an argument so expressed, and the conclusiveness of it is manifest from the mere force of the expres-

sion", i.e. without considering the meaning of the terms e.g. in this syllogism, "Y is X, Z is Y, therefore Z is X" : the conclusion is inevitable, whatever terms X, Y, and Z, respectively are understood to stand for. And to this form all legitimate arguments may ultimately be brought".

Whately further defended the scientific status of formal logic, which was strongly doubted by Bacon, Locke and D. Stewart, by drawing the reader's attention to its utility in botany, ethics, law and mathematics [1826, 133, 244, 247-252]. His chapter on fallacies and the method of counter-examples form the most original of his contributions in the rest of his work [Whately 1975, Introduction -see (2) above; Corcoran 1980, 630]. In contrast with Kirwan [1807], Whately's book lacks any reference to French logicians. It draws, however, often on Aldrich [1826, 87, 89, 134; (5) above], while its "Introduction" is enriched with references to ancient Greek philosophers, such as Socrates, Zeno or Pythagoras. According to [Van Evra 1984, 9] "Whately's lack of detailed knowledge of the history of logic was more than compensated by a clear grasp of the fundamental issues facing logic".

Soon after its publication, Whately's book saw several reviews, translations and new editions, and from 1827 up to the end of the century several new works were to be published based on it, as textbooks for students of both Oxford and Cambridge<sup>10</sup>. Its role might be compared with that of Herschel's and Peacock's Examples [1820] in Cambridge University in the 1820's and 1830's, but its success, in being popular for almost a century is comparable with that of Gregory's Examples [1841; see 3.2; 4.8; 5.10]. In fact Whately deserves the title of the "restorer of logical study in England" attributed to him by De Morgan [1860c, 247]. De Morgan would advise the study of Elements [1826] in his early work on the instruction of mathematics [1831, 71, fn; see 3.4; 6.5].

Inevitably, by being a very first exposition of formal logic in England, Whately [1826] had several weak points, bitterly attacked by his critics. Moreover, it disturbed followers of either Aristotle or Locke. Smart wrote an essay [1851] on the apparent retardation that the book caused in the progress of metaphysical

philosophy, in an attempt to revive interest in Locke's theory, which was fading away [Dessi 1988,xxviii;see(4)above]. However, most of Whately's critics, including Bentham, Solly and Hamilton on whom we will readily focus, admitted the value of his work; J.S.Mill wrote in 1828 that Whately [1826] was a "clear exposition of the principles of syllogistic logic, and vindicating it against the contemptuous sarcasms of some modern metaphysicians". The mere fact that Whately stimulated his contemporaries challenging critical comments proves the fact that his project was fulfilled [see also 6.4,(5)].

### 6.3 Bentham, Solly, Hamilton and the issue of the quantification of the predicate: 1827-1841.

G.Bentham, born near Plymouth in 1800, was nephew of J.Bentham, the well known utilitarian philosopher on ethics and law who had shared Kirwan's and Tooke's [6.2,(4)] interests. G.Bentham acquired at an early age conversational proficiency in Russian, French and German, and later in life he could read works in fourteen languages. At Montauben, France, he studied Latin, natural philosophy, Hebrew and mathematics, making his debut as a botanical author in 1826. Like his uncle he studied law, but inheriting his property he was able to give up the legal profession for botany, a subject to which he devoted over fifty years making outstanding contributions. Besides financial wealth, G.Bentham inherited also his uncle's interest in logic together with his manuscripts on a new system of logic in which the role of language was prominent. But while his uncle followed the *Idéologues* -particularly Degerando- believing firmly that Lavoisier's Condillac-inspired reform of chemical language was the best illustration of the practical value of the philosophy of language, Bentham fiercely rejected semiotic epistemology as inadequate, defending Aristotelian logic.

Urged by the need to clarify the subtleries involved in the classification of botany, Bentham used his uncle's manuscripts to write his book Outline of a new system of logic, with a critical examination of Dr.Whately's "Elements of logic" [1827]. In its "Preface", Bentham argued more emphatically than Kirwan that

French "logiques" were utterly useless;

I have also omitted all mention of French logical writers, who consider the subject in a totally different light from that in which British authors have viewed it. Condillac first rejected the whole Aristotelian theory, and applied the name of Logic to the enquiry into the mode of action of our intellectual faculties. Since then Destutt de Tracy [...] published four volumes of Elements of Ideology (including one of Logic) which have become the standard to which the subsequent continental writers have generally referred. His principles are founded on those of Condillac; he suppresses, not only the theory of syllogisms, but also the rules of the guidance of any of our intellectual faculties, confining the science to "pure speculation", unapplied to any practical use<sup>(2)</sup>.

The corpus of Bentham [1827] consists of a running commentary on Whately's work, combined with an account of his own ideas on logic. Considering the Elements [1826] as the "lost and most improved edition of the Aristotelian system" [1827, vii], Bentham acknowledged Whately's emphasis on calling logic a "science", mentioning that a similar distinction between "Art" and "Science" had been effected also by his uncle [1827, 12]. In the course of critically commenting upon Whately [1826], Bentham made it clear that most of his objections did not concern so much the specific book under review but the limited scope of traditional logic in general which was badly in need of new extensions. He incorporated thus in chapter 8 of his book a new scheme for the formalization of syllogistic premises by using equations with symbolic quantifiers.

Bentham distinguished first the following forms of identity between subjects:

1. "Between any individual referred to by one term and any individual referred to by the other",
2. "Between any individuals referred to by one term and any one of a part only of the individuals referred to by the other", and
3. "Between any one of a part only of the individuals referred to by one and any one of a part only of the individuals referred to by the other".

For example, in the second case we have "the identity between men

and animals" [1827, 123].

This distinction, evidently motivated by the subtleries of classification, had as effect the division of each of the four standard forms of propositions in two, depending on whether the predicate is taken over the entire quantity or not. Thus, letting A, E, I, O stand for the four basic forms

	A	All	X	are	Y
	E	No	X	are	Y
(63.1)	I	Some	X	are	Y
	O	Some	X	are not	Y.

Bentham arrived at the new scheme

	(1)	$tX=pY$	(5)	$pX=pY$
(63.2)	(2)	$tX\ pY$	(6)	$pX\ pY$
	(3)	$tX=tY$	(7)	$pX=tY$
	(4)	$tX\ tY$	(8)	$pX\ tY$

The symbols "=", " $\|$ ", "t", and "p" stand respectively for identity, negation, "toto" meaning "all", and "parte", meaning "some". Thus, A of (63.1) is now split in (1), read as "X in toto = Y ex parte" - meaning "All X are some Y", and (3) or "X in toto=Yin toto" - meaning "All X are all Y" [1827, 133]. In other words, Bentham quantified the predicate by specifying whether it is taken as universal or particular each time that the subject is similarly quantified. This scheme of quantification, anticipated by Continental writers a century ago, was for the first time introduced in England. However, it was with Hamilton who made systematic use of this scheme fifteen years later, that the issue of the quantification of the predicate became prominent in English literature on logic<sup>(3)</sup>.

The neglect of Bentham's original work by his contemporaries probably was due to the fact that only sixty copies of his book were sold. Undoubtedly one copy was in the possession of Hamilton, who reviewed both Bentham's and Whately's work in 1833<sup>(4)</sup>. Hamilton started lecturing on the quantification of the predicate around 1839, but the issue gained importance only through Hamilton's dispute with De Morgan in 1846. Before we proceed to Hamilton, we will discuss the contributions of Solly, whose work in 1839 suffered the same neglect as Bentham's own and which,

despite its originality, is hardly known in our days<sup>(3)</sup>.

Born at Essex in 1816, T. Solly entered Caius College at Cambridge in 1836; but, being a unitarian, he left without a degree. Admitted <sup>later</sup> as a student at the Middle Temple, he was called to the bar in 1841. Two years later he migrated to Germany where he stayed up to his death in 1875. Solly lectured on English language and literature in the University of Berlin,

writing books on philosophy and English verse. Prior to his migration to Berlin, Solly wrote one book on logic, entitled A syllabus of logic, in which the views of Kent are generally adopted, and the laws of syllogism symbolically expressed [1839]. The originality of this book lay in Solly's distinct mathematical approach, probably due to a "friend" whom Solly did not name [1839, 26; see (7) below]. It seems that, like Bentham [1827], his book was of very limited circulation. His fear of passing unnoticed was expressed in a letter to De Morgan in 1847. Afraid that the latter might not have come across his book, "I devoted a good deal of time to this subject while still at Cambridge in 1839, and eventually published a little work which I think has very probably never come before you, as it enjoyed such a limited circulation". Solly enclosed <sup>an order for</sup> a copy, eager to hear De Morgan's views on it and the latter's own novelties in connection with the mathematization of the syllogism<sup>(4)</sup>.

In the "Preface" of his book, Solly praised Whately for his able and lucid discussion of material fallacies [1839, v], but criticised two major points of his work, <sup>thus</sup> displaying an ability for penetrating analysis. For Solly "Logic is not an organum of the sciences; for it does not contain a single reference to any branch of knowledge"; "it is however of the greatest use in testing the work of another organum, by exposing all its results". He further argued that we can not apply the term "Art" to logic "without an absurdity as manifest as if we were to speak of the Art of the Differential Calculus or Conic Sections" [1839, 10-11]. He was surprised with Whately's division of logic into "science" -as conversant about knowledge only- and to "art" -as concerned with the application of knowledge to practice- arguing that this arrangement "combines under the same name two branches of knowledge whose nature and origin are equally distinct from each

other" [1839, 11-12]. For, the first distinction applies to the *a priori* laws of the mind, whereas the second to empirical psychology [1839, 1.7, 11-12; see (11) below].

In the same spirit, Solly objected to Whately's statement that "Logic is entirely conversant about language" [see 6.2, (9)], holding that it contradicts Whately's own thesis that logic's office is the analysis of the process of the mind in reasoning [Solly 1839, 14; see 6.2]. Exactly as Boole was to argue in his Laws of thought [1854, 8.4, (2)], Solly claimed that "If language had not received its stamp from the mind (the laws of which we assume to be universally the same in all countries and age), we would never be certain that we might not at some future time meet with a people whose language required quite a new logic, and in that case the science would lose its *a priori*-character, and rest on probability alone" [1839, 15].

Solly treated formal logic on the grounds that in categorical propositions "the quantity of the term that is placed last in the general categorical form is entirely determined by the quantity of the copula". Distinguishing thus eight possible combinations between subject and predicate, on lines similar to Bentham's (63.2), but independently from him, Solly confined only to the four known forms (63.1) [1839, 47-48] attaching thus no importance to the issue of the quantification of the predicate.

The main innovation in Solly's book is his frequent drawing on analogy from mathematics. In so doing, Solly was almost as bold and original as Boole would be in going beyond simple algebraic analogies, discovering "a perfect analogy between abstract conceptions and enveloping surfaces or curves" [1839, 23-27] (?). He made use of functional and differential notation in a very peculiar way which, having had no impact on his contemporaries, is beyond the scope of our present discussion. However, before we switch to the fierce opponent of the use of mathematics in logic, Hamilton, we will display one example of Solly's mathematization of the laws of syllogism, the only instance of his individual approach in logic to see some dim light of public reference.

Solly held that

The conditioning laws of categorical syllogism admit of a very

simple analytical expression from which all its properties may be readily obtained. But the more especial object in treating this subject mathematically, is the exhibition of that symmetry, from which, the equality of the number of moods that are true in the first three figures, may be derived a priori to all consideration of the moods themselves<sup>(\*)</sup>.

In brief Solly's symbolical method is summarized in De Morgan's words in 1847 as follows:

The symbolical expression here given is of a peculiar character: the algebraic signs are adopted in a sense which preserves the rules of sign, while the symbols represent the terms of the syllogism, or else the notions of particular and universal. Thus, if p stand for particular, u for universal, and m for one of the terms of a syllogism,  $m=u$  or  $m=u=0$  implies that m is a universal term, and  $(m-u)(n-p)=0$  implies the alternative that either m is universal, or n is particular. By means of such alternative relations, the conditions of validity of the various figures are expressed<sup>(\*)</sup>.

This is the sole reference to the symbolical approach in Solly's book so far [see (6), (9) above]. Three years later Mansel criticised Solly for mathematizing logic<sup>(\*)</sup>. Venn and De Morgan were further to comment upon Solly's less important contribution on the issue of quantification. According to Venn, "neither Mr Bentham nor Mr Solly seem to me to have understood exactly the sense in which their scheme was to be interpreted, nor to have attached any importance to it" [1881, 9, fn1]. Ignorant of Bentham [1827] in 1850, De Morgan mentioned Solly briefly adding "But Sir William Hamilton is the first who published the idea of taking all phases of usual quantification, and making them the basis of a system of syllogism" [1850, 42].

Born in Glasgow in 1788, W. Hamilton graduated from the University of Edinburgh in 1807. Following in his father's footsteps - a doctor who occupied the chair of anatomy and botany at Glasgow University - Hamilton studied natural sciences and chemistry. But when studying later at Oxford University - at the same time as Whately [6.2] - he developed an interest in philosophy acquiring soon the reputation of the most learned



authority on Aristotle. He returned to Edinburgh where he became an advocate in 1813, but in 1821 he applied for the chair of moral philosophy. Elected finally in 1836, he became a professor of history, philosophy and logic at Edinburgh University, a post which he held until his death in 1856.

During the period 1821 and 1836 Hamilton contributed three articles on Cousin, perception and logic respectively, which extended his fame as philosopher and logician and only thus did he manage to be elected. A propos, V.Cousin, influential on French education, attacked Condillac and contributed in bringing common sense philosophy in France around 1810, admiring, like Hamilton, above all T.Reid. In 1833 Hamilton reviewed several books on logic, especially Whately's. He accused the latter for lack of familiarity with history of logic, flooding him with quotations from classical texts. As with Solly six years later, Hamilton was shrewd to perceive that Whately's statement on logic as conversant about language is contradictory with his proper definition of logic as related with the process of the mind in reasoning and he charged him as a supporter of psychologism<sup>(11)</sup>.

Hamilton was a fierce opponent of mathematics and believed that a comparison between logic and mathematics is not even possible because logic is a more extensive science than the latter [Laita 1979, 48-49]. But, for all his hostility to algebra, the effect of his system was to make logic move closer to algebra. Treating the problem of quantification of the predicate in the same way that Bentham did, Hamilton proposed the following eight quantified forms of statements

- |        |     |      |   |               |      |   |   |
|--------|-----|------|---|---------------|------|---|---|
|        | (1) | All  | X | is            | all  | Y |   |
|        | (2) | All  | X | is            | some | Y | A |
|        | (3) | Some | X | is            | all  | Y |   |
| (63.3) | (4) | Some | X | is            | some | Y | I |
|        | (5) | Any  | X | is - not any  | Y    |   | E |
|        | (6) | Any  | X | is - not some | Y    |   |   |
|        | (7) | Some | X | is - not any  | Y    |   | O |
|        | (8) | Some | X | is - not some | Y    |   |   |

Whereas in traditional logic proposition A, or "All X is [are] Y", can be read extensively (the extension of the term X is contained within the extension of the term Y) or comprehensively

(i.e. the other way round), the new scheme, by distinguishing (1) from (2), and so on, enables a different analysis of simple categorical propositions. According to this analysis, simple propositions become identity claims about classes. For example, (7) asserts that there is a subset of the class A which is not identical with any subset of the class B. Hamilton did point out the advantages of his doctrine which, beside reducing propositions to equations, had as a result the simplification of the doctrine of conversion, the rejection of the figures and the manifestation of the absurdity of reducing syllogisms of other figures to the first<sup>(12)</sup>.

Hamilton was very proud to have extended Aristotle's system to one which apparently was both symmetrical and complete. It comes as a surprise though, that he admitted that "a proposition being always an equation of its subject and its predicate", and that he further associated his equational approach with the ideal of a "logical calculus", in the form of "a scheme of logical notation... showing out in their old and new applications the propositional and syllogistical forms, with even a mechanical simplicity" (Passmore 1968,121). While by "equations" Hamilton ~~did not intend~~ mathematical ones but rather linear formulae devoid of mathematical symbols, (though, his notation for assertion and denial according to De Morgan resembled the symbols - and +) it was exactly his analysis of simple categorical propositions which led Boole in formulating the algebra of logic<sup>(13)</sup>.

Hamilton's system had indeed its merits but also its defects which he fiercely denied. De Morgan would show in 1850 that propositions (1) and (8) of (63.3) are problematic [6.6]. Moreover, as Venn claimed in [1881,4-14], only five out of the eight propositions do correspond to mutual inclusion and exclusion of classes. When De Morgan, independently of Hamilton, introduced his own scheme of eight propositions -based on the concept of the "contrary" (negative or opposite) of a term and the "numerically definite syllogism" late in 1846- Hamilton became furious charging De Morgan with plagiarism and claiming priority over the issue of the quantification of the predicate. The notorious dispute soon broke out which, beside making known to Boole Hamilton's scheme, challenged De Morgan to amend Hamilton's

syllogism system and also to improve upon his own work. The early stage of the controversy in 1847 is hardly of any significance<sup>14</sup>; however we will see during our discussion of De Morgan [1850; 6.6] that the controversy was based upon misconceptions as the two logicians worked on utterly different lines.

The quantification of the predicate soon proved to be overrated as a logical instrument and was hardly applied after Hamilton's death. However, it did prove useful in the shaping of algebraic logic at mid-19th-century England. As Laita [1979, 60] claimed, perhaps without Hamilton we might not had had Boole. The same assertion can not apply to De Morgan's case, since the germs of his mature work on the logic of relations were found in his work published before the dispute [6.4-6.5]. However, both Hamilton's and Mansel's attacks [see (10) above] served a little more than a mere challenge for De Morgan to delve deeper into the connections between logic and mathematics and to extend his work as shaped up to 1847. Having set the preliminaries on the state of English logic in the first decades of the 19th century, we now switch to De Morgan's work as gradually shaped between 1831 and 1860.

#### 6.4 An outline of De Morgan's logic: 1831-1860.

The core of De Morgan's work on logic consists of his book Formal logic [1847b] and four papers "On the syllogism" published in the Transactions of the Cambridge Philosophical Society as [1847b, 1850, 1858 and 1860a]. For convenience we cite them as FL, S<sub>1</sub>, S<sub>2</sub>, S<sub>3</sub> and S<sub>4</sub> respectively. We will also cite his book Syllabus of a proposed system of logic [1860b] as S, his English Cyclopaedia article on "logic" as [1860c] and his last memoir on the syllogism as [1863] or S<sub>5</sub>. The Syllabus and the encyclopedia article both form a substantial survey of the author's views on logic in a rather congested form. As the material of FL and the first memoirs is ill-arranged, S is often regarded as a best entry to De Morgan's work. However, S totally lacks the charm of the first three memoirs and will be used here mainly for confirmation of De Morgan's basic claims as published prior to 1860<sup>15</sup>.

Nowdays De Morgan's fourth memoir "On the logic of rela-

tions" [1860a] is regarded as his most original contribution in logic<sup>(2)</sup>. De Morgan's logic of relations was left in an unfinished state. The material of S<sub>4</sub>, presented in a far from lucid way, was the result of enquiries which matured gradually in the course of almost thirty years. The scope of our study is to discuss the evolution of the logic of relations from the point of view of De Morgan's mathematical work, more particularly of his article on the calculus of functions [1836]. Before entering into a detailed study of his work we present the reader with a general outline of his main contributions on logic up to 1860.

Up till 1839 De Morgan's work exemplified Whately's view that logic is a science concerned with the validity and invalidity of arguments. In 1847 he introduced new concepts and types of propositions, formulating by 1850 a system which he called "an extension of Aristotle" on the grounds that "Every one of my syllogisms can be reduced to an Aristotelian form" [S<sub>2</sub>, 42]. Ultimately syllogism was considered "under the aspect of combination of relations" [S<sub>4</sub>, 241] and thus traditional syllogistic was widely and originally extended. As he wrote in the opening of [S<sub>4</sub>, 208] "In my second and third papers on logic [...] I insisted on the ordinary syllogism being one case, and one case only, of the composition of relations. In this fourth paper I enter further on the subject of relation, as a branch of logic".

Aristotelian syllogistic is a permanent point of departure throughout De Morgan's writings, but we can only partly characterize his work as traditional logic. As with his work on mathematics [3.4-3.9], De Morgan believed in a linear historical evolution of science and drew on traditional issues, such as those advocated by the Port-Royal logic, well after 1860 [6.2, (5); S<sub>5</sub>, 287-94]. But while looking back to the past [3.4, (7); 3.5; 3.6, (1)], he was fully aware of the inadequacy of the logic which prevailed with Whately and Hamilton and glimpsed ahead to a "mathematical logic"; the latter would be a result of the collaboration between mathematicians and logicians -whose mutual antipathy he bitterly criticized- in some future time [S<sub>5</sub>, 78, fn; S<sub>5</sub>, 184, fn].

What were De Morgan's own contributions towards this vaguely described ideal state of "mathematical logic"? The first hint to-

wards engraving a new direction is found in [S<sub>1</sub>, 1] where he stated "I here venture to propose a derivation and classification of the forms of the syllogism, differing very widely from that in use". Applying the mathematical process of gradual abstraction and generalization [3.5-3.6], and clothing traditional logic in a minimal symbolic veil, he headed towards a "symbolic logic" [S<sub>2</sub>, 26] holding that "symbols will one day be the scaffolding of logical education, though useless then, as now, to all who have not mastered them" [S<sub>3</sub>, 86].

Much more conservative and less original than Boole was to be in his application of symbolic procedures in logic, as he admitted in (1860c, 255). De Morgan was nevertheless accused for corrupting logic with mathematics. The very title of his book, Formal logic: or, the calculus of inference, necessary and probable (1847b) annoyed both Mansel and Smart, and it is of interest to display the partly similar views of these two logicians whose beliefs in general were philosophically worlds apart. H. Mansel, professor of metaphysics at Oxford and closely identified with Hamilton, reviewed FL in his paper "Recent extensions of Formal logic" [1851] comparing De Morgan (1847b) with two books by followers of Hamilton<sup>(3)</sup>. In this review the title of FL was linked with Condillac's, and Hobbes's, dictum "calculer c'est raisonner, et raisonner c'est calculer" (Mansel 1851, 93; 1.8).

On the other hand, in Smart's essay A letter to Dr. Whately [1851]<sup>(4)</sup>, the title of FL indicated a methodology which "can only do harm". Following Locke's epistemology, Smart accused De Morgan for being "a more through-going Aristotelian than Dr. Whately" [1851, 24, fn]. FL gave rise to many controversial reviews and remarks [S, 148-9]; none, however, must have entertained De Morgan as much as Smart's little note, for De Morgan wrote at the bottom page of his own copy: "I shall be more Aristotelian than Aristotle himself one day" (Smart 1851, 24, fn). Further on the title page under Smart's printed identification as "a follower of Locke" [see (4) above], he added: "but without the key. He is Mr. Smart, the author of curious works on logic. Rec<sup>d</sup> May 12, 1852".

These brief accusations fully describe the horror with which defenders of either tradition, Aristotle or Locke, confronted De

Morgan's attempt to update, elaborate and extend Aristotelian syllogistic by enriching it with a minimum of mathematical methodological issues and symbols. In fact, far more critical than Smart, Mansel held that Solly, Boole and De Morgan were all guilty of regarding logic a species of algebra [see 6.3.(10)].

Nevertheless FL was warmly welcomed by intellectuals who needed an escape from vague empirical and common-sense theories. In 1848 De Morgan's former teacher J.T. Higman wrote to him soon after receiving FL:

It is a subject with which I am very superficially acquainted, and in fact for many years under the influence of the writings of Lock, Campbell, Reid and Stewart held dialectics somewhat in contempt. That I was mistaken in the notions I had formed on this subject was made evident to me from reading Whately's book, on the subject, and since then such a reaction has taken place in my mind that I am now inclined to banish metaphysical inquiries [...] from the examinations for Fellowship at Trinity (subjects by the way on which in former years I wasted much valuable time) and to substitute a common system of Logic in their place. From some remarks of yours in some of the notes to Formal Logic I am inclined to think that your estimate of the value of metaphysical speculations does not differ materially from my own<sup>(3)</sup>.

Let us now record his principal innovations, pointing out those which ultimately gave rise to his logic of relations in 1860. His first motivation to draw on Aristotle was purely educational. Recommending Whately [1826] for a study of traditional logic to students of mathematics, De Morgan provided in [1831, 71-75] a semi-syllogistic proof of Pythagoras's theorem distinguishing between "is" and "is equal to" as two different copulas. Eight years later he presented a more systematic account of traditional logic in a pamphlet First notions of logic [1839] for the students of geometry in which we see a first hint towards composition of relations. After another gap of eight years De Morgan published his first memoir "On the structure of the syllogism" [1847a], followed few months later by FL; [1839] and [1847a] formed parts of FL.

In S<sub>1</sub> he introduced the concept of "universe" and of

"contrary" terms. Assume as universe the space of a square, and divide its points in Xs and not-Xs; then "the not-X is no more an aorist term than the X" [S<sub>1</sub>, 2]. Contrary terms gave rise to a system of eight basic propositions instead of the traditional four (63.1). He also suggested a symbolic language for the syllogisms derived by means of this system, proposing also the "numerically definite syllogism" where "some" can be replaced by a concrete quantifier, thus diverging from classical syllogism.

De Morgan dealt for the first time in FL with "compound" names, "complex" propositions, and proposed the substitution of the "numerical theory of probability" for the "old doctrine of models". These aspects are not discussed in our study, so we only note at them briefly. Denoting by "X,Y" the "aggregate" of X and Y, and by "XY" their "compound", he formulated what we now call as "De Morgan's law" in "The contrary of an aggregate is the compound of the contraries of the aggregant" [FL, 116; S, 182]. While the theory of compound names reminds of an algebra of classes, his modality theory involved, via probability, techniques of generalized arithmetic<sup>(6)</sup>.

Another novelty was the introduction of relational inferences which illustrate the inadequacy of the Aristotelian system which alone can not validate them. Take for example a case of what was called as "oblique inference" [FL, 114]:

Every man is an animal

(64.1) -----

Therefore the head of a man is the head of an animal<sup>(7)</sup>. But the most important issue established in FL was the "doctrine of the abstract copula" [see (7) above], a generalization of the vaguely stated double copula in [1831]. In chapter 3 of FL the copula was formally characterized by the three properties of "is": "transitivity", "convertibility" and "contrariety" [the actual terms named so in S<sub>2</sub>;6.5]. Any copule with these properties could now substitute "is" without altering the form of the syllogism.

All these innovations as published in 1847 caused a stir among loyal defenders of Aristotelian logic. S<sub>1</sub> gave rise to the dispute with Hamilton over priority on the quantification of the predicate [6.3,(14)], and both S<sub>2</sub> and FL were bitterly criticized

by Mansel [1851; see 6.3.(10)]. Under the challenge of his controversy with Hamilton, De Morgan modified and elaborated in  $S_2$  the system introduced in  $S_1$  as based on the concept of contrary terms, putting forward his "Arithmetical system" as an extension of Aristotle's [ $S_2$ , 42]. The theory of the abstract copula was also further developed in  $S_2$  and connected with the doctrine of figure. Eventually De Morgan's arithmetical system was transformed to one in which the copula "is" was extended in its "widest sense" by being replaced by "has relation to", the relation "distinctly conceived as transitive" [ $S_2$ , 61-3].

It took De Morgan eight years to complete his third memoir, partly devoted to Mansel's critique. He further applied the procedure of abstraction and generalization, reaching the stage of "nearly" formulating "the purely formal judgment" [ $S_3$ , 80]. He delved into his theory of compound terms - introduced in FL- and applying philosophical concepts, such as the duals "extension"- "intension", or "objective" and "subjective", he distinguished between various "sides of logic" revealing his tendency for classification and a rather unsatisfactory philosophical systematization.

The task to answer to Hamilton's and Mansel's criticisms frustrated De Morgan through the 1850's causing a retardation to the publication of  $S_4$  where explicit aspects from the theory of relations are instituted for the first time. The presentation of De Morgan's material becomes much more messy and unclear as his theory reaches its highest points. Theorems are demonstrated in  $S_4$  in an unrigorous way, and a new complicated notation is put forward.

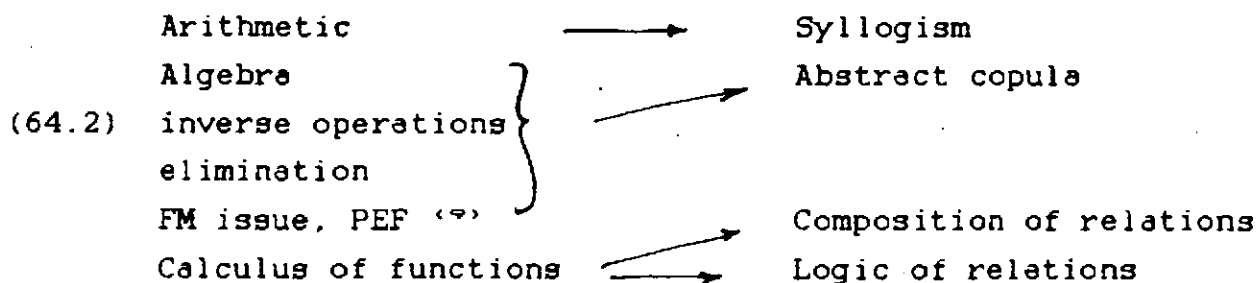
$S_4$  saw the materialization of the most important concerns of  $S_2$  and  $S_3$ . What prevailed within De Morgan's theory on the syllogism by 1860 was no more the sketches introduced in  $S_1$  and FL; as he was to claim, FL was superseded by the later two memoirs and this fact was due largely to the challenge offered by Hamilton whom he acknowledged in [ $S_3$ , 138;  $S_4$ , 147-150]. However, we have no clues whatsoever on either the real reasons why De Morgan delayed his theory of logical relations nor how exactly he came to compose his fourth memoir. He offered to us only one very characteristic example in connection with his pamphlet [1839]



which reveals the slow and peculiar evolution of his reasoning. In 1858 he wrote:

19 years ago I wrote my First Notions of Logic, intended, as the preface states, ultimately to become an appendix to my Arithmetic. I had not then any glimpse, so far as my memory serves, of the numerical syllogism: and I doubt if I could have given any very distinct account of my reasoning for appending the common syllogism to a book of numbers. But it may be that my now confirmed notion of the usual form of syllogism being arithmetical was germinating<sup>(a)</sup>.

The implicit or explicit links and analogies between logic and mathematics to be noticed in the course of our study are provided in brief in the following sketch:



This rough scheme will be illustrated in detail in our study to follow in five stages.

6.5: 1831-1850; the passage from the double copula to the abstract copula; first instances of composition of relations.

6.6: 1847-1850; the arithmetical syllogistic system; and the amendment of Hamilton's system.

6.7: 1850-1858; the drawing from algebra and the calculus of functions in De Morgan's  $S_2$  and  $S_3$ ; the pure form of judgment.

6.8: 1860; the logic of relations -main concepts and theorems.

6.9: 1836-1860; a comparison between De Morgan's calculus of functions and his logic of relations.

## 6.5 The doctrine of the abstract copula and composition of relations in De Morgan's logic: 1831-1850.

As we mentioned in [3.4.6.4], De Morgan's first account of traditional syllogistic is traced in his book On the study and difficulty of mathematics [1831], as chapter XIV on "Geometrical Reasoning". He believed that mathematical demonstration was unique for its ability to afford certainty and accuracy in reasoning. Moreover, geometry -based on self-evident first principles and involving more concrete and lucid concepts and symbols than algebra- offered the grounds for a paradigmatic illustration of the art of reasoning [1831, 2, 65; 3.4].

De Morgan first divided every assertion in three distinct parts, the "subject", the "copula", and the "predicate". For example in

(65.1) "All right angles are equal",

he regarded as the subject to be "right angles", as the predicate "equal angles" and the copula, "or manner in which the two [subject and predicate] are joined together, which is generally the verb is, or is equal to, and can always be reduced to one or the other: in this case the copula is affirmative" [1831, 68] <sup>(1)</sup>.

The four standard forms of categorical propositions (63.1) were introduced by the letters A, E, I and O (on their origin see S, 151) and examples were provided to illustrate the four figures and the nineteen legitimate moods of traditional syllogism. The syllogism

A	All the	○	is	in	the	△
(65.2) A	All the	□	is	in	the	○

---

A All the □ is in the △ ,  
 where ○, △, □ stand for circle, triangle and square respectively, is a case of the first mood of the first figure AAA [1831, 72].

He next defined "inductive reasoning" as "that in which a universal proposition is proved by proving separately <sup>for</sup> a fortiori" reasoning with the example

every one of its particular cases, illustrating next

A is greater than B.  
 (65.3) B is greater than C

-----  
a fortiori A is greater than C.

He mentioned that (65.3) can also be stated as follows

The whole of B is contained in A  
 (65.4) The whole of C is contained in B

-----  
 Therefore C is contained in A ,  
 which is the conversion of the former syllogism. Observing, however, that the premises of (65.4) "do not necessarily imply" as much as those of (65.3) he wrote "the complete reduction we leave to the student" [1831, 73].

De Morgan's last statement is typical of his intuitive rather than rigorous attitude. As we saw in [3.4-3.8] he hardly did ever provide a complete demonstration of a theorem in his [1836]; always something extra is left for the reader to assume or accomplish. A striking example of this attitude is his unfinished demonstration of Pythagoras's theorem in syllogistic form [1831, 73-74]. His proof was presented "as a specimen of a geometrical proposition reduced nearly to a syllogistic form"; to avoid "multiplying petty syllogisms", he added, "we have omitted some few which the student can easily supply" [1831, 73].

With very few exceptions, Euclidean geometry and Aristotelian logic had remained two independent disciplines for over twenty centuries. De Morgan was the only English author to cast a geometrical theorem in syllogistic form. In fact, that was his first and last attempt in this direction and, what is of most importance, an incomplete one. The incompleteness of demonstration was due to the insufficiency of the syllogism to reduce to syllogistic form those inferences which require the transitivity of equality. Reid had denied that

A is equal to B  
 (65.5) A is equal to C

-----  
 A is equal to C  
 could be reduced to syllogistic form, while Hamilton and Mansel admitted such a reduction on the grounds that a premise were ad-

ded which asserted the transitivity of equality<sup>(2)</sup>.

De Morgan must have been ignorant at that early stage of the intricacies involved in inferences such as (65.5). The main defect of his account is his dual use of equality: in (65.1) equality was used as a predicate, whereas in

(65.6) "BG is equal to BC"

as a relation [1831, 74]. In any case, he felt free to use alternately two copules, "is", as in (65.1), and "is equal to", as in (65.6), thus introducing implicitly the innovation of the "double copula" [see (1) above], generalized in 1847 in the so-called "doctrine of the abstract copula" [6.4, (7); text below].

Concluding the chapter on "Geometrical Reasoning", De Morgan revealed some vague hints on relational syllogism [1831, 76]:

In all that has gone before we may perceive that the validity of an argument depends upon two distinct considerations, -1, the truth of the relations assumed, or presented to have been proved before; -2, the manner in which these facts are combined so as to produce new relations; in which last the reasoning properly consists.

It is quite surprising though, that hardly any reference to geometrical demonstration was to be incorporated in his pamphlet First notions of logic [1839] despite its intention to serve particularly students of geometry [6.4 text and (8)]. The core of this pamphlet was a systematic introduction to traditional logic, a sort of extension of that already provided in his [1831]. The only issue worth noting is that of the a fortiori reasoning, now presented in the form

All the As make up part (and part only) of the Xs  
(65.7) All the Xs make up part (and part only) of the Bs

---

Therefore: All the As make up part of part (only) of the Bs, adding "and the words in italics mark that quality of the conclusion from which the argument is called a fortiori" [1839, 26]. We might regard "part of part" to be a first instance of composition of relations, a concept pursued further in 1850<sup>(3)</sup>.

First notions [1839] formed the first chapter of FL, in which book the issue of the "abstract copula" appeared independent of De Morgan's syllogistic. In chapter III of FL "On the ab-

abstract form of the proposition", he claimed: "In order properly to examine the laws of inference, or of anything else, we must first endeavour to arrive at a distinct abstraction of so much of the idea we are concerned with, as is itself the precedent reason, if it be right so to speak, of the law in question" (FL, 47]. By that time De Morgan had extensively dealt with the issue of both abstraction and classification in his study of the calculus of functions and the foundations of algebra. The same epistemological approach becomes evident when he distinguishes between "external objects", "names" and "ideas" in an attempt to find the changeable and invariable properties of the copula which relates concepts of these categories.

For example, let "man" and "animal" belong to the category of "external objects". Then, if "is is an is of identity" indeed "Every man is one of the animals". But, under the category of "names" the "name man is not, as a name, the name animal" and the same holds if these two concepts are regarded as "ideas". But if we take "is" as an "is of applicability" or "of possession of all essential characteristics", then "man is a name to be applied, to that same [...] animal" and "man is an idea which possesses [...] all that is constitutive of the idea animal" respectively (FL, 49-50).

We have thus distinguished three senses of the copula "is" in analogy with the three senses of regarding the concepts "man" and "animal". "Now we must ask", suggested De Morgan, "what common property is possessed by each of these three notions of is, on which the common laws of inference depend. Common laws of inference there certainly are". In reply to his question he displayed the following characteristics of the copula "is":

1. The first characteristic of "is" is that it is indifferent to conversion: "the "A is B", and the "B is A" must have the same meaning, and be both true or both false".
2. "Secondly, the connexion is, existing between one term and each of two others, must therefore exist between those two others; so "A is B" and "A is C" must give "B is C".
3. "Thirdly, the essential distinction of the term is not is merely that is and is not are contradictory alternatives, one must, both cannot, be true" (FL, 50).

Based upon this analysis, De Morgan stated his doctrine of the abstract copula as follows, justifying it by appealing to algebra:

Every connexion which can be invented and signified by the terms is and is not, so as to satisfy these three conditions, makes all the rules of logic true. No doubt absolute identity was the suggesting connexion from which all the others arose: just as arithmetic was the medium in which the forms and laws of algebra were suggested. But, as now we invent algebras by abstracting the forms and laws of operation, and fitting new meanings to them, so we have power to invent new meanings for all the forms of inference, in every way in which we have power to make meanings of is and is not which satisfy the above conditions<sup>(4)</sup>.

On these grounds he claimed that the copula "is" in the syllogism

Every X is Y  
(65.8) Some Zs are not Ys  
-----

Some Zs are not Xs  
could be replaced by the material copula "is tied to". Hence,  
(65.8) "Is true in the sense":

Every X is tied to a Y  
(65.9) Some Zs are not tied to Ys  
-----

Some Zs are not tied to Xs  
an instance "considered as a material representation of attachment together of ideas in the mind" (FL, 51]. Not all cases of inference, observed De Morgan, demand of the three properties above listed to be satisfied. For the most common case AAA, (65.2), only the second property is needed to secure its validity. The same holds true for the a fortiori inference (65.3) [FL, 51-2; (3) above].

We might thus say that the formal inferences such as (65.8) admit of interpretation -as in (65.9)- by assigning to "is" specific meanings exactly as the abstract forms of the calculus of function become specified when arbitrary functional symbols are assigned a value. In fact, the passage quoted in (4) above

reveals De Morgan's drawing on analogy from the procedure of gradual generalization through abstraction, which we have called in [(35.1): 3.6, (3)] the form-matter distinction, <sup>and</sup> which he had applied in [1836]. This instance is further stressed in [S<sub>2</sub>, 50] when he recalled that in FL he had "separated the essential from the accidental characteristics of the copula", following "the hint given by algebra". There by he showed the conditions so that a copula "substituted for the ordinary one, shall leave all the forms and conditions of inference unaltered".

De Morgan made in S<sub>2</sub> some more essential steps in rendering the copula abstract, connecting it consequently with the first explicit instances of composition of relations. First he defined an "abstract copula" by "a formal mode of joining two terms which carries no meaning, and obeys no law except such as is barely necessary to make the forms of inference follow". He next denoted an abstract copular symbol by a bar "—" and its negation by "—", listing the three properties given in FL in a different order of presentation and in a more formal way"

- 1' Transitivity:  $X-Y-Z=X-Z$
- 2' Convertibility:  $X-Y=Y-Z$
- 3' Contrariety: either  $X-Y$  or  $X- -Y$  is true [S<sub>2</sub>, 51].

He consequently provided numerous copulas, such as "rules", "joins", "is superior to", "gives", "support" or "is cousin to" in an attempt to link the three properties with the doctrine of figure. He first observed that all these copulas, except the last, satisfy property 1', while few only, such as "joins" satisfy both 1' and 2', the last one satisfying only the 3'. Choosing then the copula "gives" -in the sense of "produces"- as a representative transitive copula he found that the first three figures hold true despite the incorvertibility of the copula, but not the fourth figure. De Morgan was thus led to the conclusion that the most essential property is "transitivity" calling it hence "the condition of permanence of the copula" [S<sub>2</sub>, 53]. In a sense this characterization reminds us of the analogous "principle of permanence of equivalent forms" -cited as PEF- in algebra and his constant attempt to apply it and justify it<sup>(3)</sup>.

He proceeded to display the syllogism.

"all the piles supported arches;  
 (65.10) all the piles were supported on gravel,  
 therefore gravel did then support arches"  
 as a "good syllogism", "not capable of reduction to an Aristotelian syllogism". Necessary to the inference is the dictum "support of support is support". He then claimed [S<sub>2</sub>, 54-5]:

It may be said that this is more than an Aristotelian syllogism; I maintain it to be less, if either. The outstanding copular relation (always implied) of an Aristotelian syllogism is "is that which is gives is"; of the preceding case, "support of support is support". The former demands transitive and convertible meaning for is, or that is shall be its own correlative: the latter demands transitive meaning for support, and the allowance of its correlative.

A still further enlargement can be effected by means of relations independently of transitivity. Take, for instance, the inference

Every X has a relation to some Y  
 (65.11) Every Y has a relation to some Z  
 -----

Every X has a compound relation to some Z.

As an illustration of (65.11) he presented the "bicopular syllogism" in which "the intransitiveness of the individual copulae is supplied by the invention of a compound copula for the conclusion":

John can persuade Thomas  
 (65.12) Thomas can command Willian  
 -----

John can control Willian

In this case the postulate demanded for this inference is "Control includes the influence exerted over the governed by one who can persuade the governor" [S<sub>2</sub>, 55-56] '4'.

How far the new operation of the composition of relations can be handled by the traditional logic? De Morgan constructs a sorites, which we omit, in an attempt to reduce the bicopular syllogism (65.12) to one in only one copula, "is". So doing, based on the above mentioned postulate, he claimed, in an urge to generalize, that we can learn more about unicopular syllogism if



we consider it as a case of the bicopular.

...just as a beginner in algebra sees properties of  $axb$  which would be much clouded by taking his initial examples from  $axa$ . Indeed, the forms of logic, stunted down to the Aristotelian syllogism, much resemble those of an algebra in which all the letters are equal, and all equal to unity<sup>(7)</sup>.

Denoting the copula by a bar "—" and the syllogism (65.11)

by

$$\begin{array}{l} \text{A} \quad \text{X} \text{ --- } \text{Y} \\ (65.13) \quad \text{A} \quad \text{Y} \dots \text{Z} \\ \hline \text{A} \quad \text{X} \dots \text{Z} \end{array}$$

where "..." denotes a relation other than "—", De Morgan did effect a considerable step towards the abstraction of the copula as introduced in FL [S<sub>2</sub>, 57]. This step was mainly due to his influence from mathematics: in the calculus often symbols stand not for "magnitudes" but for "directions how to operate" [S<sub>2</sub>, 50]. In a similar manner, (65.13) provides a model for logical procedures in a formal way devoid of material elements. Nevertheless, the specific examples which De Morgan used to illustrate his theory of the abstract copula relied heavily on material elements and he admitted this when he stated "Historically speaking, the copula has been material to this day" [S<sub>2</sub>, 68].

Both Mansel and Hamilton would attack De Morgan's theory, challenging a further attempt towards the formalization of the copula in 1858 [6.7]. The most interesting aspect of De Morgan's analysis, however, is his first explicit extension of the copula "is" to "has relation to" within his syllogistic scheme studied in 6.6. By 1860 he would regard traditional logic as a special case of his calculus of relations effecting the extension implied in (7) above. The intricacies involved in reducing bicopular syllogisms to unicopular ones were only one of the reasons why the logic of relations delayed for ten years. However, by 1850 De Morgan had been convinced that the direction engraved in his logical studies was both original and valuable;

The admission of relation in general, and of composition of relation, tends to throw light upon the difference between the invented

syllogism of the logicians and the natural syllogism of the external world<sup>(\*)</sup>.

#### 6.6 De Morgan's arithmetical system: 1847-1850; a comparison with Hamilton's system.

De Morgan claimed in his first memoir "On the structure of the syllogism" [1847a] that;

...the general impression among writers seems to be that there cannot exist any other theory of the syllogism except that derived from Aristotle. If another can be produced, which is but self-consistent, true, and comprehensive, the tacit assertion of all writers is overthrown, whether that system be or not judged superior to the one handed down.

I here venture to propose a derivation and classification of the forms of the syllogism, differing very widely from that in use<sup>(1)</sup>.

Thus he opened S<sub>1</sub> in November 1846, introducing next the innovations which formed the basis of his syllogistic theory. In so doing he was not yet aware of Hamilton's "theory of the syllogism differing in detail and extent from that of Aristotle" [S<sub>1</sub>,17]. He admitted this in the "Addition" appended to S<sub>1</sub> three months later, saying "I should suppose it will be found that I have been more or less anticipated in the view just alluded to" [S<sub>1</sub>,17].

Indeed, De Morgan's extension of Aristotle's system partly overlapped with Hamilton's own (63.3). Hamilton realized this and, not trusting De Morgan's claims of ignorance, charged him with plagiarism in March 1847. A fierce dispute broke off soon with which De Morgan was occupied until 1863, that is well after Hamilton's death in 1856. Unaffected at first by Hamilton's charges, De Morgan incorporated S<sub>1</sub> in FL, developing further his syllogistic theory and appending a detailed account of the controversy at the end of his book [FL.297-323;6.3,(14)]. While the publicity of the dispute in 1847 is beyond the scope of our study, De Morgan's reply to Hamilton three years later reveals some interesting aspects of the evolution of De Morgan's ideas and shows that the controversy was based on a misunderstanding

on both sides. Of most importance though, is the fact that under Hamilton's challenge, De Morgan undertook to amend the former's defective system, thus leading to a further development of his own system as introduced in  $S_1$ . De Morgan's detailed treatment of Hamilton's syllogistic was incorporated in his second memoir "On the symbols of logic, the theory of the syllogism, and in particular of the copula" [1850], partly discussed in 6.5 in connection with the doctrine of the abstract copula.

This section picks up De Morgan's principal syllogistic innovations and hints at Hamilton's subtle role in the crucial passage from  $S_1$  to  $S_2$ . We will touch upon De Morgan's clarification of the dispute, his comparison of his system with Hamilton's own and point out the beneficial outcome of the amendement of the latter's system in the development of De Morgan's syllogistic theory. Among the issues to be raised is the algebraic origin of both De Morgan's system and notation, as well as his sound conception of consistency and extension, two notions with which a mathematician is more familiar than a logician. We start our account with the numerical syllogism which was not only conceived prior to any other novelty introduced in  $S_1$ , but also formed the cause par excellence of the controversy with Hamilton.

Let us assume syllogistic inferences in which  $Y$  is the middle term in the premises. "Aristotle", wrote De Morgan, "noticed but one way of being sure that the same  $Y$ s are spoken of in both premises: namely, by speaking of all of them in one at least" [ $S_1$ , 8-9]. As a result the middle term has to be distributed, that is to be taken universally at least once. De Morgan diverged from this principle claiming that new forms of inference should be considered, such as [ $S_1$ , 9] :

Most of the  $Y$ s are  $X$ s  
 (66.1) Most of the  $Y$ s are  $Z$ s

-----  
 Some  $X$ s are  $Z$ s

This deviation from Aristotelian syllogism had as an outcome the admission of the "numerically definite syllogism" which was introduced in  $S_1$  and fully expended in FL. Let " $m XY$ " stand for " $m X$ s are  $Y$ s" and  $s$  be the number of  $Y$ s given in advance; we hence have

m Xs are Ys  
 (66.2) n Ys are Zs

-----  
 (m+n-s) Xs are Zs

written also in the condensed form

(66.3)  $mXY + nYZ = (m+n-s)XZ$ .

If  $m=18$ ,  $n=15$  and  $s=21$ , we obtain from (66.2)

18 out of 21 Ys are Xs  
 (66.4) 15 out of 21 Ys are Zs

-----  
 12 Zs are Xs

[S<sub>1</sub>,17-19;FL,142-5;S,170-1].

These non-Aristotelian syllogisms are perfectly valid under restrictions, that is when the quantities of the middle term in the two premises together exceed the whole quantity of that term. Or, (66.3) is a valid inference under the condition

(66.5)  $m+n > s$ .

Called by Hamilton "the ultra-total quantification of the middle term", the principle running the numerically definite syllogism was first thought by Lambert. Unaware of this fact, De Morgan reconceived it and extended its use in S<sub>1</sub>, leading Hamilton to conclude that De Morgan took it from Lambert, believing at first that De Morgan's numerical system "was derived from information furnished by him [Hamilton]" [S<sub>2</sub>,49,fn 1]. Hamilton's charges were invalid<sup>(2)</sup>; but before we proceed to a discussion of the controversy which thus began, let us see the immediate outcomes of De Morgan's numerical quantification in S<sub>1</sub> and introduce his arithmetical syllogistic system drawing additionally on the improved symbolic notation given in S<sub>2</sub>.

A proper study of numerical propositions evidently demands the concept of "universe": "By the universe (of a proposition) is meant the collection of all objects which are contemplated as objects about which assertion or denial take place" [S<sub>1</sub>,2;S,156]. Given this notion and a term X we obtain the "contrary" of X, denoted by not-X or x, as "every object which has not the name X". Not distinguishing between "contrary" and "contradictory" terms or propositions, De Morgan claimed that the introduction of not-X destroys the distinction between assertion and denial [S<sub>2</sub>,

37;S.156-7]. In [1860c,260] he clarified that "no term used fills the universe". The consideration of admitting the limiting case when a term of a proposition coincides with the universe of discourse was to be among his concerns around 1862<sup>3</sup>.

His syllogistic system (66.6) -consisting of eight standard forms- was an immediate outcome of the consideration of the traditional four propositions (63.1) under the light of contrary terms. Each of A, E, I and O now produce a, e, i and o respectively by replacing X by not-X and Y by not-Y. We thus have four "universal" propositions, A, a, E, e, meaning that the whole universe must be examined to verify them, and accordingly four "particular" propositions. The manner in which the subject is spoken of is expressed; the predicate is universal [U] in negatives but particular [P] in affirmatives [S<sub>1</sub>.5-6;S<sub>2</sub>.35-36]. Following De Morgan's condensed notation for these eight forms -always to be read with reference to the order XY- we have due to the quantification by introduction of contraries the following system

- |        |     |     |       |       |    |       |    |                  |
|--------|-----|-----|-------|-------|----|-------|----|------------------|
|        | (1) | U A | Every | X     | is | Y     | UP | ))               |
|        | (2) | U a | Every | not-X | is | not-Y | PU | ((               |
|        | (3) | U E | Every | X     | is | not-Y | UU | ).(              |
| (66.6) | (4) | U e | Every | not-X | is | Y     | PP | (.)              |
|        | (5) | P I | Some  | X     | is | Y     | PP | ( )              |
|        | (6) | P i | Some  | not-X | is | not-Y | UU | ))               |
|        | (7) | P O | Some  | X     | is | not-Y | PU | ).(              |
|        | (8) | P o | Some  | not-X | is | Y     | UP | )). <sup>4</sup> |

A mere glance at this table reveals the logic of De Morgan's notation; the inclosing bracket, e.g. in A X))Y, X) or (X, means that the term is taken universally, whereas the excluding bracket, )Y or Y(, that it is taken particularly. A dot denotes negation, while an even number of dots -as in i )..(-) amounts to affirmation and to an absence of '. '. To use the contrary of a term we need to alter the curvature of its parenthesis, and annex or withdraw a negative point. Thus A can be read indifferently as X))Y=X).(y=x((y=x(.))Y. Finally, due to the perfect symmetry of notation we can immediately spot the contradictory pairs in (66.6), i.e. )) and (.(; ).( and ( ); (. and ))(; (( and ).) [S<sub>2</sub>.31,36-7].

The system (66.6) forming the basis of De Morgan's syllogistic was called by him in 1858 "arithmetical" for reasons discussed in text and [(6),(8)] below. Invented in 1847 [S<sub>1</sub>,3-5], it was slightly modified in FL and used from then onwards in the form (66.7) devoid of contrary terms:

- |        |       |             |     |                   |    |            |
|--------|-------|-------------|-----|-------------------|----|------------|
|        | (1) A | Every       | X   | is                | Y  | X))Y       |
|        | (2) a | Every       | Y   | is                | X  | X((Y       |
|        | (3) E | No          | X   | is                | Y  | X).(Y      |
| (66.7) | (4) e | Everything  | is  | X or Y or both    |    | X(.)Y      |
|        | (5) I | Some        | Xs  | are               | Ys | X( )Y      |
|        | (6) i | Some things | are | neither Xs nor Ys |    | X)(Y       |
|        | (7) O | Some        | Xs  | are not           | Ys | X).(Y      |
|        | (8) o | Some        | Ys  | are not           | Xs | X).(Y' = ) |

Drawing on S<sub>1</sub>, Hamilton accused De Morgan of inventing numerical syllogism based on his own system (63.3), on the grounds that (66.2) contained (63.3). He further claimed to have had arrived himself at the numerical form before De Morgan, but to have had rejected it as a "cumbersome and useless subtlety" [S<sub>2</sub>,35,45fn1,49fn1;1860c,263fn2]. If we look carefully at the systems (63.3), (66.2) and (66.6) we will notice that they all admit quantification of both subject and predicate. De Morgan overlooked this similarity in S<sub>1</sub> and in the main corpus of FL, to realize it only after examining the connection between (63.3) and (66.2) in the "Appendix" of [FL, 330-303]. He then set off to prove that the "exceedingly remarkable" occurrence of eight forms of quantification in Hamilton's and his own system was purely accidental [S<sub>2</sub>,34,45].

De Morgan stressed in S<sub>2</sub> that all his interest "in novelty of quantification [in S<sub>1</sub>] was directed to the algebraical form of numerically definite propositions [(66.2)], this complete distribution of all the quantifications, existing in the system of contraries [(66.6)], was overlooked" [S<sub>2</sub>,34-5]. When Hamilton claimed that his own system was contained in the numerical one, only then De Morgan realized that the distribution of quantification, the "basis of invention" for Hamilton's (63.3), was "incidental to the system of contraries"; in other words, the "accidental form" of distribution in (66.6) was by mere coincidence the "substantial form" of (63.3) [S<sub>2</sub>,34-5]. He further

tried to clarify the mutual misunderstanding by claiming :

...for though the system I now write upon [(66.6)] does contain that extent of quantification [of both subject and predicate], and although it was published [in S.] before I had any knowledge even of the fact of Sir William Hamilton having a system of his own [(63.3); see (1) above], yet I can most distinctly affirm that all my perception of complete quantification of both terms was derived from the algebraical form of numerical quantification [(66.2)]'".

No matter how far De Morgan's attempt to convince Hamilton that the controversy was groundless was indeed satisfactory, the truth was that the two logicians had worked on independent lines having different aims in mind. This conclusion is further established if we consider De Morgan's interesting assertion that the system (66.6) was a pure outcome of his original invention of "the algebraical form of numerical quantification" [(6) above].

[At this point there are some more instances in his work worth of mention which indicate his perception of numerical syllogism (66.2) prior to either Hamilton's (63.3) or to his own (66.6). Discussing Hamilton's discovery and rejection of the numerical system, he indicated an instance of a syllogism given in his [1839,14] :

Some A is X  
(66.8) Some B is not X  
-----  
Some B is not A

which did not lay "within the [traditional] forms of predication" [S<sub>2</sub>,45,fn 1]'?', thus implying, if not priority in his deviation from Aristotelian syllogistic, at least his independent discovery of non-traditional forms which, subconsciously, led to his formulation of (66.1) and consequently of (66.2). Another instance which confirms that it was by 1839 that "the now confirmed notion of the usual form of syllogism [(66.7)] being arithmetical was [then] germinating" was given in 1858, cited in [6.4.(8)]. The priority of the numerical over the arithmetical system was further stressed in 1860. In [1860c,262] it was asserted that (66.7) and its consequences "are truly particular cases of the numerical proposition" [see also (1) above]. And finally in S the order of

exposition was: numerical propositions, universe of proposition, contrary of a term followed finally by the arithmetical system (66.7) 'a'.

Having treated simply the numerically definite syllogism in 1847 [FL,141-170], De Morgan only briefly referred to it from 1850 onwards on the grounds that "syllogisms with numerically definite quantity rarely occur, if ever, in common thought" [S,172]. However, the influence implicitly derived from arithmetical quantification was immense; on one hand the mere notion of quantification -forming accidentally the basis of the syllogistic system (66.7) upon which he built the rest of his theory- in many ways foreshadowed contemporary applications of quantifiers [see (16) below] and, additionally, it played a crucial role in the shaping of the theory of relations in 1860 [see 6.8]. On the other hand it formed the initial cause of the dispute which was beneficial to De Morgan as we shall see.

Having clarified that the dispute was in large groundless, De Morgan went on in S<sub>2</sub> to comment upon Hamilton's system (63.3) and to compare it with his own (66.7). In so doing he revealed his mathematically based conception of the notions of "consistency" and "extension" missing in Hamilton, his conception of algebra as a paradigmatic science and his peculiar historically orientated approach of proceeding forward without rejecting what at first may seem singular and incomprehensible. By means of examples he first showed that while every syllogism derived by means of (66.7) can be reduced to Aristotelian form, there are syllogisms deduced from Hamilton's system which can in no way be made Aristotelian. Hence, his own system can be called an "extension" of Aristotle's [S<sub>2</sub>,42], whereas Hamilton's (63.3) is "an independent addition to that of Aristotle" [S<sub>2</sub>,43]. It is of interest to follow his argument :

It will be observed that I have not called [(63.3)] an extension of that of Aristotle. That it is more extensive, in one sense, I admit; namely, in so far as it includes all which Aristotle included, and more. But a mathematician cannot therefore call it an extension, accustomed as he is to a very precise use of that term. With him enlargement is not extension, unless the wider extent be governed by the laws of the narrower one<sup>(9)</sup>.



He next attacked the inconsistency governing Hamilton's fundamental propositions. For a syllogistic system to be self-consistent (a notion first stressed in his logic in 1847; see (1) above), the fundamental propositions should be independent from each other and contradictory in pairs [S<sub>2</sub>.44]. Now, Hamilton's propositions (1) and (8) of (63.3) violate this rule. (1) is a compound of (2), or ")", and (3), or "(", and the negation of (1) is a disjunction of (6), or ")", and (7), or "((". Finally, proposition (8) has no fundamental proposition that denies it, not even a compound of other propositions [S<sub>2</sub>.44].

Diverging for a moment at this point from our chronological exposition we would like to raise the reader's attention to the delicate issue of notation. First of all prior to entering into any substantial analysis of their systems, De Morgan proposed the new bracket-dot notation in S<sub>2</sub> and compared it with Hamilton's own [S<sub>2</sub>.29-34; see also 6.3. (13)]. This new notation was a most useful tool in his consequent comparison between the forms in (63.3) and (66.7). However, despite his scholastic commentary upon the defective (63.3) in S<sub>2</sub>, the role of De Morgan's notation can be best revealed in his condensed comparison between the two systems in [S.166,fn3] and in his article on "Logic" [1860c,257-260]. In the latter article he displayed the two systems symbolically, showing that (63.3) "fails in symmetrical distribution of quantification". And while in S<sub>2</sub> he only mildly criticised Hamilton expecting "a powerful consideration" of his comments that would lead Hamilton to improve upon his work [S<sub>2</sub>.45], after the latter's death he was bitterly hostile towards Hamilton's defenders by saying:

A system of propositions [(63.3)] which mixes the simple and the complex [...] and which offers an assertion and denial which cannot be contradicted in the system, seems to me to carry its own condemnation written on its own forehead<sup>10</sup>.

Particularly upset with the form (8) of Hamilton's system (63.3), De Morgan grasped the opportunity to draw an interesting analogy between formal logic and symbolic algebra. We read in [S<sub>2</sub>.45]:

Symbolic language gives the expression of the laws of thought in

their purest forms: and it has never deceived those who were willing to be servants before they claimed to be its masters. In the present case, there seemed something resembling a system of algebra with a singular form [(8) of (63.3)] in it. Formal logic must teach how to enunciate all definitely conceivable truth and falsehood, just as symbolic algebra must teach how to enunciate all definitely expressible quantity: and "some Xs are not some Ys" appeared to partake very much of the indeterminateness of  $0/0$ . An algebraist has not profited by the history of his science, if he dogmatically reject what appears incapable of interpretation of its system<sup>11</sup>.

On the grounds of this analysis the problematic form (8) of (63.3) was not to be rejected; the task of a historically minded algebraist (and logician) was to find a means to interpret any singular forms so as the system under discussion would be self-consistent. Urged by this task, De Morgan distinguished between the "cumular" form, as in "All X is all Y", and the "exemplar" form, that is the previous proposition now read as "Any one X is any one Y". This distinction was to prove an essential innovation in De Morgan's syllogistic; directly connected with his attempt to amend Hamilton's system, the exemplar form was amply discussed in De Morgan's later writings and its importance in geometrical and algebraical reasoning was raised [1860c,266;S<sub>2</sub>,298-9]. In [S<sub>2</sub>,46] it was claimed that "the exemplar proposition must precede in order of thought: and it is justifiable to propose it as the basis of a logical system"<sup>12</sup>. Thus, Hamilton's cumular system (63.3) was presented in the exemplar form merely by replacing "All" by "Any one" and "Some" by "Some one" [S<sub>2</sub>,47;see also S.168,169 fn1;1860c,261,265 fn1].

Being restored in the exemplar form, Hamilton's system was "free from the objections which I have urged against cumular forms, so far as contradiction is concerned" [S<sub>2</sub>,47]. Thus, the contrary pairs in the new form of (63.3) are: (1) and (8); (2) and (7); (3) and (6); (5) and (4). However, a thorough amendment of the defects noticed above needed a further development of the theory of the abstract copula which was introduced in FL [see S<sub>2</sub>,50-60;6.5]. The subtle point was to extend the copula "is" in such a way so as to correlate distinct instances of the subject

X with respective instances or with the whole of the predicate. Replacing the copula "is" by an arbitrary relation denoted as in (65.13) by the symbol "-" (its negation accordingly denoted by "-") we are able to form propositions such as

(66.9) "one X - Every Y"

which is less restrictive than "Every X is Y" -in which only one subject example can agree with one predicate example [S<sub>2</sub>,60].

"Hence follows an extended mode of interpreting Sir William Hamilton's system into consistency", claimed De Morgan after suggesting the form (66.9). Let (1) in (63.3) "All Xs - all Ys", or  $X)(Y$ , mean that "every X stands in the copular relation to each and every Y". Then (1) is contradicted by (8) "Some Xs. - - some Ys" for either each X and each Y agree, or some Xs do not agree with some of the Ys. Moreover, we can no longer assert that  $X)(Y$  is a proposition compounded of two others,  $X))Y$  and  $Y))X$ , for each X may agree with some of the Ys, and each Y with some of the Xs, without every X agreeing with every Y [S<sub>2</sub>,61].

On these grounds, Hamilton's system, read as :

- (1) Each X is related to all the Ys )(
- (2) Each X is related to one or more Ys ))
- (3) Some Xs are related to all the Ys ((
- (66.10) (4) Some Xs are related to some one or more Ys ( )
- (5) No X is related to any one Y ).(
- (6) No X is related to some one or more Ys ).)
- (7) Some Xs are not related to any Ys (. (
- (8) Some Xs are not related to some of the Ys (.)

is free from "all objections" that can be raised [S<sub>2</sub>,61-63;see also S.167-8;Prior 1962,149-50]. Despite this optimistic claim the controversy was not to be over before 1863 [see (10) above].

Before concluding our account of De Morgan's syllogistic theory we would like to add a few words on his peculiar bracket-dot notation which proved of a great convenience to him but formed one of the biggest obstacles to other readers to become familiar with his work. By means of this notation inferences flowed rapidly on a mechanical basis. He showed that a syllogism may be denoted by juxtaposition of the symbols of the premises, writing the minor first for convenience. Then, the canon of inference has as follows: "Erase the symbols of the middle term, the

remaining symbols shew the inference" [S<sub>2</sub>.31]. For example, take the syllogism

	A Every X is Y	X))Y
(66.11)	o Some Zs are not Ys	Y)).Z
	-----	-----
	o Some Zs are not Xs	Y)).Z

written in the condensed form

(66.12) X))Y)).Z=X)).Z,

or purely symbolically as

(66.13) )))=)).

[S<sub>2</sub>.31].

If we employ De Morgan's early notation of S<sub>1</sub> or FL [see (4) above] then, figure AAA, for instance, would be written as

(66.14) X)Y+Y)Z=X)Z

[S<sub>1</sub>.11]. We notice a substantial difference both in the symbolic expression of the fundamental propositions, A given first by "X)Y" and then by "X))Y", but also in the manner of denoting the inferences, (66.14) and (66.12). De Morgan confined from 1850 to the latter notation; why did he make this switch from (66.14) to (66.12)? The answer is to be found in later writings as briefly shown below.

As indicated by the following passage written in 1863, the idea of the bracket-notation originated in the algebra of fractions, and so did the switch from ")" to ")))":

In using one symbol, ), as in X)Y [in 1847], to denote both the total quantity of the subject and the particular quantity of the predicate, I followed the play by which a fraction is represented, in which one symbol distinguishes both numerator and denominator; and I ultimately marked the symbol twice [X))Y in S<sub>2</sub>]. If a fraction had been denoted by  $\frac{a}{b}$ , a and b would have been convenient symbols for a as a numerator and b as a denominator; and might be made useful even as it is<sup>(13)</sup>.

Now, the difference between the manner of denoting the inference lay in the absence of "+" from 1850 onwards. This was explained in S<sub>3</sub>; let A, B be the premises of a syllogism, and C the conclusion. The "A+B=C" is "faulty" as the premises are "compounded, not aggregated" [S<sub>3</sub>.87]. He further emphasized this



and (8) above].

Under the challenge of accusation of plagiarism, De Morgan delved further into the syllogistic theory introduced in S<sub>1</sub> and expanded in FL, producing a "powerful" symbolic language by means of which he compared his system (66.7) with Hamilton's own (63.3) [see (2), (4), (5), (14) above]. In so doing he revealed his advantages over Hamilton as a mathematician over a pure logician in an attempt to render the latter's system self-consistent [see (1), (9), (10), (11) above]. He further drew an important distinction between "cumular" and "exemplar" quantification [see (12) above], and, combining the latter notion with the theory of the abstract copula, he extended the copula "is" to "has relation to" thus amending Hamilton's system and preparing the grounds for his theory of relations in 1860 [(65.11); 6.5, (8); (66.9)-(66.10); see also (68.15)-(68.16); 6.9].

These issues help to clarify certain mistaken views on De Morgan's work [see (6) above] and fill in many gaps in the obscure evolution of his ideas. A historically minded algebraist, he drew occasionally on his mathematical background, casting syllogistic inferences in symbolical form and revealing his familiarity with the notions of "consistency" and "extension" [see (11), (9), (13), (14) above]. Further, the intricate issue of quantification was to foreshadow the application of modern quantifiers<sup>(14)</sup>.

Perhaps the most important of all the issues raised in the course of this section is the implicit help derived from his dispute with Hamilton in the shaping of S<sub>2</sub>, a turning point in the development of his syllogistic theory. The best epilogue here is De Morgan's own acknowledgement of Hamilton's beneficial role. In the "Postscript" of his next memoir published in 1858 we read :

I need hardly say that I allude to the late WILLIAM HAMILTON a name which I hold entitled to the honour of losing its conventional accompaniments. And first, I take this opportunity of acknowledging the essential benefit which he conferred on my speculations.

Moreover, in the "Preface" to his Syllabus [1860b] he wrote :

...to the late Sir William Hamilton of Edinburgh I owe it that I can present this tract to the moderately well informed elementary

student of logic, as containing matters of which he is likely enough to have heard something, and may possibly be curious to hear more<sup>(17)</sup>.

#### 6.7 De Morgan on the links between logic and mathematics: the form-matter issue:1847-1858.

In the two previous sections we had the opportunity to notice several instances where De Morgan's theory of the abstract copula and of the structure of the syllogism was influenced from arithmetical and algebraical notions and symbols<sup>(18)</sup>. Certain of these instances did not pass unnoticed by his contemporaries. Mansel, in particular, undertook to examine the validity of De Morgan's bold innovations -which extended the realms of traditional logic in a rather unorthodox way. In Mansel's review of FL in 1851 we read:

Mr. De Morgan regards the processes of arithmetic and algebra as exhibiting the pure form of reasoning [...]. His system, fully carried out, would make logic an application of mathematics: we hold mathematical, in the same manner as any other reasoning, to be an application of logic. Our difference is thus fundamental. We believe that there is no tenable principle of distinction between the matter and the form of thought which will not make the greatest part of his "Formal logic" material<sup>(19)</sup>.

De Morgan was neither the first nor the boldest to have "corrupted Logic with mathematics"; Solly and Boole had also been found guilty of subordinating logic to mathematics [Mansel 1851,96;6.3,(10)]. Nevertheless, De Morgan's offence was more challenging to the supporters of traditional logic, as his book FL claimed by its title to be a substitute for "formal logic". Thus, overlooking Boole's or Solly's works which emphatically deviated from Aristotelian logic with their explicit mathematical methods, Mansel focused on inquiring how far the content of FL was indeed "formal"<sup>(20)</sup>.

Sensitive to criticism, and keen to defend the novelties introduced in S, and FL -which were virtually all rejected by Ha-

milton and Mansel- De Morgan felt obliged to submit a full account of his own version of the form-matter distinction, as well as to speculate on the acceptable interactions between mathematics and logic. His response was eventually incorporated in his third memoir  $S_3$  (1858), which, beside being the lengthier memoir produced between 1847 and 1860, was the most chaotic piece of his logical writings. It included a rather satisfactory discussion of the form-matter issue, but it also delved into a reexamination of logic under a messy philosophical prism and an obsession with classification [6.6,(14)].

The scope of this section is to examine the degree of convergence between mathematical and logical methods in De Morgan's writings -as published prior to  $S_4$  on relations- and to point out his own comments on the mutual utility of these sciences. We know that his two elementary accounts of Aristotelian logic, [1831] and [1839], were principally meant for students of algebra and logic [3.4;6.4,(8);6.5]. We set off by examining the purpose of FL and the role of mathematical examples in  $S_2$ , proceeding next to Mansel's attack and De Morgan's reply to it.

In the manuscripts of the old "Preface" of FL we read :  
Of twenty years experience as a teacher of mathematics I may now affirm that the first half of the period established in my mind the conviction that formal logic is a most important preliminary part of every sound system of exact science and that the second half has strengthened that conviction".

Drawing a parallelism between the instruction of the formal laws of inference in Oxford and that of Euclid at Cambridge, he held that students of both sciences are unable to comprehend the teacher's language and learn mechanically by lacking a proper study of first principles.

These concerns were stressed for the first time in 1835 culminating in the treatise on functions [1836] and the pamphlet [1839] -see [3.4,(14),(15),(22);3.5,(4);6.5]. The second decade of mathematical teaching experience reinforced De Morgan's conviction for the necessity of a more solid work which would clarify logical foundational issues for the student and which would also lay the grounds for a revival and extension of tradi-



tional logic for the advanced reader. This work was FL [1847b] which, in contrast with [1839], was meant neither exclusively for the instruction of young students, nor solely for mathematicians. Considering various modifications of a logical principle, De Morgan wrote in [FL.107]:

If all my readers were mathematicians, I might pursue these extreme cases, as having interest on account of their analogy with the extreme case which the entrance of zero and of infinite magnitude oblige him to consider. But as those who are not mathematicians would not be interested in the analogy, and those who are can pursue the subject for themselves, I will go on...

Thus, De Morgan was conscious of the fact that his audience did not consist necessarily of mathematicians: from those who happened to be he simply expected the capability to draw analogies between the two sciences. But while in FL he was careful to avoid any reference to algebra -with the unique exception the instance where he alluded to it in connection with the theory of the abstract copula [6.5.(4)]- in  $S_2$  he was to be much bolder in pointing out links between logic and algebra, regarding the latter as a paradigmatic science [see 6.6.(11) and text below].

De Morgan's first appeal to algebra in order to establish a logical notion was made in [S<sub>1</sub>.3] when he wrote:

Those who teach Algebra know how difficult it is to make the student fully aware that  $a$  may be the negative quantity, and  $-a$  the positive one. There is a want of a similar conception in regard to direct [X] and contrary [not-X] terms.

In  $S_2$ , generalizing over the duality of these two notions, "negative" and "positive", he claimed that since in algebra "all oppositions are instrumentally reducible to addition and subtraction", in logic all complementary notions, such as "existent" and "non-existent", or "conjunctive" and "disjunctive" can be reduced to the case of "universal and particular" [S<sub>2</sub>.23-26]. Happy with his conclusion, he went on to make his habitual hypothetical statement:

I think it reasonably probable that the advance of symbolic logic will lead to a calculus of opposite relations for mere inference, as

general as that of + and - in algebra<sup>(6)</sup>.

The next instance of analogy to be raised was between "elimination in algebra" and "inference in logic". For example:

When I say " "John met Thomas in the street" -"John shook hands with Thomas"- that is, "John shook hands with what John met in the street" " -there is an elimination of "Thomas" perfectly answering in process to " $a+x=b$ ,  $y=c+x$ , therefore  $y=c+b-a$ "<sup>(6)</sup>.

These are characteristic instances of analogies which served as a reinforcement of De Morgan's logical arguments in  $S_1$  and  $S_2$ . He had warned the reader in the opening of the second memoir that the methods to be put forward "have nothing in common with that of Professor Boole, whose mode of treating the forms of logic is most worthy the attention of all who can study that science mathematically" ( $S_2$ .22-3; see (3) above). In other words, the reader was prepared so as not to misunderstand De Morgan's frequent appeal to algebra and erroneously conclude that his treatment meant to be mathematical. And, indeed, it is true that De Morgan did not apply any sophisticated concepts, theorems or methods in his logic -as studied so far- which were distinctly mathematical. However, as a historically minded scientist, he did his best to benefit from the recent development of algebra and to employ basic concepts and general procedures which apparently would be comprehensible equally well <sup>by the</sup> mathematician and the logician.

As a matter of fact, even this minimal use of arithmetical and algebraic notions, as in the case of the numerical syllogism (66.2), were to cause a stir among supporters of traditional logic. Above all, the doctrine of the abstract copula was the issue par excellence to be rejected by Mansel. And this issue was lying in the core of De Morgan's extension of traditional logic and it had to be ardently defended. The very notion of extension -discussed by De Morgan particularly in connection with Hamilton's system [6.6.(9)]- was borrowed by De Morgan from its application in algebra. In his review of Peacock's algebra De Morgan distinguished for the first time between generalization

by "induction" and by "extension"; the latter being intimately connected with the former's PEF [3.4,text and (15)-(22)]. Extension via abstraction was thoroughly investigated in his treatise on functions [1836] and the results of that study were further elaborated and applied in his papers on the foundations of algebra which culminated in the book Trigonometry and double algebra [1849c]. De Morgan's outcome of his experience with abstraction is summarized in  $S_2$  as follows:

In algebra, as it now stands, the forms born and educated in arithmetic have left their parent and set up for themselves. Any meanings which obey certain specified laws may be adopted as the means of giving expression to the forms: and the results must be accepted as true in every instance in which the combinations used are consistent and intelligible under the meaning given<sup>(7)</sup>.

Following the example of algebra, De Morgan had separated in FL the essential from the accidental characteristics of the copula "is" in order to render it abstract [6.5.(4)]. This process was not accepted by a "learned critic" who said that "some of [De Morgan's] modes of making the copula [abstract] are less abstract, none more so, than is and is not" ( $S_2$ ,50-51). This comment -among other motivations- urged De Morgan to dwell upon the issue of the abstract copula in his second memoir in 1850 [6.5,6.6]. However, Mansel's critical review in 1851 focused exclusively on FL. Prior to commenting upon FL, he amply displayed his own conception of the form-matter (hereafter cited as FM) distinction [1851,97-105] which in brief is as follows:

The thinking process itself may also be distinguished as material or formal. It is formal when the matter given is sufficient for the completion of the product, without any other addition than what is communicated in the act of thought itself. It is material when the data are insufficient, and the mind has consequently to go out of the thinking act to obtain additional materials<sup>(8)</sup>.

Examining upon these grounds the numerical syllogism (66.4) which he accepted as valid, Mansel wondered "is it valid in consequence of its form or of its matter?" To infer a conclusion from the premises alone we need to know that "33-21=12"; "Does he

derive this knowledge from logic or from arithmetic? In the latter case the consequence is not formal but material. It is no answer that this knowledge is possessed by all civilized men" [1851,108-9]. All the remarks concerning the numerically definite syllogism were applicable to the substitution of probability theory in the place of modality; "If all arithmetical and algebraical processes are extralogical, the theory of probabilities is of course excluded along with every other application of the calculus" [1851,111]. Holding further that "negation is not an affection of the predicate, but of the copula", Mansel rejected De Morgan's arithmetical system (66.6), and his novelty of complex syllogism, on the grounds that "his whole theory of a material universe, with its positive contraries, is extralogical" [1851,111-113].

Finally, Mansel rejected De Morgan's oblique inference (64.1) [see 6.4,(7)] and his account of the copula holding that "the true logical copula we believe to be in all cases an assertion of identity or distinctness, and as such, a form of the judgment" [1851,101,fn;105-106]. Displaying the evident fallacies

	The hand touches the pen	Paris killed Achilles
	The pen touches the paper	Achilles killed Hector
(67.1)	<hr/>	
	Therefore the hand touches the paper	Paris killed Hector

Mansel argued as follows "But how do these examples differ in form from "A gives B, B gives C; therefore, A gives C"? He will tell us that the verb "gives" communicates its action, the verbs "touch" and "kill" do not. But is this knowledge formal or material?" [1851,106-7; see also (8) above].

De Morgan and his critics both agreed that the property of transitivity is a material issue. However, they had different conceptions of the nature of the copula. Sticking exclusively to "is", Mansel and Hamilton held that the copula is a formal sign incorporated in the form of the proposition. De Morgan, on the other hand, did not differentiate between "is" and "gives", in the sense of "produces"; the latter is neither less abstract nor more material than the former. For this reason he had claimed in [S<sub>2</sub>,68] that "the copula has been material to this day", a dubious

statement which was not surprisingly refuted by Mansel [1851,101 fn]. Given the views of traditional logicians, De Morgan's statement was enough to cast doubts on how far his logic was formal or material, calling in question his own distinction between these two notions<sup>(9)</sup>.

De Morgan's dubious statement is far from being the unique case of a puzzling, obscure statement in his writings. We have, for instance, his characteristic oscillation between meaningful and formal algebra which would puzzle him from 1835 up to the 1860's [see 3.5.3.9]. But, in a sense, De Morgan was more consistent and linear in his logic; he did take a considerable step towards the formalization of the copula in  $S_2$  -totally overlooked by Mansel- and further in  $S_3$ , as we shall see below. And as we will see in 6.8, De Morgan's  $S_4$  consisted <sup>mainly</sup> of formally valid inferences. But let us see De Morgan's own account of the FM distinction provided in  $S_3$  and his reply to certain of Mansel's accusations.

According to De Morgan, the FM issue is evidently comprised into the process of "abstraction" and "generalization"; apparently unaware of the fact "that he abstracts form from matter", the mathematician does so constantly in practice [ $S_3$ ,77]. Claiming that both geometry and arithmetic basically spring out of man's mathematical ability to separate "space from matter filling it" [ $S_3$ ,77], he went on to say:

Distinctions which are of form in arithmetic become material in algebra. The lower forms of algebra become material in the algebra of the functional symbol. The functional form becomes material in the differential calculus, most visibly when this last is merged in the calculus of operations<sup>(10)</sup>.

The study of this scale of gradual abstraction and generalization had been distinctly carried out by De Morgan in his foundational study of the calculus of functions; for instance the identity " $f(x)=x^2+1$ " is a formal statement from the point of view of arithmetic, but a material one in the calculus of the more abstract operation symbols  $\Delta$ ,  $d/dx$  etc [see 3.6]. In [1860c,248] he illustrated the FM issue saying that in algebra " $8+4=4+8$  is but one material instance of the form  $a+b=b+a$ " while

it is a "form" a law of thought" in erithetic. While this process is unlimited with in mathematics (see Sylvester's statement in 5.10.(12)), for De Morgan there is a similar limited process for the gradual formalization of the judgment as displayed in the table below:

		Hypothesis
(Positively true)	Every man is animal	
-----	Every man is Y	Y has existence
(67.2) -----	Every X is Y	X has existence
-----	Every X-Y	- is a transitive relation
-----	$\alpha$ of X-Y	$\alpha$ a fraction $\leq 1$
(Probability $\beta$ )	$\alpha$ of X-Y	$\beta$ a fraction $\leq 1$

claiming that the last step "is nearly the purely formal judgment, with not a single material point in it, except the transitivity of the copula. But is is more intense than the symbol -, which means only transitive copula: for is has transitivity and more. Strike out the word transitive, and the last line shews the pure form of the judgment" [S<sub>3</sub>,80].

To defend his doctrine of the abstract copula as rejected by Mansel on the grounds of the fallacies (67.1), De Morgan wrote prior to (67.2): "I might as well say that "Every X is Y" is a material proposition: it is of the matter of X and Y whether it be true or no" [S<sub>3</sub>,80]. The two men had different conceptions of the FM issue; while Mansel gave his own in a vague way, De Morgan went deeply into the heart of the matter showing by the chain of propositions (67.2) how "there is exclusion of matter, form being preserved, at every step" in a most accurate way. To do so he drew, as it is evident from the citation in (10) above, on the calculus of functions. This fact is best acknowledged in Bochenski [1970,320]:

Neither De Morgan nor any other logician can remain at so high a level of abstraction as is here achieved. Basically, this is a rediscovery of the scholastic concept of form, made through a broadening of the mathematical concept of function, for which we refer to Peirce and Frege.

The last steps of (67.2) suffice to rescue De Morgan's name-

rically definite syllogism and the probability theory from Mansel's attacks. Confining to Mansel's objection concerning the case (66.4), we have De Morgan's answer in brief as follows: we do not expect a logician to know "as much" as that " $33-21=12$ "; it suffices for the general case (66.2) to hold true to take under consideration the restriction (66.5). If the latter condition holds, then (66.2) is valid [S<sub>2</sub>,80-81].

The process sketched in (67.2) for the formulation of the pure form of the judgment was extended to the analysis of reasoning. "A little consideration", he wrote, "will shew us that every inference which is anything more than pure symbolic representation of inference, is due to the presence of something material" [81,S<sub>2</sub>]. Assume two purely formal propositions -according to (67.2)-

(67.3)  $P - A - Q, \quad R - B - S,$

where the objects P,Q,R,S are connected via the relations A and B. To rub out R and write for it Q and infer " $P - AB - S$ " the process is material. If we substitute the material copule "is" for A and B we have the material inference " $P$  is Q, Q is S, therefore P is S".

Via this analysis De Morgan showed that traditional logic is in fact a material instance of the logic of relations. In his own words he claimed that:

In common logic, the objects of inference, being terms expressed in general symbols, are void of matter; the relations between them, and the modes of inference, are material: I speak of logic as it is. Many relations have a common form: the logician cannot yet see that when many cases, no matter what, proceed upon a common principle, his concern is with that principle. It is his business to apprehend the principle and to shew, as to the modus operandi of the mind how containing cases severally contain it, and apply it<sup>(1)</sup>.

While Mansel avoided to enter into any discussion of S<sub>2</sub>, Hamilton apparently did, objecting to the analysis of logical oppositions given in (5) above. De Morgan's commentary on this objection is worth recording:

I hope at some future time to treat of a pure form of opposition

which runs through all contraries. In [S<sub>2</sub>] I gave some account of the way in which various words [...] may be described in terms of any one of the others. An eminent critic [Hamilton] there upon says that I "formally identify" these terms. If this means that I say, in form, that they are identical, he is not correct: but if it mean that I contend for a common form running through all logical oppositions, he is correct so far as this, that I ventured to predict the future establishment of such a form. My critic adds that my system is a witches' cauldron: I accept the phrase. Algebra is a witches cauldron. It has two handles, + and -. By these we lift on the fire, at once, the distinctions of addition and subtraction, multiplication and division, up and down [...]. They all boil together, and the results are magical. The spell was impaired by the long time which certain roots (of negative quantities) took to boil; but they are now quite done. The logical cauldron, of which I have some further knowledge, I hope to set boiling at some future time<sup>(12)</sup>.

Negative numbers being acceptable by Peacock's PEF, algebra became gradually in the 1830's and 1840's a powerful magical cauldron in De Morgan's hands. This paradigmatic science offered him the ability to extend and to formalize, as well as to perceive similarities between forms and principles (see (5), (10), (11), (12) above). In reply to Mansel's accusation of subordinating logic to mathematics, De Morgan briefly clarified that his treatment was not peculiarly mathematical, saying:

I am charged with maintaining that thought is a branch of algebra, instead of algebra a branch of thought. The answer is easy enough. Logic considers, not thought, but the form of thought [...]. He who makes me confound all other thought with algebra, because I call attention to what is more visible in algebra than in other thought, though it exists in all thought, must make his own logic responsible for the instance, not mine<sup>(13)</sup>.

In reply to Hamilton's objection he vaguely implied that he may take such a bold step in the future; equally vague he were in the opening of S<sub>3</sub> when he discussed the future interaction between the two sciences:



As joint attention to logic and mathematics increases, a logic will grow among the mathematicians, distinguished from the logic of the logicians by having the mathematical element properly subordinated to the rest. This mathematical logic [...] will commend itself to the educated world by showing an actual representation of their form of thought [...] instead of a mutilated and one sided fragment, founded upon canons of which they neither feel the force nor see the utility<sup>14</sup>.

De Morgan introduced the term "mathematical logic" for the first time in this passage [S<sub>3</sub>,78]. What did he mean by it? And what did he imply when he wrote in (12) above that he would set the logical cauldron "boiling at some future time"? Further below in S<sub>3</sub> the representative form of a "mathematical" proposition in logic was taken to be "The class "man" is included in the class "animal"" [S<sub>3</sub>,96;see(67.5) below]. And there are reasons to believe that the cauldron of logical oppositions gave rise to his logic of relations in 1860 [6.8-6.9]. There are, however, many unclear instances in S<sub>3</sub> which alone do not afford at the moment a satisfactory speculation on De Morgan's concerns on the mutual application of logic and mathematics. We thus proceed to a brief commentary on the issue of symbolic notation in De Morgan's logic, concluding with a representative instance of his obsession with classification through which he reexamined logical forms.

As we saw in [6.6;text above (66.15)], both Mansel and Hamilton disapproved of De Morgan's notation. The latter replied in S<sub>3</sub> in a general way:

A great objection has been raised by the employment of mathematical symbols: and it seems to be taken for granted that any symbols used by me must be mathematical. The truth is that I have not made much use of symbols actually employed in algebra<sup>15</sup>.

A more specific reply to Mansel was incorporated in S<sub>3</sub> five years later:

To this I say first, that + and = are not signs peculiar either to arithmetic or to algebra. A person who has no counting, and as yet no symbols, might be introduced at once to the symbolic aggregation of concrete lengths, which is seen in

Thus, while there is evidence that De Morgan did draw from arithmetic and ratios to invent his notation in logic [6.6.(13)], he did not employ any complicated mathematical notation peculiar specifically to algebra. He did admit, though, in [S<sub>2</sub>,324,fn 2] that "Forgetting that I was not writing wholly for mathematicians, I used expressions on this subject which were misunderstood". De Morgan paid particular attention to the importance of notation in the development of any science: "Every science that has thriven has thriven upon its own symbols" [S<sub>2</sub>,88]. He had always in mind the paradigmatic case of algebra whose peculiar notation afforded rapidity, simplicity, condensity and symmetry, properties which his dot-bracket notation (66.15) was shown to possess too. Above all, the FM separation (as illustrated in (10),(11) above) demands of distinct symbols for the different levels in the scale of abstraction and generalization -as exemplified in the table (67.2) above. Upon these grounds he wrote in S<sub>2</sub>:

Every system of signs, before it has become familiar [...] is repulsive, difficult, unmeaning [...]. But it is too certain to need argument that the separation of form and matter requires as many symbols as there are separations<sup>(17)</sup>.

He was to follow on these lines in his new notation introduced in S<sub>4</sub> for his logic of relations [6.8-6.9]. Before we proceed to a study of the latter subject, the grounds of whose development were prepared in S<sub>2</sub> [6.8], let us conclude our account with De Morgan's division of logic - omitting his ample discussion of philosophical and logical issues. He first distinguished between the "objective" and the "subjective"<sup>a</sup> part of logic: the former consists merely of the arithmetic<sup>w</sup> branch of logic which deals with individual objects, the latter with attributes and classes [S<sub>2</sub>,89,117-119]. Thus, under the former category we have the numerically definite syllogism in which the system (66.6) based on contraries is comprised [see 6.6 text and (1)-(8)]. The subjective branch is further subdivided

[S<sub>3</sub>.96;123;S.179] as follows:

	Subject	Predicate
	Mathematical	Class
(67.4)	Physical	Class
	Metaphysical	Attribute
	Contraphysical	Attribute
		Class

Thus, De Morgan's syllogistic calculus admits of at least five different interpretations. Taking for example "Every man is an animal" we distinguish the following five reading:

- |        |                    |  |
|--------|--------------------|--|
|        | (1) Arithmetical   | Every man is an animal   |
|        | (2) Mathematical   | The class "man" included in the class "animal"                   |
| (67.5) | (3) Physical       | The class "man" has the attribute "humanity"                     |
|        | (4) Metaphysical   | The notion "animal" a component of the notion "human"            |
|        | (5) Contraphysical | "Human", an attribute to be looked for within the class "animal" |

[S<sub>3</sub>.96;S.188-190]. As we shall see in 6.8, the syllogism carried out within (2) or (4) of (67.5) is based upon combination -or composition- of relations [S<sub>3</sub>.107;6.8.(1)].

#### 6.8 De Morgan's logic of relations:1858-1860.

In 1831 De Morgan held that the reasoning properly consists in the manner in which the truths of the relations assumed "are combined so as to produce new relations" [6.5;text above (3)]. Eight years later he gave a very first instance of composition of relations in "part of part" which appeared in the a fortiori syllogism (65.7). Thus, his logic of relations was to arise gradually from his theory of the syllogism grounded upon it in 1850 and consequently developed within its realms in the late 1850's. Noticing briefly the frame-work within which he built the foundations of this novel theory in S<sub>2</sub>, and particularly in S<sub>3</sub>, we will focus in this section on the basic notions and theorems introduced in S<sub>4</sub> [1860a]. Our scope is to put the basis for a comparison between De Morgan's structure of the calculus of functions [1836] and that of relations in our concluding section. In

so doing we will avoid to enter deeply into philosophical and logical issues, merely touching upon relational syllogistic to which the second part of  $S_4$  is devoted.

De Morgan took the first important step towards his logic of relations in 1847 when he separated the essential from the accidental properties of the copula "is" [FL;6.5.(4),(5)]. In 1850 he formulated valid syllogistic inferences, "is" extended now to "has relation to", the conclusion of the inference based on composition of arbitrary relations, (65.11). Let " $—$ " and " $...$ " stand for two relations L and M; " $\overline{...}$ " denoting their compound LM. Then, (65.11) was given symbolically in  $S_2$  by (65.13).

$$\begin{array}{rcl} & A & X — Y \\ (68.1) & A & Y ... Z \end{array}$$

---


$$A \quad X \overline{...} Z$$

denoted in  $S_4$ , as we shall see, by

$$\begin{array}{rcl} & A & X..LY \\ (68.2) & A & Y..MZ \end{array}$$

---


$$A \quad X..LMZ$$

[ $S_2$ ,57; $S_4$ ,232;see (68.12) and (68.14)].

The step (68.1) in 1850 shows De Morgan's first major achievement in abstraction and generalization. The final step towards abstraction in the theory of the copula was effected in  $S_3$  in (67.2). Accordingly, the "supreme form of a syllogism of one middle term [(68.1)]" would be

$$\begin{array}{l} \text{There is the probability } \alpha \text{ that } X \text{ is in relation } L \text{ to } Y \\ (68.3) \quad \text{There is the probability } \beta \text{ that } y \text{ is in relation } M \text{ to } Z \end{array}$$

---

Whence: there is the probability  $\alpha\beta$  that X is in relation L of M to Z.

[ $S_4$ ,218].

How great is the role played by the doctrine of the abstract copula and relational syllogism of the general form, (68.1) or (68.2) within logic? "The only copula which logicians provide, for all the modes of reading", wrote De Morgan in 1858, "is the substantive verb is". Nor is another wanted, so long as one relation only is used: but more must now be found" [ $S_3$ ,100]. Lo-

gicians had admitted only of "formal" inferences, rejecting all others on the grounds that they are "material" [6.7 text and (8)]. Thus plenty of valid inferences, such as (66.1)-(66.4), (65.5) -to mention a few- can not be justified within traditional logic, unless reduced to "formal" inferences by an extra step. This extra step involves nothing more but "combination of relations", a process -practiced by traditional logicians- which "is not distinctly proclaimed, and universally applied" [S<sub>4</sub>, 216-7]. Among the reasons why this process was not acknowledged and attended to before De Morgan, was the excessive attention paid to the quantification of the predicate which "totally prevented the expression of any syllogism as a combination of relations"; for example, no one could say that the process of the figure AAA is expressed by "Species of species is species" [S<sub>3</sub>, 101; see also 6.9, (7)].

De Morgan had foreseen by the late 1840's the necessity to extend the limited realms of traditional logic. In both S<sub>2</sub> and S<sub>3</sub> he treated ordinary syllogism as "being one case, and one case only, of the composition of relations" before entering further "on the subject of relation, as a branch of logic" in 1860 [S<sub>4</sub>, 208; see 6.5, (7)]. Prior to discussing the novelties introduced in S<sub>4</sub>, let us see how he prepared the ground for the logic of relations in S<sub>3</sub>. We recall that in 1858 he had distinguished between the "ordinary" syllogism, as belonging to the "arithmetical whole", and the "mathematical and metaphysical" branch of logic which concerned correlations between classes and attributes respectively [(67.4)-(67.5)]. He stressed also that:

The syllogism of [...] wholes of mathematical and metaphysical thought [...] is, in fact, combination of relations: the act of mind by which we see that the A of (the B of Z), or the (A of B) of Z, is thinkable under one relation. Here the compound relation, or combined relation, may be represented by AB, but by no one except a mathematician who is used to the functional symbol, and to the idea of  $\phi \psi(x)$ . Its distinction between the mode of composition of x,y, and that of  $\phi, \psi$ . I use the word combination instead of composition [as in S<sub>2</sub>], to avoid raising this question, and the more readily because, until we treat of sorites, combination is of two<sup>11</sup>.

In the end of  $S_3$  De Morgan attempted to provide a brief systematic account of the main concepts discussed analytically within his philosophical frame-work. We isolate the most important issues recorded in [ $S_3$ ,119-120].

- (68.4) "When two objects, qualities, classes, or attributes, viewed together by the mind, are seen under some connexion, that connexion is called a relation",
- (68.5) "The distinction of subject and predicate is the distinction between the notion in relation and the notion to which it is in relation",
- (68.6) "Every relation has its counter-relation, or converse relation: thus if X be in the relation A to Y, Y is therefore in some relation B to X: and A and B are converse relations...",
- (68.7) "When a relation is its own converse, the proposition is said to be convertible: meaning that the converse exhibits no change of relation",
- (68.8) "When X has a relation (A) to that which has a relation (B) to Y, X has to Y a combined relation [AB]",
- (68.9)  $S_3$  is confined to "onymatic relations", i.e. to "those of whole and part in the two aspects of containing and contained and compounded and component; and also the relations which the notion of contraries, and the notion of true and false, introduce in connexion with them" (2).

We will see the development of these issues in  $S_4$ , which was written between 1858 and 1859. Concise and obscure in its presentation, this memoir is of great importance in De Morgan's contributions in logic. [6.4.(2)], its significance lying in his distinction and systematization of valid forms of relational inference unrecognized by earlier logicians. While  $S_4$  crowns the inquiries in syllogistic theory, the doctrine of the abstract copula, the form-matter distinction, the subtle influence from the theory of quantification and aspects of relational logic as gradually built in  $S_1$ , FL,  $S_2$  and  $S_3$ , it nevertheless lacks the charm of the latter memoirs <sup>and</sup> the force and wit of their arguments. Surprisingly, it is hardly mentioned in his logical publications after 1860 (3).

De Morgan illustrated with several syllogistic and a few



distinction which could not be effected by means of the words "is" and "is not". Recalling of the mathematical process of separation of the symbols of operation from those of quantity -albeit not mentioned explicitly- this distinction was claimed to be "an important step towards the treatment of syllogistic inference as an act of combination of relation" (S<sub>4</sub>, 214-215). He remarked further that "X and Y are subject and predicate: these names having reference to the mode of entrance in the relation, not to order of mention. Thus Y is the predicate in LY.X as well as in X.LY" (S<sub>4</sub>, 220). This "rem<sup>Y</sup>akable extension of the concept of subject and predicate" was not followed by De Morgan's successors [Bochenski 1970, 375].

The "converse" of a relation L -initially defined by (68.6)- was now denoted by  $L^{-1}$  and "defined as usual" by

(68.13) "if  $X..LY$ ,  $Y..L^{-1}X$ ", or,

"if X be one of the Ls of Y, Y is one of the  $L^{-1}$ s of X" (S<sub>4</sub>, 222).

The new version of the definition of a "combined" relation (68.8) was given in the new notation as

(68.14) " $X..L(MY)=X..(LM)Y=X..LMY$ ", or "X is an "L of M" of Y".

However, we are now provided with another two types of compound relations besides "LM", "LM'" and "L.M" defined respectively by

(68.15) " $X..LM'Y$ ", "X is an L of every M of Y"

and

(68.16) " $X..L,MY$ ", "X is an L of none but Ms of Y"

[S<sub>4</sub>, 221]. Conscious of the importance of these two novel types of compound relations which involved quantification of the relation itself, De Morgan wrote in a footnote:

Simple as the connexion with the rest of what I now proceed to may appear, it was long before the quantified relation suggested itself, and until this suggestion arrived, all my efforts to make a scheme of syllogism were wholly unsuccessful. The quantity was in my mind, but not carried to the account of relation. Thus

[(68.17)]  $LX))MY$ , or every L of X is an M of Y,

has the notion of universal quantity attached in the common way to LX. not to L: its equivalents  $X..L^{-1},MY$ , and  $Y..M^{-1}L'X$ , shew X and Y as singular terms, and though expressing the same ideas of quantity as  $LX))MY$ , throw the quantity entirely into the description of the relations<sup>(a)</sup>.



Finally, two more definitions were provided. Since according to the new version of (68.4), relations "exist between any two terms whatsoever", "If X be not any L of X, X is to Y in some not-L relation"; hence we have the definition of the "contrary" relation l of L by

$$(68.18) \quad X.LY = X..lX.$$

Lastly, while "(L))M" stands for relational inclusion in (68.17), "L||M" stands for relational equivalence, or

$$(68.19) \quad L||M \longleftrightarrow (L))M \text{ and } M))L$$

[S<sub>4</sub>, 224-5].

The notation for the composition of two relations is not a novel feature of S<sub>4</sub> [see (1) above and 6.9]. The definition of the contrary relation "l" of "L" is but an extension of that of contrary terms, only in contrast with the latter, the former can be compounded and thus we have for example that "a man may be the partisan of a non-partisan of X" [S<sub>4</sub>, 222-3]. The notation by lower-case letters, as in the case of contrary terms, recalls the notion of "universe" introduced in S<sub>1</sub> [6.6]. The converse of a relation L, "L<sup>-1</sup>" is so denoted after the inverse of a function  $\phi$ ,  $\phi^{-1}$  [see 6.9, (6)-(8), (16)]. The spicular notation in "(L))M" [(68.17)] is in accordance with that put forward in his syllogistic theory [see (8) above]. Finally, the most peculiar feature of his logic of relations in S<sub>4</sub> is the separation of quantity from the terms [as in the case of (66.9)] and its attachment to the relation itself, as in (68.15) and its dual (68.16). This step reveals an influence from the numerically definite syllogism—the point of departure of De Morgan's syllogistic theory—and the algebraical process of abstraction and generalization as in his theory of the abstract copula [6.5, (4); 6.7, (10), (11); see also 6.9]. The step (66.9), which we believe to had been a motivation for (68.15), had been conceived in 1850 in an attempt to amend and extend Hamilton's system of quantification (63.3) to (66.10). This fact, together with his statement in (8) above, further justify our claim that the whole of De Morgan's logic of relations, was conceived, grounded and developed strictly in harmony with the wider frame of his own logical system''.

We now proceed to mention and prove some of the theorems which followed as a consequence of the definitions (68.12)–

(68.19). The first three are but different verbal formulations of the same formula,

(68.20)  $\text{not-}L^{-1} = (\text{not-}L)^{-1}$ ;

(68.21) Contraries of converses are converses, or  $\text{not-}L$  and  $\text{not-}L^{-1}$  are converses,

(68.22) Converses of contraries are contraries, or  $L^{-1}$  and  $(\text{not-}L)^{-1}$  are contraries,

and

(68.23) The contrary of a converse is the converse of the contrary, or  $\text{not-}L^{-1} = (\text{not-}L)^{-1}$ .

De Morgan's proof of (68.21) runs as follows: " $X..LY$  and  $Y..L^{-1}X$  are identical [by (68.13)]; whence  $X..\text{not-}LY$  and  $Y..(\text{not-}L^{-1})Y$ , their simple denials, are identical; whence  $\text{not-}L$  and  $\text{not-}L^{-1}$  are converses [hence (68.20)]" [S<sub>4</sub>,223]. The proof of (68.22) can follow immediately from (68.20) and the same can be said for (68.23). We thus omit De Morgan's separate proofs as superfluous [S<sub>4</sub>,223].

The next important theorem is that

(68.24) The conversion of a compound relation converts both components and inverts their order, or  $(LM)^{-1} = M^{-1}L^{-1}$ .

De Morgan's verbal proof amounts to the following. According to (68.12) and (68.13) we have

$X..LY = LY..X = Y..L^{-1}X$ ; hence  $X..LMY = MY..L^{-1}X = Y..M^{-1}L^{-1}X$ . Thus  $(LM)^{-1} = M^{-1}L^{-1}$  [S<sub>4</sub>,223]. As an example of (68.24), if  $X$  be teacher of the child of  $Y$ ,  $Y$  is parent of the pupil of  $X$ . When inherent quantity is involved, (68.24) becomes

(68.25)  $(LM')^{-1} = M^{-1}, L^{-1}$ ;  $(L,M)^{-1} = M^{-1}L^{-1}$ .

For example, if  $X$  be teacher of every child of  $Y$ ,  $Y$  is parent of none but pupils of  $X$  [see (68.15)-(68.16); S<sub>4</sub>,223-4; Lewis 1918,46]. It was further proved that

(68.26) i)  $lM'$  and  $L,m$  are contraries of  $LM$

ii)  $M^{-1}, l^{-1}$  is the converse of the contrary of  $LM$

[S<sub>4</sub>,223-4] (10).

De Morgan formulated the following theorem "If a compound relation be contained in another relation [...] the same may be said when either component is converted, and the contrary of the other component and of the compound change places". In other words,

(68.27) If  $LM \supset N$ , then  $nM^{-1} \supset l$  and  $L^{-1}n \supset m$

[S<sub>4</sub>.224]. The proof, established upon non specified principles of inference, is hardly rigorous for contemporary standards. Nevertheless, it is the most formal demonstration in his logic, and thus worthy of notice. We will display it pointing out the principles which he implicitly used.

Let the canons of inference  $I_1$  and  $I_2$  be

$I_1$  If  $R \supset S$  and  $S \supset T$ , then  $R \supset T$

$I_2$  If  $R \supset S$ , then  $s \supset r$ .

Then the proof of the first part of (68.27) runs as follows:

If  $LM \supset N$ , then  $(n) \supset \text{not-}LM$  by  $I_2$ ; also  $\text{not-}LM \parallel LM'$  by i) of (68.26)  $(n) \supset LM'$  or  $nM^{-1} \supset LM'M^{-1}$  [(68.28)]. But an  $l$  of every  $M$  of an  $M^{-1}$  of  $Z$  must be an  $l$  of  $Z$   $(lM'M^{-1}) \supset l$  or (68.29)], hence [by  $I_1$ ] we have  $nM^{-1} \supset l$ .

The two principles hinted at in the course of his proof are instances of

(68.28) If  $R \supset S$ , then  $RT \supset ST$

and

(68.29)  $RS'S^{-1} \supset R$ ,

the latter verbally formulated. An example for the second principle would be that "If John loves every child of a parent of Mary, then John loves Mary". The second part of (68.27) can be proved in a similar way<sup>11</sup>.

In his Syllabus [1860b] De Morgan stated this theorem in the form "If two relations combine into what is contained in a third relation, then the converse of either of the two combined with the contrary of the third, in the same order, is contained in the contrary of the other two" [S,186]. He called (68.27) theorem K "in remembrance of the office of that letter in Baroko and Bokardo; it is the theorem on which the formation of what I called opponent syllogism is founded" [S<sub>4</sub>.224;S.186,fn 1].

Theorem K was the only characteristic instance from De Morgan's mature logic of relations -as presented in S<sub>4</sub>- to be referred to in other published work, such as S, and also in his correspondence. In both S<sub>4</sub> and S he provided examples to illustrate its use. Let L=master, M=parent and N=superior. Then (68.27) corresponds to the statements:

LM))N Every master of a parent is a superior,

nM<sup>-1</sup>))) Every inferior of a child is a non-master,

L<sup>-1</sup>n)))m Every servant of an inferior is a non-parent.

"From either of these the other two follow" [S,186-7], under the assumption that absolute equality is impossible<sup>12</sup>.

Providing simple constructions which evidently fail, De Morgan showed that "Identity, in theorem K. does not give identity". In other words,

(68.30) LM||N does not imply that nM<sup>-1</sup>||1 or L||NM<sup>-1</sup>.

For instance, "brother of parent is identical with uncle". But "non-uncle of child is non identical with non-brother", since it is not true "that every non-brother is non-uncle of child" [S<sub>4</sub>.225; see also (69.22)].

De Morgan proceeded to the examination of the properties of relations. "A relation is said to be convertible", he wrote, "when it is its own converse", i.e.

(68.31) when X..LY gives Y..LX.

He further claimed that "L being any relation whatever, LL<sup>-1</sup> is convertible: but LL<sup>-1</sup> and L,L<sup>-1</sup> are each the converse of the other. So far as I can see, every convertible relation can be reduced to the form LL<sup>-1</sup>[...]. But it cannot be proved that if X..LY and Y..LX, then L must be reducible to MM<sup>-1</sup>, for some meaning or other of M: this is certainly a material proposition. But I can find no case in which material proof fails" [S<sub>4</sub>.225-6].

It is argued that half of De Morgan's claim is right. That is, if L=MM<sup>-1</sup> then L is convertible, but not every convertible relation, such as "next to" is in fact reduced to the form MM<sup>-1</sup> [Merrill 1990, art.5.3]. When De Morgan goes on to say that "if all convertible relation can be expressed by LL<sup>-1</sup>X this is obviously necessary: for LL<sup>-1</sup> includes X" [S<sub>4</sub>.226], he is close at stating that every transitive and convertible relation is partially reflexive. It is puzzling that he did not develop the condition for convertibility

(68.32) L convertible  $\longleftrightarrow$  L||L<sup>-1</sup>

implied in (68.31), whereas he did develop more successfully the condition of transitivity,

(68.33) L transitive  $\longleftrightarrow$  LL))L

[S<sub>4</sub>.226; Merrill 1990, art.5.3].

Since  $L^{-1}L^{-1}$  is the converse of  $LL$  [according to (68.24)] then  $LL))L$  will give  $L^{-1}L^{-1}))L^{-1}$ , since "If a first relation be contained in a second, then the converse of the first is contained in the converse of the second" [ $S_4$ , 223, 227]. The latter was a typical statement devoid of proof [see (10) above]. Based upon these results, he claimed, we can obtain by contraposition and by theorem K further relational laws. We confine to mention the following: if  $L$  is transitive then

$$\begin{aligned} & L^{-1}L^{-1}))L^{-1} \\ (68.34) \quad & L))l^{-1}l' \\ & L^{-1}l))l \end{aligned}$$

where  $l = \text{not-}l'$ . Letting  $L = \text{ancestor}$  and  $L^{-1} = \text{descendant}$ , he illustrated these theorems. For example, the last in (68.34) corresponds to "Among non-ancestors are contained all descendants of non-ancestors" [ $S_4$ , 227; see also Hawkins 1979, 52-55; (10) above].

De Morgan devoted the second part of  $S_4$  to an application of his theorems to the syllogism. He considered in length the first case where the terms  $X, Y, Z$  are regarded as "individual notions, units of thought" [ $S_4$ , 227]. He recalled that in  $S_2$  he had enunciated "the identity of inference with combination of relation" [6.5, (7)]. He repeated that "any relation of  $X$  to  $Y$  compounded with any relation of  $Y$  to  $Z$  gives a relation of  $X$  to  $Z$ " feeling justified by the fact that his expression was found out in the mean time to be very close to Euclid's definition of compound ratio of magnitudes: "The ratio of  $X$  to  $Z$  is compounded of the ratios of  $X$  to  $Y$  and  $Y$  to  $Z$ " [ $S_4$ , 228]. Had he had "generalized the mathematical notion [ratio], from the Greek [ $\lambda\acute{o}\gamma\omicron\varsigma$ ], the process would have been both natural and valid", since " $\lambda\acute{o}\gamma\omicron\varsigma$ " means "communication" and can thus apply in "any way in which we talk about one notion in terms of another". So, much more confident in 1860 than a decade earlier, De Morgan asserted

Any way of speaking of one notion with respect to a second, joined with a way of speaking of the second notion with respect to a third, must dictate a way of speaking of the first notion with respect to the third. And this is syllogism: it exhibits, in the most general form, the law of thought which connects two notions by their connexions with a third<sup>(15)</sup>

Omitting purely logical analysis concerning the issues of the middle term, negative premises or figures, let us record <sup>a</sup>few samples from his table <sup>u</sup>of the instances of unit-relational syllogism. We have

	First figure	Second figure
(68.35)	X..LY Y..MZ	X..LY Z..MY [-Y..M <sup>-1</sup> Z]
	<hr/> X..LMZ	<hr/> X..LM <sup>-1</sup> Z.

the first provided in 1850 [see (68.1)]. For more complicated instances, theorems of quantified relations can be used. For example, we have

	X.LY [X..LY]
(68.36)	Z..MY [Y..M <sup>-1</sup> Z]
	<hr/> X..LM <sup>-1</sup> Z

the conclusions stated equally well negatively via (68.26) as X.LM<sup>-1</sup>Z or X.l,m<sup>-1</sup>Z [S<sub>4</sub>,229-233;Merrill 1990,art.5.4]. The reader can notice the formal character of these inferences [see discussion in 6.7].

De Morgan went on to consider the "quantified proposition and its syllogism" [S<sub>4</sub>,234], denoting "Every X is an L of one or more Ys" by "(X))LY", "Every L of any Y or Ys is an X" by "(LY))X" and so on, based on his standard syllogistic notation introduced in S<sub>2</sub> [see (66.7)]. Two examples of quantified syllogism would thus be

	X).(LY	X))LY
(68.37)	Y((MZ	and MZ)).z
	<hr/> X).(LM <sup>-1</sup> Z	<hr/> X)).L.M <sup>-1</sup> z

[S<sub>4</sub>,235-6]. However, this aspect of relational syllogism was not *developed* any further. He claimed that "The whole of the system of relations of quantity remains undisturbed if for the common copula "is" be substituted any other relation: so that the usual laws of quantity may be applied to the table of unit-syllogisms given above, precisely as if L and M only meant is" [S<sub>4</sub>,235].

In other words, a detailed treatment of quantified syllogism was omitted since the new forms of inference, such as (68.37),

were claimed to be contained in the logic of unit syllogism, the latter being sufficient for traditional logic. De Morgan's claim, thoroughly considered by Merrill [1990,art.5.5], is far from simple to illustrate. We will confine to say that De Morgan on purpose avoided to "enter completely upon quantified forms" restricting his brief analysis "to what may be called the Aristotelian branch of the extended subject", out of fear that a complete investigation would require the study of numerous, complex forms. For "There is no more limit to the formulæ of thought than to the formulæ of algebra". [S4.234-235].

We have no manuscript or published indication that De Morgan ever attempted to pursue the logic of relations any further. The two most probable reasons why he abandoned this project were on the one hand his concentration on the part of the logic of relations necessary for the theory of the syllogism, and on the other hand the complexity of the task as such<sup>(14)</sup>.

Before drawing his final conclusions by comparing ordinary syllogism with relational syllogism, he considered unit syllogisms in which only one transitive relation  $L$  appears in its premises and its converse  $L^{-1}$ . We thus have, for example,

X..LY  
(68.38) Y..LZ

---

X..LLZ=X..LZ [since LL))L]

[S4.136-7; Merrill 1990,art.5.6;6.9].

"When by the word syllogism", wrote De Morgan at the end of his paper, "we agree to mean a composition of two relations into one" we open the field of logic in a big way. The chance is small that connecting two notions at hazard we find out that "they are related by inclusion or exclusion, total or partial". In other words, "onymatic" or "traditional" syllogism rarely occurs in practice; much more frequent is the combination of relations, and "the introduction of composition of terms and transformation of propositions by far the most frequent of all". Further, he claimed, "syllogisms are rather chapters than sentences, in many cases [...] Nothing that I know of can be written all in syllogism, except mathematics; and this merely because, out of mathematics, nearly all the writing is spent in loading the syllogism, and very little in firing it" [S4.238-9].

6.9 De Morgan's logic of relations viewed through his treatise on functions (1836): late views on logic and mathematics: 1860-1865.

De Morgan was the first logician to argue so amply on the inadequacy of traditional logic and to view it as a particular case of his logic of relations. The significance of relations was raised by him in 1860:

I hold the combination of relations to be the actual organ of reasoning of the world at large, and, as such, worthy of having its analysis made a part of advanced education; the logician's abacus [ordinary syllogism] being a fit and desirable occupation for childhood<sup>(1)</sup>.

De Morgan's contributions in founding the logic of relations are regarded as "indispensable and of permanent value" (Lewis 1918, 50). Nevertheless, he can not be granted the title of creator of the modern theory of relations "since he did not possess an adequate apparatus for treating" this subject (Teraki 1941, 73; chap. 9).

It is true that De Morgan left his relation theory in an unclear and unfinished state. Studying his work relative to his own standards in technicalities and rigor, and focusing on the mathematical aspects which characterize it, we discern two striking omissions. He had always claimed that logic is indispensable in mathematics (3.4, (14); 3.9; 6.4, (8); 6.7]. Moreover, in 1858 he had envisaged the rise of a "mathematical logic", ardently believing in the mutual benefit of the two sciences. [6.7, (14)]. However, there is not the slightest attempt in his writings from 1858 onwards, either to examine the utility of the logic of relations in mathematics or to specify how much of actual mathematics can be included in logic. Was De Morgan indeed interested in carrying out these concerns? Which were the obstacles he might have encountered? Finally, how much did he draw on his early work on functions in his preparation for his calculus of relations in 1860?

In order to properly evaluate De Morgan's work and understand its individuality we will adopt his own historical at-



titude. We will thus compare first his methodology in [1836] with that followed in his writings between 1850 and 1860, focusing on  $S_4$ . After revealing some interesting similarities and contrasts between his early mathematical background and his logic of relations, we will proceed to a brief study of his mature concerns and views on logic and algebra up to the mid 1860's. Becoming thus familiar with the extent to which mathematics and logic were intertwined in his work we will be able to understand some of the factors that impeded the further development of either sciences.

Let us see first how much of functional notation and concepts was actually employed in  $S_2$ ,  $S_3$  and  $S_4$ . Drawing an analogy between elimination in algebra and inference in logic, De Morgan claimed that the process of algebraic elimination, contrary to logical deductions, is reversible. To illustrate this claim he wrote: "if  $y=\phi x$ ,  $z=\psi x$  give  $z=\chi y$ , then  $z=\psi x$  and  $z=\chi y$  always give  $y=\phi x$  as one at least of alternatives" [ $S_2$ , 27-8]. We record this instance as the first one in which functions appeared heuristically in De Morgan's logical writings, and as a proof that he had not read Boole [1847a] seriously -as the latter had refuted the reversibility of the elimination process in algebra<sup>(2)</sup>.

Let us proceed now to the issues of composition of relations and converse relations as primarily conceived in  $S_2$  and further discussed in  $S_3$ . In an attempt to reduce bicopular to unicopular syllogism he wrote:

The algebraical equation  $y=\phi x$  has the copula  $=$ , relatively to  $y$  and  $\phi x$ ; but relatively to  $y$  and  $x$  the copula is  $\rightarrow\phi$ . This is precisely the distinction of "John can persuade Thomas" and "John is (one who can persuade Thomas.)" The deduction of  $y=\phi\psi z$  from  $y=\phi x$ ,  $x=\psi z$  is the formation of the composite copula  $\rightarrow\phi\psi$ . And thus may be seen the analogy by which the instrumental part of inference may be described as the elimination of a term by composition of relations. For though in ordinary inference the concluding copula is usually identical with those premised, yet it is no less true that the composition must have taken place:  $X$  is  $Y$ ,  $Y$  is  $Z$ , therefore  $X$  is that which is ( $=$ is)  $Z$ <sup>(3)</sup>.

The very idea of "combination of relations", as "the actual organ of reasoning" [(1) above], seems to be explicitly borrowed

from De Morgan's treatise on functions. This is partly revealed in (3) above, and becomes much clearer in 1858 when he stressed that, if two relations are denoted by the letters A and B, then the compound relation [...] may be represented by AB, but by no one except a mathematician who is used to the functional symbol, and to the idea of  $\phi\psi(xy)$  and its distinction between the mode of composition of x,y, and that of  $\phi,\psi$ ["].

While, as we argued in 6.7, most of De Morgan's references to algebra served as a means to raise interesting analogies to illustrate and reinforce his logical procedures, the quotation above is among the few instances where he explicitly hints at the influence derived from his mathematical background in the shaping of his logic of relations. In fact, it is the unique instance where he alludes to the calculus of functions with two variables [see 3.8].

We saw in (3) above that the notion of composition of relations arose from the procedure of elimination in the bicopular syllogism (65.12). In fact, the notion of a relation, as well as that of the converse of relation, arose for the first time in  $S_2$  out of an inquiry into the connection of the doctrine of figure with that of the copula. And while the concept of relation and of composition of relations had a functional origin, that of the couple of a direct and inverse relation seem to have had a rather algebraic origin. We recall that in the opening of  $S_2$  De Morgan had claimed that the notion of contrary terms had led him to conceive logic as a "calculus of opposite relations, for mere inference, as general as that of  $+$  and  $-$  in algebra" [ $S_2$ , 26; 6.7.(5)]. Stressing his study of logical oppositions, he wrote below in  $S_2$ :

The logician, followed by the algebraist, has restricted himself to one copula: the former uses is, the latter  $=$ ; and both are used in some variety of sense. The algebraist, indeed, sometimes goes a little further, and introduces the correlatives  $>$  and  $<$ , which might be generalized for the purposes of logic into symbols of correlative copula in general["].

If we stop for a moment and combine the two citations from

$S_2$  [(3) and (5) above] with the notation given later in  $S_4$ , we may formulate the following set of correspondents;

	(i)	$x = \phi y$	$X..LY$
	(ii)	$x \neq \phi y$	$X..LY$
(69.1)	(iii)	$x < y$	$X..LY$
	(iv)	$y > x$	$Y..L^{-1}X$
	(v)	$y = \phi^{-1}x$	$Y..L^{-1}X$

We believe that the analogy (v) was in fact conceived only later in  $S_2$  when De Morgan first speculated on the link between  $\phi^{-1}$  and  $L^{-1}$  [see text below (69.3)]. In other words, the origin of the converse of a relation was initially algebraic, much as was the origin of numerical syllogism and its consequence, the system of contrary terms, in 1847 [see 6.6, (3), (6), (8), (13)].

Let us now return to  $S_2$  and examine the introduction of the converse of a relation. At this instance we will need the four figures, i.e. the four different arrangements of the subject X, the predicate Z and the middle terms Y of an inference, as given in [1839,20]:

	1	2	3	4
	Y Z	Z Y	Y Z	Z Y
(69.2)	X Y	X Y	Y X	Y X
	<hr/>	<hr/>	<hr/>	<hr/>
	X Z	X Z	X Z	X Z

We recall that as long as we deal with unidcopular syllogism, transitivity is "the condition of permanence of the copula" [ $S_2$ , 53; 6.5, (5)]. Under this principle we can obtain inferences from the first three figures. If we take, however, the transitive but inconvertible copula "to give", then the fourth figure is "incapable of inference" [ $S_2$ , 54].

In order to extend the realms of traditional logic, De Morgan tried to get rid of the restrictions imposed by the transitivity and convertibility of the copula. For the first restriction to be dropped it sufficed to regard Y,Z and X,Y<sub>A</sub><sup>as</sup> connected with two arbitrary relations, inference becoming possible by composition of relations [ $S_2$ , 55; see (65.11) and (68.1)]. For the second restriction it suffices to admit that "a direct and inverse relation always exist" [ $S_2$ , 64]. If we have the copula "has relation to", then its correlative [see (5) above] "may be expressed by has correlation to". Thus, "receives from" is the correlative of "gives to", also called as the "inverted" or the

"converse" relation [S<sub>2</sub>,63-64; see also (68.6)]. Via this new concept "the reading by figure is a matter of indifference" [S<sub>2</sub>,65] (\*).

This matter was further discussed in S<sub>3</sub>. De Morgan assumed the valid inference  $((.) = (.($  [on this notation see (66.11)-(66.13)]. Supplying terms X,Y,Z we can read it as an ordinary syllogism "Some Xs and Ys, no Y is Z; therefore some Xs are not Zs". But after the distinction (67.5) we can read it "mathematically", i.e., as relation between classes: "the compar-tient of an external is extient" [S<sub>3</sub>,134]. The general form of such a syllogism is

(69.3) "Every A of B is a C".

There are thirty-two truths of this form; another thirty-two forms are derivable from (69.3) as follows:

(69.4) "Every A is a C of every (converse of B)".

A particular instance of (69.3) is "Every complement of every species is a complement"  $((.))) = (.))$ . The same syllogism can ac-cording to (69.4) be read as "every complement is a complement of every genus" [S<sub>3</sub>,134], "genus" and "species" being regarded as "converse relations" (\*).

This new way of reading the form (69.3) enabled De Morgan to give a symbolic rule of inference for the fourth figure [S<sub>3</sub>,134-5]. While the first figure was "the most simple expression of the combination of relations" [S<sub>3</sub>,131; see (68.2)], the fourth figure proved to be "nothing but the first with its conclusion read backwards" [S<sub>3</sub>,137]. Take, for example two instances from [S<sub>4</sub>,232]

$\begin{array}{r} 1 \\ X..LY \\ Y..MZ \\ \hline X..LMZ \end{array}$	$\begin{array}{r} 4 \\ Y..LX \\ Z..MY \\ \hline X..L^{-1}M^{-1}Z \end{array}$
---	---

(69.5)

Thus, problems concerning the fourth figure urged De Morgan to introduce the converse of a relation in 1850, enabling him thus to form the syllogism (69.4) in 1858, and finally the formal in-ference (69.5) in 1860.

How did he derive (69.4) from (69.3)? As a footnote indi-cated, he drew directly from [1836] by writing

If  $\phi\psi x < \psi x$  for all values of x, which is the proper analogy for the composition of relations in the syllogism, then  $\phi x < \psi\psi^{-1}x$ , but

we must not say  $\psi x < \phi^{-1}\chi x^{(6)}$ .

According to De Morgan's notation of inclusion (68.17) in  $S_4$ , it follows from the above quotation that from

$$(69.6) \quad AB \supset C$$

we can deduce

$$(69.7) \quad A \supset CB^{-1}$$

but not

$$(69.8) \quad B \supset A^{-1}C.$$

In fact, (69.6) is read as (69.3), but the proper symbolical equivalent of (69.4) should be written as

$$(69.9) \quad A \supset CB^{-1},$$

where  $CB^{-1}$  is read as "a C of every B" [see (68.15) and (68.25)].

In  $S_3$  De Morgan distinguished between (69.7) and (69.8) by consulting his study of inverse functions according to which:

$$\phi\psi x = \chi x \longrightarrow \phi\psi\psi^{-1}x = \chi\psi^{-1}x \longrightarrow \phi x = \chi\psi^{-1}x, \text{ since}$$

$$(69.10) \quad \psi\psi^{-1}x = x$$

for every inverse of  $\psi$ . Thus the inference (69.6)  $\longrightarrow$  (69.7) is admissible. However, we can not admit  $\phi\psi x = \chi x \longrightarrow \phi^{-1}\phi\psi x = \phi^{-1}\chi x \longrightarrow \psi x = \phi^{-1}\chi x$ , or (69.6)  $\longrightarrow$  (69.8), since

$$(69.11) \quad \psi^{-1}\psi x = x$$

does not always hold true [see (36.23)-(36.24)]. As we shall see below, this restriction was to be dropped in  $S_4$ .

De Morgan stressed in the latter memoir: "I do not use the mathematical symbols of functional relation,  $\phi, \psi, \dots$ : there are more reasons than one why mathematical examples are not well suited for illustration" [ $S_4$ , 220]. Mathematical symbols were fiercely objected to by Mansel [6.7.(2), (15)-(17)] in 1851. Moreover, logic, had stressed De Morgan in both  $S_2$  and  $S_3$ , should develop, like algebra, its own peculiar symbols [6.7.(17)]. However, the most plausible reason why De Morgan avoided functional notation in  $S_4$  seems to be the fact that he had perceived the wider scope of the calculus of relations in comparison with that of functions, and thus he put forward a more general notation.

Despite his claim, there were two instances in  $S_4$  where he used functional notation for illustration. He mentioned fallacies often committed by educated people [6.8.(4)]. In a footnote he

wrote that once a nun, when asked about a gentleman who visited her, said: "his mother was my mother's only child". People concluded often that this gentleman was her grand child or grandfather, thus confusing the right answer " $\varphi\varphi^{-1}$ " [child] with " $\varphi^{-1}\varphi^{-1}$ " or " $\varphi\varphi$ " [S<sub>4</sub>, 213, fn 1]. And further below when he discussed transitive relations he wrote:

The mathematician forces the predicate itself among its own chain of successive relatives, whether the relation be transitive or not: x, as  $\varphi^0x$ , appears in the sequence ...  $\varphi^{-2}x, \varphi^{-1}x, \varphi^0x, \varphi^1x, \varphi^2x, \dots$

There is a little tendency towards the same thing in ordinary language, especially when the relation is transitive. Milton, in calling Eve "the fairest of her daughters", meaning female descendants in general, allowed  $\varphi^0x$  to be a case of  $\varphi^n x$  ("").

Up to this point we hinted amply at the algebraic and functional features that characterized De Morgan's logic of relations based exclusively upon his own pertinent statements. Focusing now on S<sub>4</sub>, let us compare it with his treatise on functions [1836] in order to examine the latter's degree of influence in the shaping of the former. De Morgan carefully separated relation from judgment in his notation (68.12).

(69.12) X..LY,

drawing apparently from the possibility to separate functional from quantity symbols (see (3) above, (69.1), and (69.14)). Thus relations like functions have a basic ontological similarity being regarded as individual objects as in  $LL=L$  or  $L))M$  and  $(\varphi+\psi)(x)=\varphi(x)+\psi(x)$  [(36.13)] or  $\varphi=\psi$  [(37.48)] respectively.

We can also correlate the triad "subject - operation - result" with "subject - copula - predicate" as they both make up a proposition, adding also in the list (69.1) the analogy

(69.13)  $\varphi(x,y)=0$  XLY or L(X,Y).

We could further produce the correspondents

	(i)	$=\varphi$	..L
	(ii)	$\neq\varphi$	.L
	(iii)	$(-)(-) = +$	$(.)(.) = ..$
(69.14)	(iv)	$\varphi(x) < \psi(x)$	LX )) MY
	(v)	$a < b$	L )) M
	(vi)	$\varphi = \psi$	L    M
	(vii)	$\varphi\psi$	LM <sup>(10)</sup>

The symbols  $LM'$  and  $L.M$  appear to have no equivalent in the calculus of functions. Nevertheless, the process of separation of quantity from the terms and its attachment to the relation itself—as in (68.15)—hints at the process of separation of symbols. For De Morgan "quantification itself only expresses a relation" ( $S_4$ , 234); thus, in a way,  $M'$  is a composite relation, or "' can be viewed as an operation upon  $M$ , much as  $D=d/dx$  operated upon  $\phi(x)$  produces  $\phi'(x)$ .

Taking now under account the notion of the converse relation, our comparative study becomes more intricate. To start with,  $L^{-1}$  was definitely so denoted after  $\phi^{-1}$  [see (8), and (69.1) above and  $S_4$ , 222]. De Morgan called the former "converse" and the latter "inverse". In [1836, art.144] he called " $\phi\psi$ " the "converse" of " $\psi\phi$ ", calling attention to the distinction between "converse" and "inverse", claiming that the latter term "it might be more logical to call the contrary but custom has settled its present meaning". The property of inverse functions

$$(69.15) \quad (\phi\psi x)^{-1} = \psi^{-1}\phi^{-1}x$$

was absent in De Morgan's foundational study of the calculus of functions. However, he alluded to it by calling  $\psi\phi$  the converse of  $\phi\psi$ , and he did formulate it further below in his treatise only for inconvertible inverses [see (69.27)]. So, it was mostly probable that he had (69.15) in mind when he proved in  $S_4$  that (68.24) or

$$(69.16) \quad (LM)^{-1} = M^{-1}L^{-1} \quad \langle 11 \rangle.$$

What about the equivalent to the functional properties (69.10), (69.11) or the uniqueness of  $L^{-1}$ ? De Morgan's account in  $S_4$  is very confusing to provide us with direct answers, but there are still few interesting observations to be made. After proving theorem K (68.27) he rightly claimed that "Identify, in Theorem K, does not give identity" [ $S_4$ , 225], in other words

$$(69.17) \quad LM = N \longrightarrow (nM^{-1} = 1 \text{ and } L^{-1}n = m)$$

is not a true inference [Theorem K is (69.17) if "=" is replaced by ")]". For justification of his claim he suggested to look at the demonstration in which he had implicitly used the axiom (68.29)

$$(69.18) \quad R(S'S^{-1}) \rightarrow R.$$

To obtain identity —(69.17)— he would have to assume also that

(69.19)  $(R)) R(S'S^{-1})$ .

The latter is a strong claim; it amounts to the acceptance of the inference "if John loves Mary, then Mary has a child and John loves every parent of that child", which is obviously wrong [6.8, (11)].

De Morgan accepted though the inferences

(69.20)  $LM \parallel N \longrightarrow (LMM^{-1} \parallel NM^{-1} \text{ and } L^{-1}LM \parallel L^{-1}N)$

and holding that

(69.21) " $MM^{-1}X$  and  $L^{-1}LX$  are classes which contain  $X$ ".

he deduced

(69.22)  $LM = N \longrightarrow (L)) NM^{-1} \text{ and } (M)) L^{-1}N$ ,

stressing that identity is not allowed in the right-hand side of

(69.22) [ $S_4$ , 225]. The statement (69.21) amounts to the principles

(69.23)  $R)) R(SS^{-1})$  and  $R)) (S^{-1}S)R$ .

For the first to hold true it is required that

(69.24) If  $X..RY$  then there exists an  $S$ , such that  $Y..SS^{-1}Y$ ,

or, in the case of the example above given, that Mary stands in the relation (Parent - child) to herself. This is again a strong claim which restricts the universe of discourse to one in which every member stands in every relation to some member or other [Merrill 1990, art.8.5]. According to Merrill, only  $(LM))N$  was needed for the result (69.22), which he showed to hold true only under an additional condition missing in De Morgan's system [1990, art.8.5, 8.9]. Moreover, he said that De Morgan could have deduced directly from theorem K the inferences

(69.25)  $(LM))N \longrightarrow (L))NM^{-1} \text{ and } (M))L^{-1}N$ .

The missing condition which Merrill provided so that (69.22) is a valid inference was that  $S$  in (69.23) satisfies the existential condition as briefly mentioned above. Perhaps De Morgan had implied this condition when he wrote in  $S_2$  that "a direct and inverted relation always exist" (see (6) above). Merrill and De Morgan seem to have overlooked the fact that the first inference in (69.22) was in fact formulated in  $S_3$  in the form (69.7) - or rather in the form (69.9), which is identical to the first part of (69.25). Moreover, the fact that De Morgan had then denied the possibility to deduce the second inference in (69.22) (see (69.6) - (69.8) and (8) above) is absent in both Merrill [1990] and  $S_4$ .

According to Merrill, De Morgan might have been led astray in



formulating (69.22) by the notation of inverse functions. While in general we agree with his remark, since we try to estimate the overall influence of the calculus of functions on the logic of relations, we would like to draw the reader's attention to a delicate point: that De Morgan was more vividly under a functional influence in  $S_3$  [(8) above] than in  $S_4$ . Discussing the impossibility to infer identity from  $LM=N$  in (69.22), he appealed to several logical examples, distinguishing next between the "much power" mathematics afford in "forcing  $NM^{-1}||L$  out of  $LM||N$ ", and the less definite character of the logic of relations, in which—as he had stressed in the opening of  $S_4$ —mathematical examples are not suitable for illustration [ $S_4$ , 225.220; see text below (69.11)]. As already seen, and will be further shown below, De Morgan was on the whole definitely under a general (almost subconscious) functional influence while constructing his logic of relations in  $S_4$ , but at the same time he consciously avoided to apply specific functional properties and procedures.

The kind of arguments displayed above in connection with the slightly problematic inference (69.22) can also apply to De Morgan's statement summarized in:

(69.26) For every convertible relation  $M$  there is an  $L$  such that  $M = LL^{-1}$  and  $LL^{-1}X$  includes  $X$

[ $S_4$ , 225-6]. A proper inquiry into (69.26) demands a reformulation of his system in contemporary standards and is omitted here<sup>(12)</sup>. However we would like to hint at a subtle tentative analogy perceived between the form " $LL^{-1}$ " and the form " $\phi\alpha\phi^{-1}$ "—properly written as " $\phi\alpha\phi^{-1}x$ " which featured constantly in De Morgan's treatise [1836]. The urge to conceive of the identity " $M=LL^{-1}$ " could be perhaps explained by the problem par excellence in [1836] of how to find an  $\phi$  such that given  $\beta$  and  $\alpha$ ,  $\beta x = \phi\alpha\phi^{-1}x$  (see (37.4), (37.23)). We add that under certain conditions, if  $\psi x = \phi\alpha\phi^{-1}x$ , then  $\psi x = F(F^{-1}x-1)$  (where  $Fx$  the exponential function [see (37.65); 3.7, (9)]), in other words the actual form " $FF^{-1}$ " was very closely used<sup>(13)</sup>.

Reassuming our initial questions concerning the properties (69.10), (69.11) and (69.15) we can make the following observations: Confining himself to syllogisms in  $L$  and  $L^{-1}$  only, where  $L$

was transitive. De Morgan wrote that  $X..LZ$  and  $Y..L^{-1}Z$  give  $X..LL^{-1}Z$ , "not necessarily either  $LZ$  or  $L^{-1}Z$ , though possibly either". He added that still " $X..LL^{-1}Z$ " "is a conclusion, and to some persons an important one: if  $L$  mean descendent, and therefore  $L^{-1}$  ancestor, then,  $Z$  being the Queen,  $X$  is entitled to an honorary degree". [S<sub>4</sub>, 237]. If we take  $L$  to mean "only child of female parent" then  $X$  is the Queen, i.e.  $LL^{-1}$  becomes the identity relation and  $LL^{-1}Z=Z$  according to the functional property (69.10) which holds for all inverses of a function  $\phi$ . But, surprisingly enough the identity relation is not made explicit in S<sub>4</sub> [see (17) below].

Due to the wider scope of the calculus of relations when compared with that of functions, De Morgan seems to have viewed converse relations in analogy with inconvertible, rather, than with convertible inverses - the latter uniquely defined by both (69.10)-(69.11). Inconvertible inverses of a function  $\phi x$  - denoted by  $\phi_{-1}x$  - played a rather marginal role in the core of his methodology in [1836], and we thus omitted a study of them in chapter 3 [see 3.6; (13)]. However, we have spotted an interesting instance at the end:

We shall here proceed to consider the manner in which the inconvertible inverses of a function must be used in connection with the direct function, when we take into consideration direct functions with more values than one<sup>(14)</sup>.

Among the cases studied was the following: "If we assume  $\chi x = \phi\psi x$ , then  $\chi_{-1}x = \psi_{-1}\phi_{-1}x$  and any form of  $\psi_{-1}$ , applied to any form of  $\phi_{-1}$  gives one of the forms of  $\chi_{-1}$ " [1836, art. 244]. We thus notice that the property (69.15) appears in the form

$$(69.27) \quad (\phi\psi x)^{-1} = \psi_{-1}\phi_{-1}x$$

in [1836] only in connection with inconvertible inverses.

This instance hints at the tentative conjecture that De Morgan viewed relations as "direct functions with more values than one" and the converse of a relation as inconvertible inverse. The omission of any discussion of the identity relation and of the uniqueness of  $L^{-1}$  is striking in S<sub>4</sub>. However, there is an interesting footnote added in 1864 in connection with convertible and

transitive relations which reveals his later reconsideration of converse relations -perhaps under the light of his treatise of functions. The passage runs as follows:

When  $M^{-1}$  has only one  $M$ , write it  $\dot{M}^{-1}$ . Then  $\dot{M}\dot{M}^{-1}X$  is  $X$ , and  $\dot{M}^{-1}MX$  maybe other than  $X$ , but is transitive. For  $\dot{M}^{-1}\dot{M}\dot{M}^{-1}MX$  is  $\dot{M}^{-1}(\dot{M}\dot{M}^{-1})MX$ , or  $\dot{M}^{-1}MX$ . Are all transitive and convertible relations reducible to  $\dot{M}^{-1}M$ ? (13)

Whereas  $\psi^{-1}$  need not be the unique inverse of  $\psi$  so as  $\psi\psi^{-1}x=x$  holds true, in the calculus of relations this restriction is necessary due to the extreme vagueness and generality of the conception of a relation. The logical character of inverse questions, "A always gives B: what gives B?" (1849c, 93, fn) had occupied De Morgan in his work on algebra a decade before the construction of  $S_4$  [see 3.9 below (11)]. It is thus surprising that a systematic approach towards the logical, in the sense of foundational, character of conversion had escaped his notice in  $S_4$  (14).

Other striking omissions hint at the fact that  $S_4$  was apparently rather hastily written, though not altogether forgotten later on -according to the passage in (15) above. We observe a total absence of commutative and periodic relations -supplied by Venn two decades later. From the definition of convertible relations (68.31) it is implied that  $L \parallel L^{-1}$  or  $L=L^{-1}$ . The identity  $L=L^{-1}$  could have served in order to provide properties of convertible relations missing in  $S_4$ . If we draw on [1836] we have that  $\phi x = \phi^{-1}x \longrightarrow \phi^2 x = \phi\phi^{-1}x = x \longrightarrow \phi$  periodic of order 2 [see (36.27)]. If we confine now to convertible relations with unique inverses then we have  $L^2 = LL^{-1} = I [=L^0]$ , or  $L$  is periodic of order 2. As an example of such a relation, take "contradiction of". De Morgan was very close to such examples in [1836] when he claimed that just as  $-(-a)=a$ , then "discontinuous of discontinuous is continuous". [see (36.5)]. And while he did refer to successive functions  $\phi x$ ,  $\phi^2 x$ , ... in  $S_4$  [see (9) above], he avoided to follow even a similar notation for transitive relations and to write  $LLL$  as  $L^3$  or  $L^0$  as  $I$  (17).

Borrowing some foundational notions and procedures from algebra -the latter taken in its wider sense encompassing instances

from the calculus of functions- De Morgan viewed logic as a magical cauldron of oppositions. When he claimed in 1858 to set this cauldron boiling "at some future time" [6.7.(12)], he might had meant its extended version of the logic of relations where the two basic sets of relational oppositions were  $L, I$  and  $L, L^{-1}$  [see 6.8]. We have amply covered so far the genesis and development of this logic of relational oppositions stressing the influential role of his algebraic background. Due to his messy writings it is not always easy to sharply distinguish between mathematical examples which served merely for illustration of logical procedures and those which served as paradigmatic cases for imitation. Nor can we always be accurate in the distinction between purely arithmetical, algebraic or functional influences, as certain analogies drawn so far hint at a both algebraic and functional origin of certain logical notions and processes. For example, elimination in syllogism apparently was based on composition of relations

$$\begin{array}{rcl}
 (69.28) & \begin{array}{l} x = \varphi(z) \\ z = \psi(y) \end{array} & \begin{array}{l} X..LY \\ Y..MZ \end{array} \\
 & \hline & \hline
 & x = \varphi\psi(y) & X..LMZ
 \end{array}$$

following the example of "algebraic equations" [see (3) above]. However, we can not omit to mention the extended application of the process of elimination in the realms of "functional equations" in [1836] (18).

We may draw the following sketch which indicates a similarity in role between mathematical and logical structures in his work;

	Ordinary arithmetic	Unit syllogism
	Generalized arithmetic	Numerical syllogism
	Algebra	Arithmetical system
(69.29)	Differential calculus	Mathematical logic
	Calculus of functions	Relational syllogism
	Calculus of operations	Quantified relational syllogism

The key notion in this scale of gradual abstraction and generalization in mathematics is initially Carnot's distinction of the degrees of indeterminateness which characterize algebraic symbols, introduced in 1794 after Lagrange [1.8.(5); 2.9]. Argogast provided mathematicians with the method of separation of symbols, which, together with Carnot's issue amounted to a first

distinction between form and matter in De Morgan's treatise [1836]. This distinction was so called by De Morgan only later on in his work on logic, enabling him to extend the copula "is" to "has relation to" and to present a scale of gradual generalization in the realms of logic [see 6.4, (9); 6.7, (10), (17), (67.2)].

The form-matter issue -hereafter cited as FM- was to be the crucial link between mathematics (always taken to mean algebra in its widest sense) and logic in De Morgan's writings in the 1860's. In  $S_4$  he claimed that the transition "from  $X..LY$  to  $Y..L^{-1}X$ " is a form of thought, and a more general form than any other case of conversion admitted by the logician in the common syllogism. It is that which is common to the transitions: " $X$  a genus of  $Y$ , therefore  $Y$  a species of  $X$ ", " $X$  a parent of  $Y$ , therefore  $Y$  a child of  $X$ " [...], and is therefore more abstract than any of them, and equally form without matter to all of them" ( $S_4$ , 230, fn 1). We recall that De Morgan was accused of introducing material elements in his logic with the theory of the copula in FL [6.7, (2), (9); (67.1)]. Nevertheless, in  $S_4$  he reached the highest point of formalization, since most of the inferences there displayed were purely formal. Further discussing the FM issue he wrote in [ $S_4$ , 241]

It is to algebra that we must look for the most habitual use of logical forms. Not that onymatic relations are found in frequent occurrence: but so soon as the syllogism is considered under the aspect of combination of relations, it becomes clear that there is more of syllogism, and more of its variety, in algebra than in any other subject whatever, though the matter of relations -pure quantity- is itself a small variety. And here [Algebra?] the general idea of relation emerges, and for the first time in the history of knowledge, the notions of relation and relation of relation are symbolized. And here again is seen the scale of gradations of form, the manner in which what is difference of form at one step of the ascent, is difference of matter at the next<sup>19</sup>.

The few historians to comment upon this passage take it for granted that "here" stands for  $S_4$  [see (19) above]. But we had evidence that neither the notion of relations and of their combination, nor the FM issue were in any way first introduced in

S<sub>4</sub>; all the basic concepts and principles were present in S<sub>2</sub> [1850] and were further developed in S<sub>3</sub>. Composition of functions and the application of the FM issue were already familiar to De Morgan by 1836. We are thus led to interpret "here" as "algebra" and to thus confirm that the roots of his logic of relations were of an algebraic-functional origin, no matter if De Morgan is not as explicit about it in S<sub>4</sub> as he was in S<sub>3</sub> [see (8) above; 6.7.(10)].

For all his mathematical influence, De Morgan omitted some obvious analogies between functional and relational properties in S<sub>4</sub> [see text and (16)-(17) above]. Moreover, there is no evidence of any consideration of the mutual utility of mathematics and logic, as boldly but vaguely suggested in S<sub>3</sub> [6.7.(14)]. How far was De Morgan eager and capable to apply algebra to logic and vice-versa? Few fragments from his late writings hint at a tentative, though not very illuminating, answer. We will end our account by commenting upon these instances, reminding the reader of his singular attitude towards the evolution of science which partly explains certain of his striking omissions.

De Morgan started his career in the 1830's occupied by foundational and educational concerns and somehow aware of his limited power as an original researcher. Very characteristically he claimed in [1836, art.1; see 3.5.(3)] that he viewed his role as that of a "colonist" rather than of an "invader". He thus undertook a confusing systematization and unification of the calculus of functions, often posing open questions and suggestions for the benefit of the researcher. Historically minded, he would accept ambiguity and confusion, optimistic in that any errors, unrigorous procedures or tentative analogies noted in his work would be soon clarified by his followers. His motto in general is encapsulated in the following statement put forward in S<sub>3</sub>:

We go as far as we can, and we try to see what we can: to ask a question is a step in knowledge, and even if there be no answer it is a preparation for an answer<sup>(20)</sup>.

In connection with his limited pursuit of links between algebra and logic from 1858 onwards, he wrote in S<sub>4</sub>: "But the relation of algebra to the higher developments of logic is a subject of far

too great extent to be treated here" (S<sub>4</sub>,241). Apparently, De Morgan had perceived a further proximity between the two sciences than that discussed in S<sub>2</sub>, but he must have felt unable to cope with it due to the complexity of the task and his poor technical machinery.

De Morgan had lamented the antipathy between Cambridge and Oxford since 1835 believing that the isolated study of mathematics and logic was an impediment in the instruction and evolution of either sciences [see 3.5; 6.7.(4); S<sub>2</sub>,26; S,184]. In S<sub>3</sub> he appealed prophetically to a "mathematical logic" as a result of a cooperation between the two Universities in the future, [6.7.(14)], and in S<sub>4</sub> he concluded his memoir by saying that this antipathy "will go on for a time, and for a time only" (S<sub>4</sub>,241-2). Had De Morgan had viewed "mathematical logic" as applied mathematics, then his lament would have been groundless. Of Boole's work he wrote in 1860 that it cannot be "separated from Mathematics, since it only demands algebra, but such taste for thought about the notation of algebra as is rarely acquired without much and deep practice" [1860c,255]. De Morgan had sharply distinguished between his own methods and those put forward in Boole [1847a] in S<sub>2</sub> [6.7 text and (3)] and omitted any allusion to the latter in S<sub>3</sub> in connection with "mathematical logic".

His attitude towards Boole's work leads to the conjecture that what De Morgan must have expected from a future collaboration of the two sciences was something essentially different from that put forward in Boole's work or from what we nowadays mean by "mathematical logic". Judging from his own contributions and concerns, as scattered in his writings from 1835 up till 1865, we tend to believe that what he had meant was a further development of logic on the lines already engraved by him.

We want to stress that De Morgan followed throughout his writings a conceptual approach, showing much more interest in elucidating first principles [3.5.(4); 6.7.(4),(11)] than in inventing new rigorous technical processes. When he wrote in his paper "On infinity" [1865,180] that "If I speak very decidedly about the consequences of the neglect of pure logic by mathematicians, as I have done elsewhere about the neglect of mathe-

metrical thought by logicians ...". De Morgan implied by "thought" foundational principles, rather than techniques. And these principles can be briefly encapsulated in Peacock's PEF and the FM issue [6.4,(9); 6.5,(5)].

Despite its unfinished state,  $S_4$  seems to be De Morgan's final contribution in the ascent from traditional to advanced relational logic --the latter forming "the actual organ of reasoning of the world at large" [(1) above]. In that memoir he had succeeded not only in extending logic in the way foreshadowed in 1847 [see 6.6,(1)], but also in getting rid of any material elements he had been accused of in 1851 by Mansel [6.7,(2),(9)]. His final concern with logic in 1863 was to prove to Hamilton's followers that their traditional logic was old-fashioned and erroneous and could only be freed from error by his own reformulation as put forward in  $S_2$  and systematically presented in  $S$  [see 6.6,(10)].

From 1860 up to his death in 1871 De Morgan contributed several mathematical papers on arithmetic, divergent series, statistical tables and roots of functions, including a <sup>single</sup> one on the foundations of algebra [1865]. His concern in that paper was with clarifying the notions on "infinity" and "equality" in algebra, paying attention to the lack of formal character of certain forms [see (39.15)]. What is mostly interesting about this paper is his frequent allusion to logic for illustration and reinforcement of his arguments. We can take this attitude as a sort of illustration of what he had earlier meant with the mutual utility of the two sciences. Arguing that algebra will be formal only until symbols are so understood that " $2x-x$  gives  $2-1$ " [see 1865,181] he added:

There is nothing in a purely formal identity which admits of particular and exceptional cases: that "something which is both A and B is something which is both A and C necessitates something B is also C", is an assertion which cannot be denied by finding out some particular A for which it cannot be true<sup>(21)</sup>.

Mathematical training had facilitated De Morgan to extend the forms of logic on a consistent basis. Now logic strengthens his ability to perceive inaccuracies and gaps within algebra by



drawing on his latest contributions. When Peacock founded symbolic algebra upon the PEF, that approach was very near "to the assertion that algebra is, like logic, a formal science" [1865, 180]. However, something was then missing and that was the FM distinction "which now rules in the definition of pure logic";

My mode of statement would be that algebra ought to be a formal science: I do not maintain that it is. It will become a formal science when all its forms, without exception, shall be true of every material instance, equally without exception<sup>(22)</sup>.

The formal character of algebra in the late 1840's had inspired De Morgan towards formulating the theory of the abstract copula in 1850 and reaching the highest degree of formalization in logic in 1860 (see 6.5, (4), (5); 6.7, (7), (10)-(11); 6.8). But then, returning to algebra he realized that it was in need of an extension of the meaning of the sign of equality [1865, 182; 3.9; (16); (22) above] just as the copula "is" had been in need of a similar extension fifteen years earlier. Initially logic was lacking the formal character of algebra, but now the situation was reversed. We may thus state that according to his own concerns, it was not logic but algebra which was left in an unfinished state and any attempt to develop further or mutually apply either sciences would be hopeless if algebra was not properly considered under the light of the FM issue.

Boole's Mathematical analysis of logic [1847a] and its mathematical background.

### 7.1 An outline of Boole's life and work.

George Boole was born in Lincoln in 1815. It was his father who instructed him in elementary mathematics, and when 10 years old a bookseller became his tutor in Latin. Soon Boole took up Greek, and at the age of 14 he translated a poem of Meleager<sup>(1)</sup>. He had such a passion for literature that by the age of 16 he could read Aristotle, Virgil, Dante, Montaigne and Goethe in the original languages. The stimulations thus derived, led to the discovery of his own poetic tendency which marked his temperament throughout his life<sup>(2)</sup>.

Language is directly connected with grammar, and certain philologists, like the German W.A. Becher, laid emphasis in their grammars on the links between language formation and process of reasoning. This was an additional stimulation for Boole, for when he later composed his treatises on logic, The mathematical analysis of logic [1847a] and An investigation of the laws of thought [1854], the traces of the influence derived from his study of language on his logic were apparent. For Boole quotes both Becher and R.G. Latham in these works, the latter being a modern authority in grammar whom Boole had consulted on logical matters<sup>(3)</sup>.

At the age of 16, in 1831, Boole switched to the study of mathematics. Lacroix's (1802; see 1.8), an elementary textbook on differential and integral calculus, occupied him for two years. He later regretted for spending so long on it and would advise beginners to use primarily textbooks used in Cambridge university, which were richer in examples than the French ones (MacHale 1985, 18; Rhees 1955, 77). He then proceeded to study Laplace's Mécanique Céleste and Lagrange's Mécanique Analytique and Calcul des fonctions. We saw in chapters 4-5 how influential both Laplace and Lagrange had been in the shaping of Boole's research in analysis.

At that time Boole worked as a teacher in Duncaster, and in 1833 he became the headmaster of a school in Waddington, near Lincoln. He studied Newton's Principia, and as early as 1831 he was asked to deliver a speech on Newton in the Mechanics Institute in Lincoln. Boole declined, feeling too young to master such a big topic [Neil 1865, 85].

Around 1832 Boole, influenced from Hebrew literature where God was spoken as the great All or Unity, contemplated a mode of study in which the characteristic element was the unity of opposite forms. This mode proved effective later on, and Boole was relatively soon to contemplate in embryo the possibility of applying symbolic expression to logical relations<sup>(4)</sup>. Another important influence was derived from his comparison of the methods in Newton's Principia and Lagrange's Mécanique, which led him to the consideration of the role of abstraction, generalization, intuition and induction in the process of discovery<sup>(5)</sup>. In 1835 he delivered a speech on Newton, winning the respect of many people in Lincoln [Neil 1865, 87-88]. Passages from the speech show evidence of Boole's contemplation over systems of effectively constructed symbols; of abstraction and of his deep interest in ancient philosophies, particularly religion<sup>(6)</sup>.

Around 1838, Bromhead, one of the founders of the Analytical Society [Enros 1983, 27], initiated Boole in the spirit of the Society. They corresponded for two years and in the letters exchanged Lagrange's influence on Boole in reducing the expression of physical problems to purely algebraic terms is apparent. By that time Boole was acquainted with the works of Cauchy, Monge, Fourier, Jacobi and V.A. Lebesgue [MacHale 1985, 46-9].

Boole visited Cambridge around 1839. He met several analysts, among them Gregory. A strong friendship was thus born between Boole and Gregory which lasted up to the latter's premature death in 1844. As we saw in chapter 4, Gregory provided Boole with an extra stimulus for the study of symbolic algebra and the calculus of operations. Boole later acknowledged Gregory's generous help mentioning the latter's contributions in shaping and publishing Boole's first paper on symbolic methods, [1841c], in his textbook on differential equations published in 1859<sup>(7)</sup>. When Boole was tempted to follow a course in Cambridge,

Gregory dissuaded him by saying that he would thus undergo a great deal of mental discipline, which is not agreeable to a man who is accustomed to think for himself [Harley 1866a, 12].

Another interesting friendship was born in 1842. On the occasion of the publication of De Morgan's textbook on mathematical analysis, Boole introduced himself in a letter to him [Smith 1982a, 7]. In a voluminous correspondence that lasted up to Boole's death in 1864, the two men exchanged opinions on a diversity of common interests such as: history of philosophy and science, education, metaphysics, mathematics, logic, even homeopathy. But, contrary to what one would expect, they exerted little influence on one another<sup>(8)</sup>.

First Gregory and next De Morgan were to be consulted about the publication of Boole's first masterpiece, "On a general method in analysis" [1844]. The latter's reaction was that of enthusiasm and Boole was immensely pleased [Smith 1982a, 13] <sup>(9)</sup>. The nearly rejected paper by the Royal Society finally proved to be a success and won the Gold Medal of the Society [Harley 1866a, 17-18; MacHale 1985, 61-2].

Arthur Cayley, excited by Boole's work on linear transformations [1842b], started corresponding with him in 1844. In the early letters the topics of discussion were invariant theory and quaternions [MacHale 1985, 56-8]. Soon after the publication of MAL in 1847, Cayley touched upon some delicate problems of Boole's logic. This correspondence forms an interesting source of information, unused so far, for our study of Boole's methodology in logic [see 7.5; 8.2-8.3]. Moreover, Boole's work in logic motivated Cayley to write one paper on that subject in 1871 [Jourdain 1910, 342-3].

In 1845 Boole went to Cambridge again to read a note "On the equation of Laplace's functions" [1845c] to the meeting of the British Association for the Advancement of Science. There he made a contact with the mathematical physicist W. Thomson who introduced him to several analysts, among them C. Graves. The latter offered to introduce him to W.R. Hamilton but Boole declined the offer, as we learn from a letter of his to Thomson in 1846, for reasons of modesty<sup>(10)</sup>.

Graves shared with Boole a common interest, the calculus of

operations. Despite the little personal contact he had with Boole, he admired the latter's "powerful method" in analysis<sup>(11)</sup>. Boole enthusiastically sent to Graves the manuscript of MAL prior to its publication [MacHale 1985,70]. Graves offered some modifications which proved quite definite in Boole's later formulation of his method in logic, as we will see in 7.6.

By that time Boole was ready to handle the construction of a calculus of logic of which he had dreamt in his youth. This calculus was to be analogous to the one he had mastered so well in handling problems of analysis, the calculus of operations. And when he got acquainted with the topic of dispute between De Morgan and Hamilton, Boole resumed the "almost forgotten thread" of his former inquiries and composed MAL [MAL,1].

De Morgan's introduction of the concepts of the universe of discourse and contrary terms as well as Hamilton's views on the study of mathematics and the quantification of the predicate, proved to be of significant influence on Boole, as will be seen below<sup>(12)</sup>. Moreover, both logicians - greatly esteemed by Boole - had used symbolic, though not strictly algebraical, means in order to represent propositions and syllogistic deductions. All these new ideas motivated Boole to write up his own ideas on logic and thus present to the public a new approach to this subject. MAL appeared on the same day as De Morgan's FL. Neil commented upon the two books as follows: "We may remark that these books, notwithstanding the apparent similarity of subjects, are little if at all connected with each other" [1865,91]<sup>(13)</sup>.

Despite his estimation of Hamilton as a man of "genius and learning" [MAL,12]<sup>(14)</sup>, Boole argued in the "Introduction" of MAL against Hamilton's view that the study of mathematics was "at once dangerous and useless". He also denied the view that mathematicians are not capable to inquire into the origin and nature of their principles [MAL, 11-13]. Boole opens his "Introduction" p[MAL,3] as follows:

They who are acquainted with the present state of the Theory of Symbolical Algebra, are aware, that the validity of the processes of analysis does not depend upon the interpretation of the symbols which are employed, but solely upon the laws of their combination...This principle is indeed of fundamental importance; and it

may with safety be affirmed, that the recent advances, of pure analysis have been much assisted by the influence which it has exerted in directing the current of investigation.

However, in the "Preface" Boole, cautious to prevent any misconceptions of his new symbolic approach, wrote that his method in logic was to be "chiefly valuable as an evidence of the speculative truth of its principles. To supersede the employment of common reason, or to subject it to the rigour of technical forms, would be the last desire of one who knows the value of that intellectual toil and warfare which imparts to the mind an athletic vigour, and teaches it to contend with difficulties and to rely upon itself in emergencies" [MAL, 2] <sup>(15)</sup>. The same passage was to be once more stressed in his [LT,12] <sup>(16)</sup>. This passage strongly reminds us of De Morgan's desire to render the student's grasp "more intellectual and less mechanical" [1836,art.3;3.5]. What repeatedly Boole tried to emphasize in most of his work in logic, was to make it clear that, despite its symbolic forms, logic is not to be identified or confounded with mathematics. His friend and biographer Harley was among the first to comprehend this [1866a, 36].

In 1848 a paper titled "The calculus of logic" was published in the Cambridge and Dublin Mathematical Journal, here after cited as COL. In ~~fact~~<sup>fact</sup> the paper was a summary of MAL. Probably, being more accessible than the 'little read' MAL [Bryant 1901,107], COL, was moderately read [MacHale 1985,72]. On the same year Boole wrote a short paper on quaternions. On this subject he would later contribute more concerning the Laplace equation [see 5.8].

The following year he was elected as a professor of mathematics at the newly-formed Queen's College at Cork where he remained up to his death in 1864 <sup>(17)</sup>. In 1851 he delivered a long speech on the "Claims of science" (hereafter cited as "Claims"). In this speech Boole took the opportunity to dwell upon the concept of unity and clarify the interrelation between logic and mathematics. Boole recalled of the Ionic school of philosophy according to which the universe was a unity. Also, in Xenophane's language, he said, "the One is God". Thus existence was viewed according to these dualistic theories as an outcome of a blending

of opposing elements [1851,207-8]. In Boole's language this knowledge is embodied in the equation

$$(71.1) \quad x + (\text{not-}x) = 1,$$

1 standing for both unity and universe and  $x$ , not- $x$  for two contrary, in the sense of complementary, classes. His wife was later to call (71.1), which in fact includes the law of the excluded middle, "The Mystic law or psychological equation" (M.Boole 1972, 63-65. See also 7.4; 8.4; 8.8).

Boole compared in his speech logic and mathematics, distinguishing them as two branches of a wider science. We will discuss the matter later on, but at this stage I would like to quote one of the conclusions to which he arrived at: [1851,194-5]:

If it is asked whether we are able to deduce the actual expressions of its fundamental laws, I reply that this is possible, and that the result, constitute the true basis of mathematics. I speak here, not of the mathematics of number and quantity alone, but of mathematics in its larger, and I believe, truer sense, as universal reasoning expressed in symbolical forms, and conducted by laws, which have their ultimate abode in the human mind.

From what we saw so far, the emphasis on the role of notation was one of secondary importance in Boole's system<sup>(1a)</sup>. Boole's educational concern as well as the fact that he viewed both logic and mathematics as branches of a universal calculus of symbols, will form the basis of our study throughout this and the next chapters. For, to interpret his intention as one of application of symbolic forms to logic solely for rendering his deductive system mechanical, would be a big mistake. Thus we would offer an incomplete image of Boole's work stripped of its historical background and its significant educational, psychological and metaphysical overtones.

Actively working on mathematics throughout 1851, Boole produced three papers on probability [Harley 1866a, 24-25; MacHale 1985, 279-80]. The motive to do so was partly derived from De Morgan's "Essay on probabilities" (1838) and the respective chapters in his FL, and partly from a problem by Mitchell of the distribution of the fixed stars [MacHale 1985,128]. Dealing with this new subject, greatly encouraged in that by Donkin [MacHale 1985,213], Boole realized its close proximity to logic

and developed fully this view in chapters 15-21 of his LT. The book was ready for print in 1853 but problems in publication arrangements kept it unpublished till 1854 [Neil 1865, 166].

Boole had arranged that LT would be distributed among all thinkers who would be interested in the subject it dealt with. We learn this from Neil [1865, 166], who had the honour of receiving the book with Boole's compliments. Neil had the following opinion on this work which he published around 1855: "The writer displays singular sagacity, stretch and tension of reasoning, an enviable power of lucid exposition, a wonderful capacity for abstract speculation and recondite thinking. The author is profound and erudite; he has brought the far-grasping power of an able mind to the consideration of the subject, and has given in clear, distinct, and expressive language a large development to the philosophy of thought. The subtlety, power, and persistency shown in carrying out the strict and unyielding system of symbolical expression and interpretation for which the author contends are indicative of a mind of superior order. The book opens up views of the relations and forms of thought capable of vast results" [1865, 166].

Some of Boole's contemporaries received Boole's elaborate philosophical enlargement of MAL with enthusiasm<sup>(19)</sup>. For, in both the introductory and final chapter, Boole took the opportunity to stress the philosophical dimension of his subject [see also 8.7, 8.9]. Unfortunately we know that the book had no such impact in areas outside of mathematics as Boole had hoped [MacHale 1985, 128].

Rather too emphatically, and for reasons that are not very clear, Boole said in the "Preface" that the book "is not a republication" of his former treatise, "its methods [LT] are more general, and its range of applications wider. It exhibits the results, matured by some years of study and reflection, of a principle of investigation relating to the intellectual operations, the previous exposition of which was written within a few weeks after its idea had been conceived" [LT, iii]. Despite the fact that MAL was hastily written according to Boole, and of a much smaller size compared with LT, not only most of the key ideas of Boole's general method as presented in LT



originated in MAL, but, as some historians claim, Boole's original ideas are more satisfactorily exposed in MAL<sup>(20)</sup>. On the comparison of these two works we will extensively comment upon in the following chapter.

Boole explicitly stated in LT the possibility of an algebra of symbols that would admit solely the values 0 and 1. "The laws, axioms, and the processes of such an algebra will be identical in their whole extent" with those of an algebra of logic. "Differences of interpretation will alone divide them. Upon this principle the method of the following work is established". [LT, 37-38]. This algebra in many respects resembles what we nowadays call "Boolean algebra"<sup>(21)</sup>. According to G. Birkoff the "Boolean algebra" of classes, largely originated in this classic book, has had an ever-increasing influence on all branches of mathematics" [MacHale 1985, 130]. Due to the link of Boolean algebra with the function of computers Boole is called "Father of Computer Science", but this title is too exaggerating and distorting for reasons we have already stated above<sup>(22)</sup>.

In 1855 Boole married Mary Everest. Mary's father was a friend of Babbage and Herschel, and early in her life she was interested in mathematics. The marriage was successful and Mary shared her husband's interests, becoming a devoted disciple [M. Boole 1972, 5]. During twenty years after Boole's death Mary Boole was committed to spreading her husband's ideas about mathematics and logic, developing at the same time her own views on education.

As the time passed by, Boole's interests in psychology and religion became more and more apparent. Of equal primary importance was to him the educational aspect of both mathematics and logic. For Boole, as his students were later to recall, had a genuine compassion for them. The honour of discovery in dealing with a problem was to be shared between teacher and students. [Rhees 1955, 76-7]<sup>(23)</sup>.

In 1857 Boole had in part prepared a work which was to bear the title of The philosophy of Logic. MacMillan company had announced that the book was "nearly ready" but Boole was "so ill to satisfy with himself that he shrunk from laying the work before

the public at that time" [Neil 1865,172;8.7,(1)]. The reason why Boole undertook to write this book is included in its "Preface":

It must be possible to interpret within the purely logical sphere and by purely logical ideas and conceptions all the processes methods and results to which that analogy [between algebra and logic] has led. Now such an interpretation is the object of the present work. I seek to bring into light and prominence the philosophical elements which in my former exposition [LT] were too much hidden beneath the veil of a symbolical notation... I wrote the former book [LT] for mathematicians. The subject is of wider interest. This is intended for the general public. Mathematics will not appear except in the notes<sup>(24)</sup>.

In 1857 Boole communicated to the Royal Society of Edinburgh a paper "On the application of the theory of probabilities to the question of the combination of testimonies and judgements". The paper, published in the Transactions of the Society occupied 56 quarto pages. Boole was awarded for this paper the Keith medal of the Society in 1858 [MacHale 1985, 62]. In 1859 he produced his textbook on Differential equations and in the following year a textbook on Finite differences [1860]. These two works display a wide range of original research in respect with integrating factors, the Riccati equation, singular solutions, summation of series and, above all, symbolical methods applied to various types of differential, finite difference and functional equations. Besides, Boole's extensive acquaintance with the works of his contemporaries in the Continent, England and Ireland is shown by his numerous references to them. Particularly striking is the fact that De Morgan is mentioned often in connection with his work on differential equations.

Both textbooks were regarded as classics in Cambridge, and Boole's treatment of convergence and symbolical methods, looking backwards rather to Lagrange than to Cauchy's new analysis, was followed by Milne-Thomson and Forsyth at the turn of the century<sup>(25)</sup>. From that time onwards Boole focused entirely on aspects of differential equations with an exception of a paper on the roots of algebraic equations, a subject that had interested Harley a lot [MacHale 1985,223. chapter 15].

In 1860 Boole was proposed as a candidate for a post in Ox-

ford University. Such a post had been for him a life's dream, for there he could combine research and teaching in both mathematics and philosophy. But, afraid to participate in the religious disputes that took place in Oxford, he withdrew [MacHale 1985, 168].

In the last years of his life Boole viewed aspects of logic and mathematics from a rather metaphysical point of view. In 1862 he wrote to De Morgan: "I do not so much care about the mere forms of Logic as about the philosophy of the connection between thought and speech" [MacHale, 1985, 224-5; Smith 1982a, 102]. This is particularly evident in certain extracts from Boole's work during the period 1855-64 [see 8.7, 8.8]. The subject of singular solutions was viewed from a metaphysical angle. When a former student of his confided "in a puzzled" way to Mary Boole that the respective chapter in Boole [1859] "does not read like a chapter of an ordinary book", Mary attributed this to Boole's "devout study of the mystic literature of all ages" [M.Boole 1972, 69]. Later on she was to write a book [1897] in which she compared Boole's methodology with that of Father Alphonse Gratry (1805-1872) [24].

## 7.2 : Key features of Boole's Mathematical analysis of logic [1847]. Plan of our study.

According to Boole, logic is based primarily on man's faculty "to conceive of a class and to designate its individual members by a common name" [MAL, 4]. Then, by a mental act, called "election", we are able to select those members of a given class and contemplate them apart from the rest<sup>11</sup>. This "elective operation" can be repeated with other elements of distinction in a similar way as in common language "we accumulate descriptive epithets for the sake of more precise definition" [MAL, 5].

Boole believed that the theory of logic is intimately connected with that of language. In fact he held that "A successful attempt to express logical propositions by symbols, the laws of whose combinations should be founded upon the laws of the mental processes which they represent, would, so far, be a step toward a philosophical language" [MAL, 5]. This view, though little pursued either in MAL or in LT, would underline most of Boole's logical

researches. We will have the opportunity to notice certain instances in which Boole did consult the grammar of ordinary language for his construction of a logical calculus actively illustrating similar views held by Hobbes, Condillac or Whately, though his direct influences were to be derived from more contemporary grammarians such as Blanco White and R.G.Latham<sup>(2)</sup>.

However, what will be particularly evident throughout his work in logic under study, is his drawing by analogy from the

"well constructed language" of algebra. This influence is derived mostly from his own mathematical background, particularly the algebra of differential and finite difference operations, as in his [1844], then from similar views that appear only theoretically in Condillac's or Whately's works in logic. Like De Morgan, Boole believed that "An instance in which a language undoubtedly sub<sup>h</sup>co<sup>h</sup>es the purposes of reasoning is presented in the science of algebra"<sup>(3)</sup>. But, as we shall see, he was to carry out this analogy between forms and processes of algebraical analysis and logical analysis, more boldly and substantially than De Morgan.

In MAL the variables  $x, y, z, \dots$  of Boole's logical calculus stand for the "elective operations" mentioned above and not for the classes  $X, Y, Z, \dots$  upon which they operate, as later in LT. This distinction is of primary importance for it sheds light into the origins of Boole's inventions first introduced in MAL and later developed under certain modifications in his LT<sup>(4)</sup>.

As we shall see Boole often viewed the function of elective symbols  $x, y, z, \dots$  as analogous to that of differential operators. In fact, in some notes of probable date around 1848 [see (2) above] Boole compared the function of elective symbols with that of certain discontinuous integrals whose values range in the set  $\{0, 1\}$  [see 8.3, (2)]. These bold comparisons should not come as a surprise. The outcome of his researches in analysis was that "if we examine the more complex forms and processes of algebraical analysis we find that there are all reducible to an ultimate dependence upon the laws of great simplicity" [N<sub>7</sub>; see 7.2, (2)].

Boole's method in MAL is founded upon the possibility to give to the "elementary laws" according to which the variables  $x, y, z, \dots$  operate an "exact symbolical presentation". Moreover,

certain unexpected discoveries [see 8.2, (6)] led Boole to believe that "The laws we have to examine are the laws of one of the most important of our mental faculties. The mathematics we have to construct are the mathematics of the human intellect" [MAL,7]. Boole not only borrows mathematical procedures for his construction of a calculus of logic, but as the above statement somehow vaguely indicates, this calculus is indeed a branch of what we might call "Universal calculus of symbols" or "General mathematics"<sup>(3)</sup>. Other instances though will further justify this interpretation of the framework of his treatises on logic.

After laying down the first principles of his method, Boole expressed both categorical and hypothetical propositions as equations. Their variables standing for elective symbols, he called them "elective equations" [see 7.2, (1)]. By means of this method Boole was able to deduce the laws of conversion and syllogism [MAL, 6]. The power of his method though, lies in its ability for generalization which renders, possible the resolution of "the most complex system of propositions". In this method Boole claims that "Every process will express deduction, every mathematical consequence will express a logical inference. The generality of the method will even permit us to express arbitrary operations of the intellect, and thus lead to the demonstration of general theorems in logic analogous, in no slight respect, to the general theorems of ordinary mathematics" [MAL,6].

The general method involves a study of the properties of elective functions  $\phi(x)$ ,  $\phi(x,y)$  and hence the solution of elective equations. No matter how complex such an equation can be "it can be solved and every solution interpreted" [MAL, 7]. In other words, every elective equation is interpretable as a proposition. Boole offers two methods. The first, called "method of substitution", does not involve his general theory. The second, the "method of development", is based on this theory giving formal results that have consequently to be interpreted.

Now, the main distinction between the "basic logic", as in chapters 1-5, and the "general method in logic"<sup>(4)</sup> as presented in the two last chapters, 5-6, lies in that the second involves numerical coefficients, other than 0 and 1. All terms that involve such coefficients are uninterpretable in Boole's system.

This peculiarity arose serious objections as Boole does little to fully explain his procedures in MAL. The most characteristic case of reaction was that of Cayley who expressed his perplexities in connection with this matter in letters, and Boole's answers provide us with some interesting information on how he dealt with this "problem" [see 7.5, 8.2]. In brief his answer is that terms may be uninterpretable but equations always can be interpreted. In these letters Boole draws by analogy from imaginary numbers [8.2. text and (7), (8)]. But the best way to explain his admittance of uninterpretable forms is to look back to his [1844, 1845d, 1846, 1847b, 1847c] where, in the course of symbolic procedures, certain terms are uninterpretable as they stand and only the final result admits of interpretation.

Boole had expected reactions towards his symbolic procedures in logic, particularly by logicians such as Hamilton who were against the use of any mathematical handling of logic. He tried to justify his approach appealing to the efficiency of symbolic methods in the "progress of scientific discovery" [MAL,9], bearing in mind their successful applications in mathematical and physical problems as in his mathematical papers cited above. This powerful tool of analysis, he claimed, if treated with "perfect comprehension" of what renders it lawful, provides us with an intellectual discipline of a high order, "an exercise not only of reason, but of the faculty of generalization". He added cautiously that "each subject of applied mathematics should be treated in the spirit of the methods which were known at the time when the application was made, but in the best form which those methods have assumed" [MAL,10].

Apart from the hostility of certain logicians towards any mathematization of logic, Boole was also aware of the probable errors into which a mathematician could fall if he abused the "unmeasured capabilities" of the calculus of operations. The laws of correct reasoning were paid little attention to, according to him<sup>(7)</sup>. As far as symbolic reasoning in general is concerned, the laws that render it lawful are the following [LT, 68]:

- 1st. That a fixed interpretation be assigned to the symbols employed in the expression of the data; and that the laws of the

combination of those symbols be correctly determined from that interpretation.

2nd, That the formal processes of solution or demonstration be concluded throughout in obedience to all the laws determined as above, without regard to the question of the interpretation of the particular results obtained.

3rd, That the final result be interpretable in form, and that it be actually interpreted in accordance with that system of interpretation which has been employed in the expression of data.

This list is drawn from a later book [1854], but its essence underlies not only the whole of Boole's methodology in MAL but more generally his whole attitude towards symbolic procedure in mathematics. As no restriction is imposed upon the nature of the data, the laws or the interpretability of the results, the quotation strongly suggests what we have claimed above. That is, that Boole perceived the universality of symbolic approach which, under certain conditions, can be equally successful in both mathematical and logical fields.

It is in general a known fact that, as with the case of De Morgan, Boole's methods of solution and demonstration lack the appropriate rigour in the modern sense. Even at his time the very calculus of operations was handled more formally by Cauchy who had rejected Lagrange's algebraic calculus and had suggested more rigorous foundations in analysis [1.7]. Boole implicitly was to assume certain axioms while handling symbolic processes in logic based on his self-confidence acquired while applying them in problems of analysis. Inevitably, this very approach in another field, which lacked the historic background of the calculus of operations, led him to minor or major fallacies which have been pointed out recently<sup>(a)</sup>.

However, Boole's system has proved to be consistent and though he does not speak of consistency he must have had it in mind. Our study will focus on the following themes, mainly in connection with MAL:

1. The analogies that exist between Boole's calculus of logic and his calculus of operations in general, including a diversity of other mathematical concepts<sup>(a)</sup>.
2. The two methods for the solution of elective equations, in-

cluding their interpretability, illustrating certain analogies with the solution of differential equations<sup>10</sup>.

3. The correlation between ordinary language, logic and mathematics; in particular the view that mathematics and logic form two separate and parallel branches of what Boole called as "universal reasoning expressed in symbolical forms" [1851; see 7.1, (5)].

In this chapter we will focus mainly on MAL for, on one hand all the main concepts of logic later developed in LT are introduced in MAL, and on the other hand, the link between Boole's calculus of logic and calculus of operation is more apparent in MAL than in other works [see 7.2, (4)]. In sections 7.3-7.7 we will cover the symbolic expression of laws and categorical propositions, the solution of elective equations by substitution, examples of conversion and syllogism, hypothetical propositions, properties of elective functions and finally the solution of elective equations by development.

At each stage of this study a work from a later period will be consulted only so far as it helps to clarify the matter under discussion more accurately. Recent papers on Boole's work on logic will be consulted, but we will mainly draw from Boole's own work on logic and mathematics including some unused manuscripts from the collection of the Royal Society of London. As in the case of De Morgan, we will view Boole's work with an eye of a historian, taking under account strictly the spirit of the time when his mathematised logic was born.

### 7.3 Symbolic expression of laws in MAL.

In MAL the symbol 1 represents the Universe of discourse. According to Boole it comprehends "every conceivable class of objects whether actually existing or not" [MAL,15]. If X stands for an individual member of a given class, x stands for the elective symbol that selects from the subject (the class u or v) upon which it operates all the Xs that it contains<sup>11</sup>. If no subject is expressed then x is supposed to operate upon the universe 1, and we thus have:



(73.1)  $x=x(1)$   
 [MAL, 15-16] '2'.

Next Boole defines the product  $xy$  as representing, in succession, "the selection of the class  $Y$ , and the selection from the class  $Y$  of such individuals of the class  $X$  as are contained in it". The first law he lays down is that "The result of an act of election is independent of the grouping or classification of the subject": In other words, if  $u$  and  $v$  are two subclasses of the universe,  $u+v$  represents the "undivided object" and the law is thus expressed as:

(73.2)  $x(u+v)=xu+xv$   
 [MAL, 16-17] '3'. This law is the equivalent of

(73.4)  $n(u+r)=nu+nr$ ,  
 the distributive law in analysis, where  $n$  stands for  $d/dx$  or  $\Delta$  or  $\delta$  acting upon the subjects  $u$  and  $r$  [1844, 225].

According to the second elementary law, multiplication of elective symbols is commutative; hence

(73.5)  $xy=yx$   
 [MAL, 17]. When  $xy$  is supposed to act upon a subclass  $u$  of the universe, Boole implicitly assumes that the operation is effected according to

(73.6)  $(xy)u=x(yu)$   
 [Hailperin 1984, 42]. The law (73.5) has also its equivalent in the algebra of differential operators,

(73.7)  $npu=pnu$ ,  
 $n, p, u$  as above [1844, 25].

Throughout MAL Boole makes use of the law (73.2) in the form  
 (73.8)  $x(y+z)=xy+xz$ ,  
 $x, y, z$  all elective symbols, but defines it so only in subsequent works [see COL, 127; LT, 29-33]. Hereafter, when referring to the first two elementary laws, we will mean (73.5) and (73.8). If we assume besides (73.6) the law  $1u=u$ , provided by Boole only in [LT, 47-8], we deduce that 1, distinguished from the universe, is a unit of multiplication in Boole's system [see (2) above].

The third law says that "the result of a given act of election performed twice, or any number of times in succession is the result of the same act performed once", hence

(73.9)  $x^2=x$ .

from which it follows that

$$(73.10) \quad x^n = x,$$

$n$  a positive integer [MAL, 17-18; COL, 127] '4'.

The law (73.9), or index law, is peculiar to logic and is the key to interpretability in Boole's calculus since only terms that obey (73.9) are admitted as interpretable. However, Boole provided an example of ordinary arithmetic, viz., that the  $n^{\text{th}}$  power of a positive number is positive, where (73.9) holds true '5'. But, more generally, (73.10) has also an analogous law in the algebra of differential operators, namely:

$$(73.11) \quad n^m n^u = n^{m+u}$$

[1844, 225] '6'.

Boole saw these analogies and wrote in his notes [Na-N<sub>6</sub>; see 7.2, (2)]:

The laws  $x(y+z)=xy+xz$  and  $xy=yx$  are the only laws of algebraic symbols devoid from the conception of quantity. The law  $x^m x^n = x^{m+n}$  merely implies that if the result of the operation  $x$  be performed  $n$  times, and then  $m$  times upon the result, the final result is the same as if it were employed  $m+n$  times. In full it merely expresses a law of number, i.e. of the number which expresses how often an operation is performed and indicates that that number may be considered as a whole  $m+n$ , and resolved into parts. Hence the law  $x^m x^n = x^{m+n}$  is a law quite independent of the nature of the operation denoted by  $x$ . It is for example true in the Differential Calculus that  $(d/dx)^m (d/dx)^n = (d/dx)^{m+n}$ . So [to] the above [meaning the laws (73.5), (73.8), (73.10)] it would seem that we ought to add the law whose expression is  $x+y=y+x$ .

We see by now a first instance where our assumption about analogy and independence between the two calculi, of logic and operations, as holding simultaneously in Boole's mind is justified. Undoubtedly his early mathematical papers, [1844, 1845d], must have inspired his composition of MAL. However, Boole does not seem to have imposed the results obtained in his old calculus on the new one. On the contrary, having laid the foundations for his calculus of logic he gradually amends it and enlarges it while noticing the analogies existing between his foundations of logic and the calculus of operations. It is of interest to note how he arrived in the above quotation at including a new law

$$(73.12) \quad x+y=y+x,$$

first published in his [LT,33] '7).

Having laid down the three elementary laws in MAL, he went on to state the following as the "one and sufficient axiom" involved in his logical deductions :

(73.13) "Equivalent operations performed upon equivalent subjects produce equivalent results" [MAL,18].

In COL this axiom is stated in a slightly modified way, and only in the manuscript notes quoted so far did he verbally state that (73.13) implies

$$(73.14) \quad (x=y, z=w) \text{ ----> } x \pm z = y \pm w \quad \text{and}$$

$$(73.15) \quad (x=y, z=w) \text{ ----> } x.z = y.w.$$

See [N<sub>+</sub>;LT,36; Corcoran 1980, 616-618 and Hailperin 1984, 43; On substraction see also (3) above].

Associativity is surprisingly missing as a law, not only from MAL but also from LT '9'. Division is not defined as the inverse of multiplication. Postponing his approach towards dealing with  $x/y$  in connection with the solution of elective equations for his last chapter, Boole made at the end of his first chapter "On first principles" the following comments. First he claimed that in virtue of the first two elementary laws "all the processes of common algebra are applicable to the present system" [MAL,18].

This claim was to raise reactions for, as it is implied from his next remark, though algebra and logic bear certain similarities, at the same time they differ in certain substantial respects. He wrote "From the circumstance that the processes of algebra may be applied to the present system, it is not to be inferred that the interpretation of an elective equation will be unaffected by such processes. The expression of a truth cannot be negatived by a legitimate operation, but it may be limited" [MAL, 18-19]. Thus, for example, it holds that

$$(73.16) \quad y=x \text{ ----> } xy=xz.$$

"This is a perfectly legitimate inference, but the fact which it declares is a less general one than was asserted in the original proposition" [MAL, 19]. In other words, the converse of (73.16)...

$$(73.17) \quad xy=xz \text{ ----> } y=z,$$

does not hold true according to Boole. To his dispute with

Cayley about his claim that "all the processes of common algebra are applicable to the present system" we will refer in 7.5 and 8.2. We will now proceed to the symbolic expression of categorical propositions.

#### 7.4 Categorical propositions expressed as elective equations in MAL.

The chapter "Of expression and interpretation" opens with a quotation from Aldrich and Whately. Boole admired Whately a lot, and according to one of his students he was acquainted with Whately's work in logic well before 1846 [Rhees 1955, 76; see also LT, iii; MacHale 1985, 33,39].

In order to deal with negation, he introduced the class not- $X$ , denoted symbolically by  $1-x$ . This was Boole's only device for negation. Any statement regarding something that cannot hold true would be represented by an equation and the fact that inequalities, such as  $x \neq y$ , could not be expressed by his language, [Corcoran 1980, 635,fn 6], was not a major drawback in his system, since he did not need to state such relations. [See also 8.4]. Then Boole stated verbally what we call the principle of the "excluded middle", which takes the form

$$(74.1) \quad x+(1-x)=1,$$

as follows: "The class  $X$  and the class not- $X$  together make the Universe" [MAL, 20]. Law (74.1) is not symbolically expressed in any of Boole's works and only in [LT, 76], while developing 1 in respect with quality  $x$ , he states that it gives as a result  $x+1-x$ .

Let the four basic propositions be:

- A    All    $X$ s are  $Y$ s
- (74.2) E    No     $X$ s are  $Y$ s
- I    Some  $X$ s are  $Y$ s
- O    Some  $X$ s are not  $Y$ s.

Dealing with A he thought as follows: "As all the  $X$ s which exist are found in the class  $Y$ , it is obvious that to select out of the Universe all  $Y$ s, and from these to select all  $X$ s, is the same as to select at once from the Universe all  $X$ s" [MAL, 21].

Hence, A is expressed by the elective equation

$$(74.3) \quad xy=x.$$

By means of law (73.8) and transposition - missing in MAL<sup>(1)</sup> - he transformed (74.3) into

$$(74.4) \quad x(1-y)=0.$$

It was thus that 0, the second constant that appears in the foundations of Boole's system, was first introduced implicitly, and not as "Nothing", or the class of "no beings" as later in [LT, 47].

As he deduced (74.4) from (74.3), we can deduce from (73.9)

$$(74.5) \quad x^2-x=0 \quad \text{or} \quad (74.6) \quad x(1-x)=0.$$

The three laws, (74.5), (74.6) and (73.9) are equivalent. Law (74.6) is called by Boole the "Law of duality" and is regarded as equivalent to Aristotle's "principle of contradiction" [LT, 49, 51]. However, the form (74.6) is not explicitly stated in MAL and only implicitly it is used in the two last chapters of MAL as alternative to the index law of interpretability<sup>(2)</sup>.

The equation

$$(74.7) \quad xy=0$$

denotes that  $x, y$  represent disjoint classes. It is obvious that (74.7) can hold true for  $x, y$  simultaneously non-empty classes (e.g.: animals and stars). In fact (74.7) is the formal expression for proposition E [MAL, 21]. To express the particular-affirmative I, he thought as follows: "If some  $X$ s are  $Y$ s, there are some terms common to the classes  $X$  and  $Y$ . Let those terms constitute a separate class  $V$ , to which there shall correspond a separate elective symbol  $v$ , then  $v=xy$ . As  $v$  includes all terms common to the classes  $X$  and  $Y$ , we can indifferently interpret it as Some  $X$ s, or Some  $Y$ s" [MAL, 21]. According to Jourdain [1910, 338 fn] "equation  $v=xy$  is rather a definition of  $v$ ", and I is read "the class  $V$  is not the null-class".

According to Boole "the complete theory of categorical propositions, which respects the employment of analysis for the deduction of logical inferences" involves the equations:

A	$x(1-y)=0$	(1)
(74.8) E	$xy=0$	(2)
I	$v=xy$	(3)
O	$v=x(1-x)$	(4)

formulated as shown above [MAL, 22].

But Boole did not stop here. He went on to provide alternative forms for (1)-(4) which he was to use throughout the rest of MAL. Moreover, certain of the new forms would substitute all equations in (74.8) in the new list of fundamental categorical propositions introduced consequently in COL and LT. In so doing, he fell into certain fallacies, for the new forms were not strictly equivalent to those in the list (74.8). One of the reasons for transforming this list was to render it symmetrical. For, in [LT, 64-65] Boole's system of the "three primary forms of equations" was to be

$$X=vY$$

$$(74.9) \quad X=Y$$

$$vX=vY$$

To provide an alternative for (3) in (74.8) he multiplied it with  $x$ , compared the result with the former equation, and hence obtained the form

$$(74.10) \quad v=vx.$$

Similarly he obtained  $v=vy$ , thus,

$$(74.11) \quad vx=vy.$$

To  $vx$  he assigned the interpretation "Some Xs" and to  $vy$  "Some Ys". He then remarked that "this system [(74.11)] does not express quite so much as the single equation [(3)], from which it is derived. Both, indeed, express the Proposition, some Xs are Ys, but the system [ $vx=vy=v$ ] does not imply that the class  $V$  includes all the terms that are common to  $X$  and  $Y$ " [MAL, 22-23].

Thus Boole bears in mind his statement at the end of the previous chapter [see (73.16); MAL, 19] but the very fact that he was to replace (3) with (74.11) in [COL, 129] shows that he violated the restrictions he had imposed upon his calculus in the foundational chapter. Having provided also the form

$$(74.12) \quad vxy=0$$

as alternative for (4), he went on to display, rather vague explanations for preferring the new forms to the old ones [MAL, 23].

Universally in these cases, difference of form implies a difference of interpretation with respect to the auxiliary symbol  $v$ , and each form is interpretable by itself. Further, these differences do not introduce into the calculus a needless perplexity. It will hereafter be seen that they give a precision and a

definiteness to its conclusions, which could not otherwise be secured.

Before we proceed to mention the alternative forms that he provided for the universal propositions A and E by means of the "auxiliary symbol v", let us comment upon v's function up to this point. It is certainly obvious in the cases mentioned above that v has an existential import in Boole's system. When prefixed to one elective symbol it means "some" in the sense "at least one", "some" or possibly "all". Its character hence is indefinite and this suggests a probable influence from Hamilton's claim that "in fact, definite and indefinite are the only quantities of which we ought to hear in logic" [Laita 1976, 151-2].

This influence was certainly derived from Boole's acquaintance with the object of controversy between De Morgan and Hamilton. Due to the former he came across the concepts of universe and contrary terms which he might have contemplated upon much earlier but De Morgan's introduction probably reassured Boole that he was working on the right track. Moreover, the concept of unity was of particular importance in both Hamilton's and De Morgan's theories [Laita 1976, 149-151; 1978, 61-64].

The elective equation  $vx=vy$  can be read according to Boole's comments above as "Some Xs are Some Ys", which is in form identical with Hamilton's respective <sup>form (4) in</sup> (63.3). At this point I would like to point out that in [LT, 228-9] Boole expressed in his symbolic language all the eight fundamental types of propositions adopting thus a scheme equivalent to that of De Morgan<sup>(3)</sup>:

Boole illustrates at the end of this chapter the claim that "all the equations by which particular truths are expressed, are deducible from any one general equation, expressing any one general Proposition, from which those particular Propositions are necessary deductions" [MAL, 23]. For a general equation he takes (74.13)  $x=y$

implying the equivalence of X and Y. Multiplying it with y we have

(74.14)  $y=xy$  or (74.15)  $(1-x)y=0$ ,  
interpreted as "All Ys are Xs" [see (74.3)].

Next he claims that (74.15) is regarded as an equation in which y is sought to be determined in terms of x, has as general

solution

$$(74.16) \quad y=vx$$

which implies that "All Ys are Xs" and that "Some Xs are Ys". Finally, he multiplies (74.16) with  $v$  and assuming implicitly that  $v$  is non-empty and  $v^2=v$ , he arrives at

$$(74.17) \quad vy=vx$$

which implies that "some Ys are Xs and some Xs are Ys" [MAL, 24] (""). The verification that (74.16) is the solution of (74.15) is at this point to be carried out by substituting the former in the latter thus deducing  $vy(1-y)=0$  which holds true due to the index law. The method by means of which (74.16) is derived will be discussed in the following section where we will illustrate the analogy between solutions of elective and differential equations.

Before proceeding to 7.5 we provide a list of Boole's complete system of all alternative symbolic expressions for the categorical propositions A, E, I and O, each accompanied by its auxilliary equations by means of which we can interpret  $v$  in it ("").

$$A \quad x(1-y)=0 \quad (1)' \quad E \quad xy=0 \quad (2)'$$

$$A, I \quad y=vx \quad (3)' \quad vx=\text{some Xs with } v(1-x)=0$$

$$E, O \quad y=v(1-x) \quad (4)' \quad v(1-x)=\text{some not-Xs with } vx=0$$

$$v=xy \quad (5)' \quad v=\text{some Xs or some Ys}$$

$$(74.18) \quad I \quad vx=vy \quad (6)' \quad vx=\text{some Xs, } vy=\text{some Ys}$$

$$vx(1-y)=0 \quad (7)' \quad v(1-x)=0, v(1-y)=0$$

$$v=x(1-y) \quad (8)' \quad v=\text{some Xs or some not-Ys}$$

$$O \quad vx=v(1-y) \quad (9)' \quad vx=\text{some Xs, } v(1-y)=\text{some not-Ys}$$

$$vxy=0 \quad (10)' \quad v(1-x)=0, vy=0 \quad [\text{MAL, 25}].$$

## 7.5 Solution of elective equations by substitution.

The distinction between Boole's two methods of solution of elective equations is clearly provided in his manuscript notes quoted in 7.2- 7.3 [N10; see 7.2, (2)]:



We can interpret each form as  $x, xy, x\overline{1-y}$  and when these are constituents we can interpret any aggregate of such forms and these are the only forms which we can interpret. And when we have a logical problem to solve by the solution of an elective  $=^n$  it is clear that unless our solution be brought to an interpretable form we cannot avail ourselves to it.

He next went on to claim that [N<sub>10</sub>]

The very possibility of reasoning depends upon the possibility of expressing a solution in an interpretable form. We might therefore substitute the most general interpretable form, for the symbol to be determined in the  $=^n$ , and determine the coeff. by comparison of the [...] numbers. But the method of development enables us to accomplish the more formal part of the analysis more readily and gives the same formal result which may be interpreted. The intermediate process does not involve the principles of interpretability but only the formal laws of the symbols.

Boole's general method in logic, or method of development, depends upon the theorem

$$(75.1) \quad \phi(x) = \phi(1)x + \phi(0)(1-x),$$

$\phi(x)$  elective function [MAL, 60-61]. Similar formulae are deduced for functions of more than one elective symbols. Substituting constants 1 and 0 for these symbols, numerical constants other than 1 and 0 appear in the intermediate processes of his logical calculus, and this is the point of distinction between what we called "basic logic" and "general method in logic" in 7.2.

Postponing to section 7.7 the discussion of the latter, formal, method as dependent on (75.1), we will present here his manipulation of elective equations as based on the method of solution by substitution. Despite the fact that he introduced this method in the last chapter of MAL, we consider it independent from the general theory laid in advance in the 6th chapter of MAL and also as important to discuss it at this point, since it will throw light on his treatment of conversion and syllogism.

We regard, together with Boole,

$$(75.2) \quad y = vx + v'(1-x),$$

$v, v'$  arbitrary elective symbols, as the most general interpretable form of an elective symbol  $y$  in respect to another  $x$ <sup>(1)</sup>. By substituting this form of  $y$  in the equation we want to solve,

we have to determine  $v$  and  $v'$  by comparison of coefficients as in the algebra of polynomials.

Chapter 7 "Of the solution of elective equations" opens with the following remarks [MAL, 70]:

In whatever way an elective symbol, considered as unknown, may be involved in a proposed equation, it is possible to assign its complete value in terms of the remaining elective symbols considered as known. It is to be observed of such equations that from the very nature of elective symbols they are necessarily linear<sup>(2)</sup>, and that their solutions have a close analogy with those of linear differential equations, arbitrary elective symbols in the one, occupying the place of arbitrary constants in the other. The method of solution we shall in the first place illustrate by particular examples, and afterwards, apply to the investigation of general theorems.

Let the equation under solution first be

$$(75.3) \quad (1-x)y=0.$$

$y$  wanted in respect to  $x$ . We substitute (75.2) in (75.3) and we have:

$$(1-x)(vx+v'(1-x))=0 \text{ ----> } vx(1-x)+v'(1-x)^2=0 \text{ ----> } 0 + v'(1-x) = 0$$

or

$$(75.4) \quad v'(1-x)=0.$$

We can now substitute (75.4) in (75.2) and arrive at

$$(75.5) \quad y=vx$$

as a consequence of equation (75.3).

But Boole reasoned as follows: Since (75.4) may be generally true, "without imposing any restriction upon  $x$ , we must assume  $v'=0$ , and there being no condition to limit  $v$ , we have  $y=vx$ . This is the complete solution of the equation. The condition that  $y$  is an elective symbol requires that  $v$  should be an elective symbol also (since it must satisfy the index law), its interpretation in other respects being arbitrary" [MAL, 70-71].

We thus have a more direct, though still not formal, method for the solution of (75.3) than that of verification by substitution of the result in the equation under solution suggested in the end of the second chapter [see 7.4]. In the list (74.8) provided there, equation (75.5), or (3)', is accompanied by the so called auxiliary equation  $v(1-x)=0$  and the interpretation of

$vx$  as "Some Xs". From this interpretation we gather that  $v$  can not denote an empty class. As a consequence, since  $y=vx$ ,  $y$  is nonempty too. But in algebraic terms  $y=0$  satisfies (75.3), thus (75.5) can not be regarded as the most general solution. Under this context it is a fallacy to infer (75.5) as logically implied from (75.3), a process which Boole often applies in his treatment of conversion and syllogism [on this fallacy see Corcoran 1980, 619-629].

However, we have evidence that he was aware that (75.5) was less general than (75.3) in the same way as  $xy=xz$  implies less general information than  $y=z$  [see 7.3]. But the very fact that in his subsequent manipulations he did regard (75.5) and (75.3) as equivalent can be explained in two ways: either, that Boole had implicitly dismissed from his system the possibility that  $y$  can obtain the value 0 in equations such as (75.3) -on the basis that otherwise (74.3) cannot be interpreted as a categorical proposition- or, that, aware of the so-called fallacy of solution, he committed it on the ground that no inconsistency of any sort was to follow, as in fact was the case with his system. Most probably, despite any analogies drawn from algebra or analysis, he did not view his calculus strictly algebraically and this will be evident in what will follow in this chapter.

The step  $v'=0$  following from (75.4) seems redundant. However, it is characteristic of Boole's reasoning in general -as we shall see in 7.7- and we can not pass it over. It is not necessary, though, that  $v'=0$  is implied from  $v'x=0$  when no restriction is imposed upon  $x$ , for we know that  $v'x$  can be zero while both  $v'$  and  $x$  denote non-empty classes. In fact, certain perplexities regarding his system arise from the peculiar function of this symbol.

Let us resume again (75.3) algebraically,  $y$  the quantity being sought. In general

$$(75.6) \quad y=(1-x)^{-1}0.$$

If  $x$  is a symbol of quantity,  $y=0$ . Let  $x$  stand for the operator  $d/dx$ . Then  $y=(1-d/dx)^{-1}0$ , or, we have

$$(75.7) \quad y=ce^x,$$

$c$  arbitrary constant. Thus,  $v$  in (75.5) corresponds to  $c$  in (75.7) and  $x$  to the function  $e^x$ .

The hint for this illustration is provided by Boole himself in a letter to Cayley. Soon after receiving MAL in December 1847 Cayley wrote to Boole perplexed on what ground  $\frac{1}{2}x + \frac{1}{2}x = x$  does hold true in Boole's system since not all algebraic operations, as for example division [see 7.3], are admitted in it<sup>(3)</sup>.

Boole answered:

I am glad to hear your objections because it gives me an opportunity of replying to them. When I speak of the operations of common algebra being applicable to my system, I mean of course the symbolical operations—those which depend upon laws of combination, not upon interpretation. Thus, if  $(z^2 - a^2)u = 0$  and  $z$  and  $a$  follow the laws of quantity we have:  $(z+a)u = (z-a)^{-1}0$ . If  $z$  and  $a$  are symbols of quantity the second member is 0 but not unless. If  $z$  were  $d/dx$  it would give a term involving an arbitrary constant but so far as operations alone are concerned we proceed with reference only to the laws of combination. The equation  $x(u-v)=0$  gives  $u-v=x^{-1}0$  whether the symbols are quantitative or elective but in the former case the result is equivalent to  $u-v=0$ —in the latter to  $u-v=w(1-x)$ ,  $w$  being an arbitrary elective symbol.

Thus, elective symbols resemble more differential operators than algebraic symbols. Moreover, he did regard terms such as  $x/2$  as uninterpretable, but nevertheless he admitted them in his system, claiming that "All that we can desire of a calculus of logic is that it should enable us to express any proposition and to interpret any equation and to represent any act of reasoning"<sup>(4)</sup> without bothering about uninterpretable terms. We will further discuss this point in subsequent sections.

Returning to our examples, equation

$$(75.8) \quad yx=0,$$

solved on grounds similar to (75.3), gives as solution

$$(75.9) \quad y=v'(1-x)$$

[MAL, 71].

We now proceed to an equation in three variables,

$$(75.10) \quad (1-x)zy=0$$

interpreted by Boole as "All Ys which are Zs are Xs". First we will provide a sketch of a proof given by him in a notebook which appears to be written prior to MAL. We will next provide his solution as in MAL, and finally illustrate the analogy between

the solution of the elective equation (75.10) and the same equation in which x,y stand now for differential operators.

$$\begin{array}{ll}
 "y(1-x)z=0 \quad z=? & \text{Let } x=0 \quad y=0 \\
 (i) \quad z=a+bx+cy+dx & x=0 \quad y=1 \\
 y(1-x)(a+bx+cy+dx)=0 & x=1 \quad y=0 \\
 & x=1 \quad y=1
 \end{array}$$

Then we have

$$\begin{array}{ll}
 0 \cdot a = 0 & a = v \\
 a + c = 0 & c = -a = -v \quad [\text{see C 73.14}] \\
 0 \cdot (a+b) = 0 & \\
 0 \cdot a + b + c + d = 0 & a + b = v' \\
 & b = v' - v \\
 & a + b + c + d = v' \quad \text{---->} \\
 & d = v' - v' + v \\
 \text{---->} \quad z = v + v' - vx - vy + v' - v' + v \cdot xy = \\
 & = v - vx - vy + vxy + v'x - v'xy + v'xy \\
 & = v(1-x)(1-y) + v'x(1-y) + v'xy \quad (5)
 \end{array}$$

This early method does not differ substantially from that exposed in MAL. Only now the most general expression for the symbol sought is given instead as in (i), in the form

$$(75.11) \quad y = v(1-x)(1-z) + v'(1-x)z + v''x(1-z) + v'''xz,$$

where  $v, v', v'', v'''$  are arbitrary elective symbols. Also, the sketch above reminds us of the algebra of polynomials. The process of substituting elective symbols by the constants 0,1 was to be further generalized in theorem (75.1) [see 7.7]. By substituting now the form (75.11) in (75.10) he deduced for solution, on grounds similar to (75.3) and (75.8):

$$(75.12) \quad y = v(1-x)(1-z) + v''x(1-z) + v'''xz,$$

where  $v, v'', v'''$  arbitrary. The "rigorous interpretation of this result is that Every Y is either a not-X and not-Z, or an X and not-Z, or an X and Z" [MAL, 71].

As was expected the two results are the same, only the second method is certainly less laborious than the first one. "It is deserving of note", wrote Boole after deducing (75.12), "that the above equation may, in consequence of its linear form, be solved by adding the two particular solutions with reference to x and z; and replacing the arbitrary constants which each involves

by an arbitrary function of the other symbol, the result is":

$$(75.13) \quad y = x\phi(z) + (1-z)\psi(x)$$

[MAL, 71].

Thus, analogy with differential equations is mainly drawn from the second, more general, method than from the early sketch (see (5) above). What he meant by the above remark is as follows: Solve  $(1-x)y=0$  and  $zy=0$  and add the partial solutions,  $y=vx$  and  $y=v'(1-z)$ , substituting  $v$  with  $\phi(z)$  and  $v'$  with  $\psi(x)$ . To prove the equivalence between forms (75.12) and (75.13) he regarded  $wz+w'(1-z)$  as  $\phi(z)$  and  $w''x+w'''(1-x)$  as  $\psi(x)$  getting from (75.13) the form:

$$y = wxz + (w' + w'')x(1-z) + w'''(1-x)(1-z).$$

Now, replace  $w' + w''$  by  $w'$  and whence the form derived is equivalent to the former solution (75.12) [MAL, 71].

Mentioning further an example of an equation in four variables, Boole went on to the following general remarks [MAL, 72]:

These instances may serve to shew the analogy which exists between the corresponding order of linear differential equations. Thus, the expression of the integral of a partial differential equation, either by arbitrary functions or by a series with arbitrary coefficients, is in strict analogy with the case presented in the two last examples. To pursue this comparison further would minister to curiosity rather than to utility. We shall prefer to contemplate the problem of the solution of elective equations, under its most general aspects, which is the object of the succeeding investigations.

We will end by illustrating the analogy mentioned above with equation (75.10) regarding  $x$  and  $z$  as  $d/dx$  and  $d/dz$  respectively. We thus have under solution the partial differential equation

$$(75.14) \quad \left(1 - \frac{d}{dx}\right) \frac{d}{dz} y = 0.$$

It follows that  $(d/dz) y = (d/dx - 1)^{-1} 0 = f(z)e^x$ , hence  $y = \int f(z)e^x dz + (d/dz)^{-1} 0$ , or

$$(75.15) \quad y = e^x \phi(z) + \psi(x).$$

Another way to write the above solution is in the form

$$(75.16) \quad y = e^x \phi(z) + z\psi(x) + (1-z)\psi(x).$$

In both cases  $\phi(z)$  and  $\psi(x)$  are analogous to the respective arbitrary elective functions in (75.13).

## 7.6 Examples of conversion, syllogism and hypotheticals in MAL.

Boole first stated the laws of conversion recognized by Whately: simple conversion by transposition of the terms, conversion by limitation and conversion by contraposition or negation [MAL,26]. He then illustrated with one example<sup>of</sup> each of these cases. As the most characteristic of his examples, we choose the one for conversion by limitation which holds for the cases of A and E.

The universal-affirmative and universal-negative were written<sup>respectively</sup> in the form

$$(76.1) \quad x(1-y)=0 \quad \text{and} \quad yx=0$$

[see list (74.18)]. Now, these equations give on solution respectively the forms

$$(76.2) \quad x=vy \quad \text{and} \quad x=v(1-y)$$

"the correctness of which may be shewn by substituting these values of  $x$  in the equations to which they belong, and observing that those equations are satisfied quite independently of the nature of the symbol  $v$ " [MAL,27]. Now, (76.2) are read as I and O respectively, hence we have conversion by limitation [MAL,27-28].

Boole's procedure in this case is in fact faultless, for, as he had mentioned earlier, an equation implied from another equation does not express quite ~~as~~ much as the primitive one [MAL,18-19,22-23]. But, no matter how the correctness of (76.2) is justified (see the alternative way of proving the validity of (76.2) in 7.5), he commits throughout the so-called "fallacy of solution", for, (76.2) are not the most general solutions of (76.1) and therefore cannot be implied from them strictly mathematically [Corcoran 1980,624;7.5]. He then proceeded to introduce a new approach to deal with conversion. In fact he produced a system "of independent laws of transformation" following, as he said, "a distinct mathematical process" [MAL 28,30]. Once more, with one example we will illustrate his approach assuming again as premises A and E as in (76.1).

This time Boole introduced  $v$  directly by multiplying with it the two forms in (76.1). As a result he got

$$(76.3) \quad vx(1-y)=0 \quad \text{and} \quad vxy=0.$$

Thus A was converted to I and E to O but notice that in the previous case I was read as "Some Ys are Xs", since v in the first equation of (76.2) was affixed to y, and here I is read as "Some Xs are Ys", for v is affixed to x. Thus a "universal Proposition may be changed into its corresponding particular Proposition" [MAL,30]. The introduction of v once more has as effect the limitation of "universal" to "particular" but more directly than before. Boole avoided the term "limitation" in his list of general transformations though. We can see v's function as that of an operator, say d/dx, when affixed to an equation, say  $\phi(x,y)=0$ .

Syllogisms were deduced in MAL on lines similar to algebraic elimination. Two schemes were used, the second stated only verbally:

$$\begin{array}{rcl}
 & ay+b=0 & b=ay \\
 & a'y+b'=0 & a'y=b' \\
 (76.4) \quad \text{-----} & \text{and} & (76.5) \quad \text{-----} \\
 & ab'-a'b=0 & a'b=ab'
 \end{array}$$

[MAL,32,34] <sup>(1)</sup>. He observed that solution of equations was necessary when both premises were of the form  $ay=0$ , for then the result of the elimination would be  $0=0$ . Only one of the two equations has to be solved and the choice is indifferent [MAL,32] <sup>(2)</sup>. Scheme (76.4) was used only once in the first of the four cases that Boole distinguished, and, as we shall see from the following example, the elimination is fallacious.

In the first case v does not enter in the premises. Let  
 (76.6)  $(1-x)y=0$  A and  $z(1-y)=0$  A.  
 He wrote the second premise as  $zy-z$ , and, using (76.4) he derived as conclusion  $z(1-x)=0$ , or "All Zs are Xs" [MAL,34]. We notice however, that  $zy-z=0$  is a tautology, in other words  $z(1-x)=0$  is deduced solely from the first premise which does not imply it, hence the elimination is fallacious [Corcoran 1980,627].

However, the next case, when the premises are of the form  $ay=0$  and thus v is introduced in the solution of one of them, is even more puzzling. We will first present Boole's procedure and then De Morgan's objections towards it in an unsent letter written in 1847.



Let A and E as in

$$(76.7) \quad y(1-x)=0, \quad zy=0.$$

The first is substituted by its solution [(75.5)], hence, applying scheme (76.5) we have:

$$\begin{array}{rcl} & y=vx & \\ & 0=zy & \\ (76.8) & \text{-----} & \\ & 0.1=vzx & \end{array}$$

or  $vzx=0$ , "Some Xs are not-Zs". Attention was called to the fact that the interpretation of  $v$  in  $vzx=0$  is strictly to be determined by  $y=vx$ , as "Some Xs" and not as "Some Zs" [see (74.18); MAL.35].

De Morgan suggested the following scheme:

$$\begin{array}{rcl} & y=vx & \\ & 0=zy & \\ & \text{-----} & \\ & y.0=vxzy & \end{array}$$

or  $vxzy=0$ . He then reminded Boole that division in his system is not allowable, " $xy=yz$  does not give  $x=y$ ". "I think with Mr Graves [see text below] that  $y=vx$  is the primitive form. But  $v$  is not a definite elective symbol, make it what you know it to be, and I think the difficulty vanishes".

He thus suggested a scheme devoid of  $v$ ,

$$\begin{array}{rcl} & y=yx & \\ & 0=zy & \\ & y.0=zxy^2 & \\ \text{or } & zxy=0 & (1) \end{array}$$

De Morgan read (1) as "some Zs are not Xs, the ZYs. But they are nonexistent. You may say that nonexistents are not Xs. A nonexistent horse is not even a horse; and, (a fortiori?) not a cow.

This is not suggested by your paper; but appears in my system". Finally De Morgan suggested (1) to be written as  $(xy)z$  read as "Those Xs which are Ys are not Zs". In fact he claimed that no elimination of the middle term is possible by lawful deductions in Boole's system [Smith 1982a.26-7].

What De Morgan tried to convey in this letter is that to carry out the elimination as in (76.8) means that we either replace the value of  $y$  from the first premise in the second, thus

$z(vx)=0$ , or we solve algebraically the second equation with respect to  $y$  and substitute the result in the first,  $y=0/z$ , hence  $0/z=vx$  or  $0=vxz$ . This suggestion seems exaggerated, but it implies that De Morgan saw that through his schemes for elimination Boole implicitly admitted division in his system. He wrote in that letter "I see that 0 must be treated as a magnitude in the form  $y*0 / y$  is 0: but  $0/y$  is not capable of interpretation. In fact, your inverse symbol is not interpretable, except where use of the direct symbol has preceded".

Summing up, Boole seems to eliminate  $y$  in (76.8) by multiplying the two equations obtaining  $0.y=vxzy$  and then eliminating  $y$  which is not lawful. De Morgan was critical enough to see this and the erroneous substitution of an equation by its solution objecting to the role of  $v$ . Moreover, he was more careful how to handle parentheses than Boole was!

In the third case, when  $v$  is found within one of the equations, but not introduced by solution, Boole gave an example not determined by the Aristotelian rules of deduction [MAL,37]. Its form is very similar to that of (76.8), thus both the process of the elimination and the interpretation of the result are called in question.

In the fourth case  $v$  enters into both equations and no inference is possible as the result of elimination is reduced to the form  $0=0$ . In fact the conclusion of the syllogism is in all such cases of the form

$$(76.9) \quad vv't=vv'z,$$

where  $t$  is of the form  $x$  or  $(1-x)$  (we have assumed that  $y$  is eliminated but this puts no restriction whatsoever).

Making use of the auxiliaries that accompany such expressions for  $I$  and  $O$  [see list (74.18)] he concluded from (76.9) that either  $vv'=vv'$  -in the case of  $II$ -or  $vv'=0$  -in the case of  $IO$ . The first case is reduced to  $0=0$ , therefore no conclusion can be derived from the premises [MAL,39]. But the second case also leads to  $0=0$ '<sup>3</sup>. Without noticing the redundancy of the second subclass, he claimed that this distinction between invalid cases where one class leads to  $0=0$  and the other to the "irreducibility of the final question to the form  $0=0$ " deserves attention and has been overlooked by logicians so far'<sup>4</sup>. Boole's sole criterion of

invalidity is the deducibility of  $0=0$  from the premises of the syllogism and their auxiliaries. Suprisingly he does not mention the method of counter-examples which Whately had made so much use of in his book [Corcoran 1980, 630,633].

Boole next compared system (74.8), by means of which he had primarily expressed the main 4 categorical propositions, with the following

	A	$y=vx$
(76.10)	E	$y=v(1-x)$
	I	$vy=vx$
	O	$vy=v(1-x)$ .

In the former system we are presented with the forms in which  $v$  must be employed, in the latter with those that it may. Influenced by C. Graves, he showed preference to the forms of A and E that involve  $v$  than to those that are independent of it. In COL and LT system (76.10) was put forward as giving the primary forms of A,...,O. For he regarded it simple, symmetrical and more convenient than the first one (74.8) [MAL,32-3,42-45; COL,128;LT,64] '3'.

The chapter on syllogism ended with the proof of the following theorem suggested by Graves:

"Given the three propositions of a syllogism prove that (76.11) there is but one order in which they can be legitimately arranged and determine that order" [MAL,45-47] '4'.

To deal with hypotheticals, he suggested certain modifications in the interpretation of the symbols of his calculus. Symbols  $X,Y...$  stand for propositions instead for classes and the hypothetical universe 1 comprehends now "all conceivable cases and conjectures of circumstances". As for  $x,y,z...$ , their function is that when attached to any subject expressive of such cases they select those cases in which the proposition  $X$  or  $Y$  is true. Finally,  $1-x$  selects those cases for which  $X$  is false [MAL,48-9].

For two propositions we have four distinct cases:

	X true, Y true	$xy$
(76.12)	X true, Y false	$x(1-y)$
	X false, Y true	$(1-x)y$
	X false, Y false	$(1-x)(1-y)$

and inductively for  $n$  propositions,  $2^n$  cases. Boole noticed that "however few or many those circumstances may be, the sum of the elective expressions representing every conceivable case will be unity" [MAL,50]. To express that a given proposition  $X$  is true we notice that the selection of those cases in which  $X$  is false gives 0, therefore  $1-x=0$  or

$$(76.13) \quad x=1.$$

Accordingly the expression that says " $X$  is false" is

$$(76.14) \quad x=0.$$

When  $X$  and  $Y$  are simultaneously true,

$$(76.15) \quad xy=1$$

and when both false,  $(1-x)(1-y)=1$ , or

$$(76.16) \quad x+y-xy=0.$$

If either one or the other is true, then it is not true that they are both false, hence  $(1-x)(1-y)=0$ , or

$$(76.17) \quad x+y-xy=1$$

[MAL,51].

Let us see now few examples of how he tackled conditional syllogism in MAL. The conditional proposition "If  $X$  is true,  $Y$  is true" was expressed as

$$(76.18) \quad x(1-y)=0.$$

He reasoned as follows: "Here it is implied that all the cases of  $X$  being true, are cases of  $Y$  being true". The former cases being determined by the elective symbol  $x$ , and the latter by  $y$ , we have in virtue of (74.3) equation (76.18) [MAL,54].

The following is an example of "complex constructive Dilemma, the minor premiss not exclusive". Let the premises be:

$$(76.19) \quad \begin{array}{ll} x(1-y)=0 & \text{If } X \text{ true, } Y \text{ true} \\ w(1-z)=0 & \text{If } W \text{ true, } Z \text{ true} \\ x+w-xw=1 & \text{Either } X \text{ is true or } W \text{ is true} \end{array}$$

[See (76.17), (76.18)]. Boole simply wrote: "From these equations, eliminating  $x$ , we have  $y+z-yz=1$  which expresses the Conclusion, Either  $Y$  is true, or  $Z$  is true, the members being non-exclusive" [MAL,56-57]. Elimination in the example (76.19) is carried out in fact by multiplying the third equation by  $(1-y)$  and the result with  $(1-z)$ , for we need to eliminate both  $x$  and  $w$ . We thus have:  
 $(x+w-xw)(1-y)=1-y \quad \text{---->} \quad w(1-y)=1-y \quad \text{---->} \quad w(1-y)(1-z)=(1-y)(1-z)$   
 $\text{--->} \quad y+z-yz=1.$

The chapter ended with Boole's observation of the distinction between propositions such as "All inhabitants (of an island) are either Europeans or Asiatics" and "Either all inhabitants are Europeans, or they are all Asiatics": He called the first proposition categorical and the second hypothetical. He then argued against the view of certain grammarians who "regard it as the exclusive office of a conjunction to connect Propositions, not words" [MAL,59,fn]'7'. However, despite his correct sensitivity to the above distinction in the structure of a proposition, he did not develop his idea any further. Neither in MAL, nor in LT did he include propositions that contained conjunction or disjunction of terms [Corcoran 1980, 633, 637 fn 16].

#### 7.7 Boole's general method in logic as in MAL.

According to the principles of Boole's calculus of logic [see 7.3-7.4] all logical statements under study were presented in the form of one or more than one elective equations. Dealing with Aristotelian logic occasionally a solution of an equation was required with reference to a particular variable [see 7.5-7.6]. But up to this point, any method applied for the solution of an elective equation was adapted to the problem under study<sup>11</sup>. Thus, a general method was needed in order to tackle elective equations in their arbitrary, general form.

Boole distinguished two cases: equations in the form

$$(77.1) \quad \phi(x,y,z\dots)=0 \text{ and } (77.2) \quad \phi(x,y,z\dots)=w,$$

where  $x,y,z,w\dots$  are elective symbols. By means of the theorem of expansion [see (77.9) below] equations (77.1), (77.2) would assume the form

$$(77.3) \quad a_1 t_1 + \dots + a_n t_n = 0 \text{ and } (77.4) \quad a_1 t_1 + \dots + a_n t_n = w$$

respectively;  $a_1, \dots, a_n$  are fractional numerical coefficients,  $t_1, \dots, t_n$  functions of  $x,y,z$  of the form  $xy(1-z)$ ,  $(1-x)(1-y)z$  etc.

The last, and most crucial step of his general method, involves the logical interpretation of the equations (77.3), (77.4). He provided two propositions, (77.20) and (77.29), by means of which (77.3)-(77.4) are reduced to simple equations,

such as  $(1-x)y=0$  or  $w=y+v(1-x)$ , interpretable in logic. Of all the possible fractional numerical values that  $a_i$  can assume, only four will appear in the final reduction. Namely the values 0, 1, 0/0 and 1/0 <sup>(2)</sup>. Boole substitutes  $v$  for 0/0 and by means of (77.29) suppresses any term that admits 1/0 as its coefficient; thus in the final step we are presented with familiar elective equations of basic logic, always interpretable, as we saw in the previous sections.

The demonstrations of the basic theorem of expansion, as well as of the propositions of interpretability, have certain weak points. Moreover, Boole's definition, in the sense of logical explanation, of 0/0 and 1/0 is poor and inconsistent with his former definition of  $v$  in 7.4. There are but very few examples in the end of the book to illustrate his theory. And though division is implicitly present in one of them, he totally omits any reference to it <sup>(3)</sup>. In what follows we will present all the main theorems of Boole's general method, as in MAL, focusing on certain mathematical elements involved. We will refer to the weak points involved in his proof procedures but we will postpone a deeper study of his free use of numerical coefficients, particularly 0/0, 1/0, for subsequent sections <sup>(4)</sup>.

"Since elective symbols combine according to the laws of quantity", wrote Boole, "we may, by Maclaurin's theorem, expand a given (elective) function  $\phi(x)$ , in ascending powers of  $x$ , known cases of failure excepted". Thus we have for any elective function which is supposed to be developable in ascending powers of  $x$

$$(77.5) \quad \phi(x) = \phi(0) + \phi'(0)x + \phi''(0)x^2/1.2 + \dots$$

According to the index law,  $x^n = x$ , (77.5) was written as

$$(77.6) \quad \phi(x) = \phi(0) + x[\phi'(0) + \phi''(0)/1.2 + \dots].$$

Substituting 1 for  $x$  he obtained from (77.5) the following formula:

$$(77.7) \quad \phi'(0) + \phi''(0)/1.2 + \dots = \phi(1) - \phi(0).$$

Finally, combining the latter with (77.6) he arrived at

$$(77.8) \quad \phi(x) = \phi(0) + [\phi(1) - \phi(0)]x$$

which can also be written as

$$(77.9) \quad \phi(x) = \phi(1)x + \phi(0)(1-x)$$

[MAL, 60-61] <sup>(5)</sup>.

Thus, "Every function of  $x$ , in which integer powers of that

symbol are alone involved, is by this theorem reducible to the first order", commented Boole. He called the quantities  $\phi(0)$ ,  $\phi(1)$ , "moduli of the function  $\phi(x)$ " and regarded them as of great importance in the theory of elective functions exposed in the 6th chapter of MAL [MAL, 61].

To reinforce the justification of this purely analytical proof, he suggested in a footnote an alternative method to prove (77.8), based on the formula of expansion in finite differences. In addition he cited the results of two other expansions which terminate at the second term as (77.8). The content of this footnote is greatly indicative of Boole's mathematical influence at that time and, though later he put forward a principle which rendered a purely analytical device such as Maclaurin's theorem redundant [see 8.5], it is of great interest to examine closely how he linked his general method in logic with his *general method* in analysis.

Boole suggested to develop  $\phi(x)$  according to the formula

$$(77.10) \quad \phi(x) = \phi(0) + \Delta\phi(0)x + \frac{\Delta^2\phi(0)}{1.2} (x-1) + \dots$$

Since  $x$  is an elective symbol,  $x(x-1)=0$ , and thus all the terms after the second vanish. Also, he wrote  $\Delta\phi(0)=\phi(1)-\phi(0)$ , and hence (77.8) follows at once [MAL, 60-61,fn]. This demonstration presents a simplicity of which Boole makes no note at all. That is, while in order to obtain (77.6) from (77.5) one has to assume the extension of distributivity for infinite terms, here this assumption is redundant. But he did not bother to comment upon this assumption, and it is quite likely that he did not notice it.

Next Boole wrote that a mathematician may be interested in the remark that (77.8) is not the only case in which an expansion stops at the second term. So, he reminds the reader of two formulae deduced in his paper on developments written shortly after his [1844], namely of

$$(77.11) \quad \begin{aligned} \phi(d/dx + x^{-1}) &= \phi(d/dx) + \phi'(d/dx)x^{-1} \\ \phi(x + (d/dx)^{-1}) &= \phi(x) + \phi'(x)(d/dx)^{-1} \end{aligned}$$

[1845d, 219-220] '6'. Obviously, the fact that (77.8) is of the

first order, is due to the peculiarity of the index law, the key feature of elective symbols [see also 7.5 (2)]. However, he regarded this fact<sup>as</sup> remarkable and directly associated it with a similar<sup>made</sup> observation<sup>in</sup> [1845d, 219] where he had claimed that (77.11) stop at the second term "regardless of the form of  $\varphi$ ".

Encouraged by this association he wrote in that footnote that though the theorems (77.8), (77.10) and (77.11) "have only been proved for those forms of functions which are expansible by MacLaurin's theorem, they may be regarded as true for all forms whatever; this will appear from the applications. The reason seems to be that, as it is only through the one form of expansion that elective functions become interpretable, no conflicting interpretation is possible" [MAL, 60 fn]'. In fact, as we shall see in [8.2, (6)], Boole had doubted that his method, as primarily based on his theorem of expansion, would prove applicable to all possible cases of elective equations. When he realised that by obtaining the propositions of interpretability (77.20)-(77.29), he felt that his rather bold procedure was justified.

Boole observed that theorem (77.9) can be extended to functions of more than one variable. Suppose we have a function  $\varphi(x,y)$ , denoted by him as  $\varphi(xy)$ . Since  $x,y$  obey the same laws as quantitative symbols, that is commutativity and distributivity, we can expand  $\varphi(xy)$  with reference to  $y$ ,

$$(77.12) \quad \varphi(xy) = \varphi(x0) + [\varphi(x1) - \varphi(x0)]y$$

and then expand the result with reference to  $x$ ,

$$(77.13) \quad \varphi(xy) = \varphi(00)(1-x)(1-y) + \varphi(01)y(1-x) + \varphi(10)x(1-y) + \varphi(11)xy,$$

"the order of expansion being quite indifferent" [MAL, 62].

Among the first direct implications of (77.9) was that if

$$(77.14) \quad \{\varphi(x)\}^{\sim} = \varphi(x)$$

holds true, then (77.14) is also satisfied by the moduli of  $\varphi(x)$  and similarly for functions of more than one variable [MAL, 61-63]. Below he proved the inverse of this statement. In other words, if the moduli of a function  $\varphi(xyz\dots)$  are all either 0 or 1, that is they admit only of these numerical values which obey the index law, then condition (77.14) is satisfied by  $\varphi(xy\dots)$ . This condition, which introduces "symmetry into our calculus, and provides us with fixed standards for reference" was to be further



developed in LT as the basis of a perfect method [MAL,61-63,66; See also 8.6].

In the most general case let us have a function  $\phi(xy\dots)$  of  $m$  variables. The number of the moduli  $a_i$  is easily found to be  $2^m=r$ . When expanded,  $\phi$  will assume the form

$$(77.15) \quad \phi(xy\dots) = a_1 t_1 + \dots + a_r t_r.$$

The terms  $t_i$  are combinations of the forms  $x, y, z, \dots, 1-x, 1-y, 1-z$ .

It readily follows by inspection of (77.9), (77.13) that

$$(77.16) \quad t_i \cdot t_j = 0, \quad i, j = 1, \dots, r \quad \text{and} \quad (77.17) \quad t_i \cdot t_j = 0, \quad j \neq i,$$

that is,  $t_i$  are mutually exclusive. Boole called  $t_i$  the constituent functions of  $\phi$  [MAL,63-64] (' $\theta$ ').

Another property follows readily by inspection:

$$(77.18) \quad \text{The sum of all the constituents of an expanded function is unity.}$$

Expand, for example 1 with reference to  $x$  and  $y$ . It will follow from (77.13) that

$$(77.19) \quad 1 = xy + x(1-y) + (1-x)y + (1-x)(1-y).$$

Next follows the theorem for the interpretation of equation (77.1). Let a random term be of the form " $a_i t_i$ ",  $a_i \neq 0$ . Then, it is proved that:

$$(77.20) \quad \text{Equation } \phi(xy\dots) = 0 \text{ implies that } a_i t_i = 0 \text{ and the combined interpretation of these several equations will express the full significance of the original equation.}$$

To prove (77.20) we multiply  $a_1 t_1 + \dots + a_r t_r = 0$  successively with those  $t_i$  whose coefficients  $a_i \neq 0$ . Then, according to the properties (77.16), (77.17), we have

$$(77.21) \quad a_i t_i = 0$$

Since  $a_i$  is a numerical constant  $\neq 0$ , he deduced from (77.21) that

$$(77.22) \quad t_i = 0,$$

an equation always interpretable in logic [MAL,64-5]. For the proof to be acceptable rigorously a principle has to be added in the list of laws of Boole's system, namely that

$$(77.23) \quad ax = 0 \rightarrow x = 0,$$

where  $a$  is any rational number  $\neq 0$  (' $\theta$ ').

As an application of this theorem take  $\phi(xy) = x-y$ . Then, the equation

$$(77.24) \quad x-y=0,$$

$X$ s and  $Y$ s are identical, is reduced, when  $\phi(xy)$  is expanded ac-

according to (77.13), to

$$(77.25) \quad x(1-y)-y(1-x)=0.$$

Now, (77.25) is equivalent by means of (77.20) to the system

$$(77.26) \quad x(1-y)=0, \text{ All } Xs \text{ are } Ys; \quad y(1-x)=0, \text{ All } Ys \text{ are } Xs$$

[MAL,65]. We notice that the negative coefficient  $-1$  of  $y(1-x)$  does not affect the reduction of (77.25) to a system of interpretable equations [see also (86.7)-(86.8)]. Boole remarked that if the simultaneous satisfaction of equations thus deduced, as (77.26), may require that an elective symbol vanishes this would only imply the nonexistence of a class. Moreover, if it happens that such a deduction leads to a result of the form

$$(77.27) \quad 1=0,$$

this would indicate the nonexistence of the logical universe. "Such cases", he wrote, "will only arise when we attempt to unite contradictory Propositions in a single equation" [MAL,65].

Thus, (77.27) is a criterion of nonconsistency. It is also implied that Boole regarded the unit operator as distinct from the zero operator, and property

$$(77.28) \quad 1 \neq 0$$

should also be included in the list of the fundamental laws of his system [Hailperin 1976,91]. "It appears from this Proposition [(77.20)]", wrote next Boole, "that the differences in the interpretation of elective functions depend solely upon the number and position of the vanishing moduli. No change in the value of a modulus, but one which causes it to vanish, produces any change in the interpretation of the equation in which it is found" [MAL,65] (10).

Suppose now we have under solution (77.2), or  $\phi(xyz\dots)=w$ . Expanding  $\phi$  as before in a series of terms of the form "at", we have the second theorem of interpretation which says that:

"We shall be permitted to equate separately to 0 every  
(77.29) term in which the modulus  $a$  does not satisfy the condition  $a^n=a$  and to leave for the value of  $w$  the sum of the remaining terms."

The proof of (77.29) is carried out on lines analogous to that of (77.20). Let for simplicity (77.2) when expanded to assume the form:

$$(77.30) \quad w=a_1t_1+a_2t_2+a_3t_3+a_4t_4$$

with  $a_1^n = a_1$ ,  $a_2^n = a_2$ ,  $a_3^n = a_3$ ,  $a_4^n = a_4$ . Squaring (77.30) and subtracting (77.30) from the result we have  $(a_3^2 - a_3)t_3 - (a_4^2 - a_4)t_4 = 0$ . Now, this case can be tackled by theorem (77.20) since it is of the form (77.1) and the coefficients of  $t_3$ ,  $t_4$  do not vanish. Hence, (77.30) is finally reduced to

$$(77.31) \quad w = a_1 t_1 + a_2 t_2$$

accompanied by the subsidiary equations

$$(77.32) \quad t_3 = 0 \quad \text{and} \quad t_4 = 0$$

[MAL, 66] <sup>(11)</sup>. Equations (77.32) are interpretable but  $a_1$ ,  $a_2$  may admit beside 0,1 which obey the law  $a^n = a$ , also the value 0/0. Boole will comment upon this coefficient in the last chapter of MAL.

Chapter 6 ends with two more properties of elective functions. These properties were not applied in MAL and were totally omitted in LT. For this reason we will not present their proofs but solely state them as indicative of Boole's desire to develop the possibilities of his method and enlarge the list of the properties of elective functions. He first proved that:

$$(77.33) \quad \psi(a_1 t_1 + \dots + a_r t_r) = \psi(a_1) t_1 + \dots + \psi(a_r) t_r,$$

whatever the values of  $a_i$  or the form of  $\psi$ . The next theorem is an application of the former. It says that:

$$(77.34) \quad \text{"Whatever process of reasoning we apply to a single given Proposition, the result will either be the same Proposition or a limitation of it"}^{(12)}.$$

The last paragraph at page 69 is indicative of the spirit under which Boole composed his method:

The purport of the last investigation will be more apparent to the mathematician than to the logician. As from any mathematical equation an infinite number of others may be deduced, it seemed to be necessary to shew that when the original equation expresses a logical Proposition, every member of the derived series, even when obtained by expansion under a functional sign, admits of exact and consistent interpretation.

In the final chapter, the 7th, he considered the solution of elective equations of the form (77.1) and the reduction of a system of equations to one equation equivalent to it. He distinguished between two methods for the solution of (77.1). One involves the method of substitution, already expounded in 7.5,

whereas the other involves processes analogous to those of symbolical algebra. He showed preference though to the latter method which was the only one to be presented in COL and LT.

Let us take the equations

$$(77.35) \quad \varphi(xy)=0 \quad \text{and} \quad (77.36) \quad \varphi(xyz)=0,$$

y and z to be determined respectively. By means of the first method he developed  $\varphi(xy)$  in (77.35) based upon (77.13), replaced y from formula (75.2) in the result of the expansion, determined the values of v, v' in (75.2) and thus obtained

$$(77.37) \quad y = \frac{\varphi(10)}{\varphi(10)-\varphi(11)} x + \frac{\varphi(00)}{\varphi(00)-\varphi(01)} (1-x)$$

[MAL,72-3].

Equation (77.36) was solved by the second method as follows: We expand  $\varphi(xyz)$  only with reference to z by (77.9) and solving algebraically for z we get

$$(77.38) \quad z = \frac{\varphi(xy0)}{\varphi(xy0)-\varphi(xy1)}.$$

The right-hand side of (77.38) is meaningless; but this might startle, he said, only those unaccustomed to the processes of symbolical algebra. But Boole, as he was repeatedly to mention when objections were raised towards this algebraic procedure, did admit of uninterpretable terms in the intermediate steps of his deductions, <sup>being</sup> interested mainly in the final restoration of the result within logic. Now, we expand  $\varphi(xy0)$  etc in (77.38) as functions of x,y by means of (77.13) and finally obtain z in the form

$$(77.39) \quad \begin{aligned} z = & \frac{\varphi(110)}{\varphi(110)-\varphi(111)} xy + \frac{\varphi(100)}{\varphi(100)-\varphi(101)} x(1-y) + \\ & + \frac{\varphi(010)}{\varphi(010)-\varphi(011)} (1-x)y + \frac{\varphi(000)}{\varphi(000)-\varphi(001)} (1-x)(1-y). \end{aligned}$$

One who may doubt the correctness of the result, wrote Boole, may verify the conclusion by the method of substitution [MAL,73].

Before proceeding to illustrate the above theory <sup>with</sup> examples of logic, he commented briefly upon 1/0 and 0/0. "The values of the moduli  $\varphi(00)$ ,  $\varphi(01)$ , etc being constant, one or more of the

coefficients of the solution may assume the form 0/0 or 1/0 . In the former case, the indefinite symbol 0/0 must be replaced by an arbitrary elective symbol v. In the latter case, the term, which is multiplied by a factor 1/0 (or by any numerical constant except 1), must be separately equated to 0, and will indicate the existence of a subsidiary Proposition" [MAL,74]. Boole's arguments and explanations as in the quotation above are very poor, and his procedure far from clear (see further remarks in 8.5, 8.7).

We will examine two examples, the other two that are given being quite similar to them. First take the equation

$$(77.40) \quad x(1-y)=0, \text{ "All Xs are Ys",}$$

y to be determined. Boole put  $x(1-y)=\phi(xy)$ , determined the moduli  $\phi(10)$  etc of  $\phi(xy)$  and by means of (77.37) arrived at

$$y = \frac{1}{1-0}x + \frac{0}{0-0}(1-x) = x + \frac{0}{0}(1-x), \text{ or,}$$

$$(77.41) \quad y = x + v(1-x),$$

v an arbitrary elective symbol. Thus "Y consists of the entire class X with an indefinite remainder of not-Xs. This remainder is indefinite in the highest sense, i.e. it may vary from 0 up to the entire class of not-Xs" [MAL,74].

Now take the proposition "All Ys are Zs and not-Xs" given by

$$(77.42) \quad y(1-z(1-x))=0.$$

To find the class Z we solve (77.42) formally by means of (77.39)

$$\text{and obtain} \quad z = y(1-x) + \frac{0}{0}x(1-y) + \frac{0}{0}(1-x)(1-y) + \frac{1}{0}xy. \text{ According}$$

to (77.29) and Boole's comments upon 0/0 and 1/0, z is reduced to

$$(77.43) \quad z = y(1-x) + vx(1-y) + v'(1-x)(1-y)$$

accompanied by the subsidiary proposition

$$(77.44) \quad xy=0.$$

Thus "No Ys are Xs", and Z consists "of all Ys which are not Xs and an indefinite remainder of not-Ys", for he suggested (77.43) to be written as:

$$(77.45) \quad z = y(1-x) + (1-y)\phi(x)$$

[MAL,75-76].

Once more under the influence of the solution of differen-

tial equations , he applied Lagrange's method of indeterminate multipliers in order to reduce a system of elective equations to one equation and study the relations connecting the elective symbols. He proved in a sort of appendix in the end of chapter 7 that given three equations  $\phi(xyz)=0$  ,  $\psi(xyz)=0$  and  $\chi(xyz)=0$ , then the equation

$$(77.46) \quad \phi(xyz)+h\psi(xyz)+k\chi(xyz)=0,$$

h, k arbitrary constants (not elective symbols), is equivalent to the given system. In other words we can obtain the value of x, y or z independent of h and k in its most general form and any subsidiary relation which can exist between the remaining variables.

The proof of this equivalence is rather long and complicated. Its procedure , mainly algebraic , is based upon the formula (77.39) [see also 8.6]. Omitting the proof which justifies the use of indeterminate multipliers [MAL.78-81] we will provide the unique example dealt with this method.

Suppose we are given the equations

$$(77.47) \quad x(1-z)=0 \quad , \quad z(1-y)=0$$

interpreted in the usual form of A. According to the method mentioned above, <sup>the system</sup> (77.47) is equivalent to

$$(77.48) \quad x(1-z)+\lambda z(1-y)=0.$$

To determine z we apply formula (77.39) to the function  $\phi(xyz)=x(1-z)+\lambda z(1-y)$ ,  $\lambda$  regarded as a numerical constant, and thus obtain

$$(77.49) \quad z=xy+\frac{1}{1-\lambda}(1-y)x+\frac{0}{0}(1-x)y.$$

Now Boole implicitly assumes that  $1/(1-\lambda)$  is a numerical constant other than 0, 1 and 0/0 (or simply he regards  $\lambda \neq 0$ ) which therefore satisfies the law  $a \cdot 0 = 0 \cdot a \neq 0$  and due to (77.29), (77.49) is reduced to the equations

$$(77.50) \quad z=xy+v(1-x)y \quad \text{and} \quad (77.51) \quad x(1-y)=0.$$

Thus, "Z consists of all Xs that are Ys, with an indefinite remainder of not-Xs that are Ys; the latter, that all Xs are Ys, being in fact the conclusion of the syllogism of which the two given Propositions are the premises" [MAL.76]. Thus, by means of a combination of his general method in logic and the method of indeterminate multipliers, a system of equations can be solved

and the elimination of the variable under determination is effected.

Boole remarked in the end of this chapter that all the equations discussed so far can, by assigning an appropriate meaning to the symbols, be considered as examples of hypotheticals. He also observed that every elective equation is reducible to a system of equations  $t_i = 0$ , with  $t_i \cdot t_j = 0$  when  $i \neq j$ . In other words "all categorical Propositions are resolvable into a denial of the existence of certain compound classes, no member of one such class being a member of another" [MAL,77] <sup>(13)</sup>. He wondered how an affirmative proposition can be constituted solely by a system of negations as argued above. Focusing on categorical propositions, he answered that "there is a Universe of conceptions, and that each individual it contains either belong to a proposed class or does not belong to it". This assumption provides the positive element sought above.

Thus the theory of categorical propositions (and, by analogy, of hypothetical ones) rests at once upon "a positive and upon a negative foundation". The positive foundation involves the existence of a universe of conceptions within which each variable of the proposition either represents or does not represent a given class. In his words the positive foundation "contemplates the particular as derived from the general". Now, the negative foundation concerns the restrictions we have to impose upon the given variables, or, it is "ever proceeding by limitations" [MAL,78] <sup>(14)</sup>. From these very last comments the metaphysical dualism of Boole's reasoning in logic is once more apparent. This reasoning can in fact be applicable to other branches apart from logic. For example, while dealing with singular solutions of differential equations in his [1859] Boole distinguished once more a positive and a negative element [see 8.8] .

Summing up briefly we have seen the following:

- 1) Elective symbols are often treated in a manner similar to differential operators [7.3,7.5].
- 2) Algebraic techniques - division implied within the general method - are applied including the admittance of uninterpretable terms, a lawful procedure of symbolic algebra [7.5-7.6].
- 3) Analytical techniques such as Maclaurin's theorem and

Lagrange's indeterminate multipliers are used as means of demonstration [7.7].

4) Heuristic links are provided to show the analogy between <sup>the</sup> calculus of logic and <sup>the</sup> calculus of operators [7.5,7.7]

5) Various instances such as our remark above about singular solutions prove that Boole was contemplating upon a universal calculus of symbols, logic and mathematics being two distinct domains where it could be applied<sup>15</sup>.



## Chapter 8

Boole's logic: 1847-1864; a comparison of Laws of thought [1854] with Mathematical analysis of logic [1847a].

### 8.1 Introduction

In chapter 7 we focused on Boole's first work on logic, MAL[1847a], and its mathematical background. Drawing by analogy from the laws of differential operators, Boole laid down the principles of his basic logic [7.2-7.4]. He next applied his calculus of logic to numerous examples of traditional logic, thus gaining assurance for the power of his method [7.6]. The secret of his procedure lay in the ability not only to formulate a problem of logic as an elective equation, but also to solve every such equation and interpret the result in terms of logic. Two methods of solution were suggested: the first was broadly similar to the solution of differential equations, [7.5], the second formed the core of his general method in logic and was based on the expansion theorem (7.9), [7.7].

Boole provided very little explanation for either of these two methods for the solution of elective equations in MAL. His admittance of uninterpretable terms in the course of his symbolic procedures puzzled some of his contemporaries. Among them was the 23-years-old Cayley: letters exchanged between these two mathematicians in December 1847 form an interesting source of information on certain obscure aspects of Boole's logic in MAL [see 7.5, (3)].

Up to 1854 only two minor papers on logic were published by Boole. The first, COL [1848b], was a summary of MAL [7.1]. This paper is indicative, as we shall see, of certain changes that took gradually place in the development of his reasoning in logic during the transitional period 1848-1853 [8.3]. The second, entitled "MM. [sic] Boole's theory of the mathematical analysis of logic" [1848c], consisted of Boole's brief answer to Cockle's remarks on German philosophy<sup>11</sup>.

During this period we also notice Boole's lecture, "Claims" [1851], where for the first time he talked of the philosophical

interpretation of his equation (71.1) stressing the role of the concept of universe and unity, as well, as viewing mathematics and logic as two distinct branches of a universal calculus of symbols [7.1, 8.2].

This transitional period has largely been overlooked by historians so far. Moreover, though detailed studies have been carried out on his two major work on logic, MAL and LT, no comparison has been drawn between these two books. Often LT is called Boole's masterpiece [see 7.1, (20)]. However, on one hand the content of this book is mostly based upon Boole's discoveries in MAL, and on the other hand, due to the changes that took place during the transitional period [8.3], certain aspects of his method are deprived of their early context within which they had been initially conceived [see 7.2, (4)]. Boole's new attitude, more coherent from some points of view, presents certain elements worth noting.

We will compare LT with MAL in stages. First we will draw from Boole's correspondence with Cayley and from his manuscript notes  $N_7-N_{27}$  [see 7.2, (2)] in order to clarify certain obscure points of MAL. Through this clarification our views on MAL will be further reinforced [8.2]. Another essay entitled "Sketch of a theory and method of probabilities founded upon the calculus of logic" (hereafter cited as "Sketch") will serve both as a means for this clarification, and also in order to observe the gradual changes that occurred in the period 1848-1853 [8.3]. This essay, edited by [Rhees 1952, 141-166], was written late in 1848 or early in 1849. It was not designed for publication but was meant so as to register Boole's actual state of knowledge [Smith 1982a, 34]. Quotations from  $N_7-N_{27}$  and "Sketch" will show that the former notes had the same character as "Sketch" and were written prior to it, around 1848.

Having dealt with the period 1847-1853 in 8.2 and 8.3 we will proceed to a study of LT. Section 8.4 will cover the new grounds upon which Boole based his basic and formal logic in LT. The operation of division will be discussed in 8.5. Our study of LT and its comparison with MAL will end with a few comments on his new processes for elimination, reduction and syllogistic deduction [8.6].

The last three sections will be devoted respectively to Boole's manuscript papers on logic written as a sequel to LT, his views on symbolical mathematical procedures as in his textbook on differential equations [1859], and finally to a study of his epistemological views including a comparison with Gräy's analogous views in his Logique [1855].

## 8.2 Clarification of certain aspects in MAL founded upon Boole's writings: 1847-49.

In this section we consider Boole's thoughts on:

1. The nature of numerical coefficients in logic,
2. The admittance of uninterpretable terms, such as  $x+x$ , in the course of logical procedures,
3. The role of mathematics and ordinary language in Boole, and finally,
4. The notions of universe and unity.

In both  $N_7-N_{27}$  and "Sketch" Boole tried to convey that the idea of number is not peculiar to arithmetic but is an element employed for the purposes of reasoning in general. The notion of number links arithmetic (mathematics) with logic: two subjects which form however, two independent branches of symbolical reasoning. In the passage that follows below we see the spirit under which he admitted uninterpretable terms in logic in analogy with mathematics. Finally, drawing from the Notes  $N_7-N_{27}$  and Boole's letter to R.Latham in 1855, both the formulation of  $x+x$  and its interpretation will be considerably clarified. In the notes  $N_7-N_{27}$  we read:

If we had a problem to solve relative to some particular numbers such as 10, 20, 30, and could not conceive of others -we might investigate the laws to which 10 20 30 are subject and employ general symbols subject to these laws finally interpreting the result- the intermediate steps being uninterpretable for other numbers [...]. In fact we may employ signs to represent particular conceptions determine the laws of those signs operate in accordance with these laws so as to pass through forms quite uninterpretable with reference to the original conception [...]. Here the dominion of the laws is more general than the particular operations in which

they are first observed to prevail [...] . (For example) in applying the method of indeterminate multipliers in the calculus of Logic we pass through uninterpretable forms to forms finally interpretable through development. Number is introduced in the Constant multiplier.

There is an important distinction between the interpretation of the symbols in any proposed form of analysis and the forms under which these symbols enter into the analytical expression. There seems to be no limit theoretically to the former but the latter are apparently fixed and are the same for all symbols. Suppose  $x, y, z$ , to represent any operation, we can then only combine them mentally by distribution or succession, and even some of these modes may give uninterpretable results. We can conceive of such forms as  $(x+y)1$   $xy1$   $xx$  and can in various instances interpret them and determine the laws of combination. But the very laws of combination have reference to special forms. An operation [...] may be regarded either as producing its effect coordinately with some other operation or in a determinate succession with reference to that operation or with reference to itself by repetition.

From this repetition we got  $xx$  or  $x^2$ ,  $xxx$  or  $x^3$  whence we have the idea of number (as first appearing in logic). Or we have  $x+x=2x$ ,  $x+x+x=3x$  whence also the idea of number. In the latter case it must be supposed that the  $x1$  in  $x1+x1+x1+\dots$  refer to different or mutually exclusive entities so that we may have the possibility of aggregation. We can also conceive of the existence and investigate the nature of an operation which being performed  $n$  times may produce the same effect as the operation  $x$  performed  $m$  times and thus we get  $x^{m/n}$ . And we can conceive of an operation the result of which being aggregated  $n$  times shall give the same result as the operation  $x$  aggregated  $m$  times, i.e. of  $\frac{m}{n}x$  and from these considered, single operations, we have by the principle of aggregation already stated  $ax^a+bx^b+cx^c$ ,  $abc$   $a\beta\gamma$  being integral or fractional<sup>(1)</sup>.

In the above passage Boole tried to justify his symbolic procedures in logic drawing from instances of symbolic algebra. The style of his writing is quite different from that displayed in his published works, and judging also from the fact that cer-

tain points, such as the formulation of  $ax^a+bx^b+\dots$ , are not to be found elsewhere in his work, it is rather certain that he wrote these notes in order to analyse his thoughts for the sake of his own study. The formulation of  $x+x$  is explained though, only on formal grounds. The conceptual foundation of the formulation of this term is given in analogy with ordinary language in the opening of a letter of Boole to Latham, dated 22 May 1855<sup>(2)</sup>. We read:

I differ from you in the first place because I do not think that the "true plural" is what you take it to be. I do not think that there is any identification of the several individuals which it comprises with one of them [...]. The plural of "I" is not according to my view I+I+I... . A plural is a name for a collection of individuals agreeing in the properties of that quality which is denoted by the singular but differing to any extent in other respects. "Men" is not This man + This man + This man... But This man + That man + Another man... as all the individuals agreeing in the properties of humanity<sup>(3)</sup>.

Thus, it becomes evident that Boole viewed aggregation of members of the same class in logic as equivalent to the plural of a name in ordinary language. On these grounds, he claimed further below in his letter, " $x+x+x\dots$  n terms is not the same as " $x$ " which is in fact the representative of  $xxx\dots$  n terms"<sup>(4)</sup>. Summing up, the  $x_1$  in  $x_1+x_1+\dots$  in our first quotation stands for different representatives of the class of  $Xs$ , whereas in  $x\dots x_1$ , it stands in an abstract manner for a random individual who has this property. Only under this analysis we can admit the term  $x+x$  or  $2x$  in Boole's system. And, since according to his theory [7.7] any numerical coefficient, such as 2, disappears in the final result, the uninterpretability of  $x+x$  in logical terms can be ignored. Thus, definitely the law

$$(82.1) \quad x+x=x$$

cannot be included in his system, as various logicians or historians have claimed<sup>(5)</sup>.

We now proceed to some further clarifications on how he viewed numerical coefficients, other than 0 and 1, in his system. The passage below is drawn from a letter to Cayley dated 10

December 1847 . What is mostly interesting about this quotation is that we learn how unexpected discoveries forced Boole to believe that his method not only was sound, but, that in fact, through it, he did discover the laws of reasoning. The confidence thus gained is particularly evident in LT.

And now for the numerical constants. They never appear in the expression of a proposition and all the details can be accomplished without them. Instead of taking 1 for the universe I might take u -but 1 answers quite as well and is more simple. The equation "All Ys are Xs" is  $y=vx$ , or, on multiplying by  $1-x$ ,  $y(1-x)=0$  of which the former is a solution and here and in every similar case both of expression and operation there is nothing that cannot be interpreted. This remark applies to all that I have said in the Chapters of Conversion, Syllogism and Hypotheticals. In the chapter on the General Properties of Elective functions I proposed a different object, viz what are the properties of a function  $\phi(xyz)$  in which  $xyz...$  are elective symbols but the function unrestricted so as to allow of its involving numerical Constants. You will allow that this was a thing which I was at liberty to do. The general result is that any equation  $\phi(xyz)=0$  is reducible to the form  $a_1t_1+a_2t_2+...+a_nt_n=0$  in which  $a_1, a_2...a_n$  are numerical constants or 0 and  $t_1, t_2...t_n$  constitute elective functions which are interpretable in logic -and further that this equation is resolvable into a series of equations of the form  $t=0$  all interpretable in logic, Prop, 2nd, p 64 [(77.20)] , so that the numerical elements disappear altogether from the final result. I did not anticipate this, I thought it exceedingly unlikely that every equation  $\phi(xyz)=0$  should be interpretable but when I found that this was the case and that it gave us the solution of equations to general theorems I accepted it not only as a proof that the laws I had investigated<sup>d</sup> were really the laws of thought but also as a means of giving to the process of the calculus an analytical generality and simplicity which they could not otherwise have had<sup>(4)</sup>.

Young Cayley showed a suprisingly strong interest in comprehending Boole's procedures. In particular he was extremely puzzled with the appearance of numerical coefficients and the initial point put forward by him was "How  $\frac{1}{2}x + \frac{1}{2}x = x$ " and "what  $\frac{1}{2}x$

stands for" in his first letter, dated 2 December 1847. This question motivated Boole to set out to clarify his own procedures and the above quotation inspired Cayley to a <sup>lengthy</sup> answer. However, in order to be more specific, Boole invented an argument drawn from trigonometry in order to justify the appearance of uninterpretable terms. This argument he was to use thereafter with confidence despite the fact that Cayley was far from convinced by it.

Boole claimed that the equation  $\frac{1}{2}x + \frac{1}{2}x = x$  is analogous to  $\sqrt{-1} \times \sqrt{-1} = -1$  in a system of pure quantity. Cayley disapproved of this analogy and wrote that an analogous difficulty in the sphere of mathematics would be "what does  $\frac{1}{2}d/dx$  mean but that there is no difficulty about"<sup>(7)</sup>. Boole insisted in his next letter that an interpretation of  $x/2$  in logic was equally needless as that of  $\sqrt{-1}$  in algebra or geometry. He stressed that "it never occurs except in an equation and all equations are interpretable in logic. I wish you would just consider this question. Can anything more be required than the expression of any proposition, the interpretation of any equation and the derivation of any results that exist?".

This question sums up indeed the very basis of Boole's reasoning in logic. And thanks to Cayley's insistence he provided some clarifications about the obscurities of his exposition in MAL. At the end of this letter, dated 8 December 1847, he wrote: "I hope now you have set to work to examine my principles you will not stop short but prove them to the bottom. I do not fear the result. I had rather have one such reader as you than a thousand who take everything for granted".

Boole must have been really touched by Cayley's interest. Comparing their correspondence with those between Boole and De Morgan or Jevons we notice that it is exceptional in consisting of detailed letters in an attempt to discuss the subject under question from all possible angles. However, the common point in all such correspondences is a lack of flexibility on either side which finally leads to "correspondence without communication"<sup>(8)</sup>.

Boole drew another analogy from imaginaries in order to persuade Cayley. He claimed that just as when  $a$  and  $b$  are real the equation  $a+bi=0$  leads to  $a=b=0$ , so  $\phi(xyz\dots)=0$  is reduced, by

means of (77.20) , to a system of interpretable equations of the form  $t=0^{\prime\prime}$ . Despite Cayley's numerous objections he felt content with his arguments and wrote in the appendix of ["Sketch", 164-5]:

The introduction of the numerical constants  $c$ ,  $c'$  etc, into this system, may perhaps be objected on the following ground. We can interpret  $x$  as a logical symbol it may be said, but we cannot interpret  $cx$ . A satisfactory answer can be given to this objection upon more than one distinct ground. In the first place, it may be observed that in every branch of analysis the formal laws of combination of the symbols are of wider extent than the laws of their interpretation [...]. Thus in the arithmetic of sines we employ the form  $\sqrt{-1}$ , the representation of an operation quite uninterpretable in arithmetic, with perfect security.

The same argument was put forward also in [LT, 69] in a passage regarding the conditions of valid reasoning by the aid of symbols. But even Venn, who followed closely Boole's account in his Symbolic Logic [1881], was not to accept this analogy<sup>(10)</sup>.

In the appendix of "Sketch" Boole followed a style very similar to that of his notes  $N_7-N_{27}$  cited in the beginning of this section. What is particularly evident from the following quotation from "Sketch", is the fact that he viewed numbers as universal symbols, equally necessary in logic and mathematics, employed on similar formal grounds but in a different sense:

The introduction of numerical constants into my calculus, the symbols of which represent operations, of whatever kind they may be, is however in itself perfectly consistent. We conceive of an operation as capable of being repeated. Hence the idea of integral numerical quantity. With the idea of operation is also connected the antithesis of the Direct and the Inverse, and it is in this way that we pass from the idea of integral number to that of fractional number [see details in  $N_{12}$ ]. It hence follows that the idea of Number is not solely confined to Arithmetic, but that it is an element which may properly be combined with the elements of every system of language which can be employed for the purposes of general reasoning, whatsoever may be the nature of the subject. I think it important to notice that, while Number thus properly and naturally may be employed in the logical symbols themselves,



(except perhaps the 0 and the 1), are not in any sense numerical. Let  $x, y, z$  represent three entirely distinct classes of things, together filling up the Universe of discourse. Then  $x+y+z=1$ . Here none of the symbols,  $x, y$  and  $z$ , can be replaced by 0, because none of the classes is supposed to vanish; nor by 1, because otherwise the sum of the three would be more than 1; nor by any other numerical value, because no other numerical value satisfies the law  $x^2=x$ . Thus they are not numbers, but signs used in subjection to the laws of thought as manifested in language<sup>(11)</sup>.

By now the concept of numerical coefficients and the ways in which they are permitted to be employed in Boole's logic are clarified considerably. Further comments on the analogies and differences between mathematics and logic are to be found in the notes N<sub>7</sub>-N<sub>27</sub>. On page N<sub>13</sub> we read:

Both the system of elective symbols and the system of numerical magnitude set out from the same point -the consideration of the whole or unity. In the case of magnitude we are led to contemplate different wholes [...] in the system of elective symbols we have but a single whole, the Universe whether it be the actual Universe or some portion of it to which the discourse is limited [...]. A great distinction between the system of elective symbols and that of quantity is that in the former the equation may relate to several distinct classes of things included in the Universe 1- in the former [latter] we only relate to one description of thing viz the unit itself to which all the numerical coefficients are supposed to apply. In the mathematics of quantity we proceed by the combination or repetition of the unit. In the mathematics of Logic we proceed by the analysis of the Unit. The analysis of the Unit is effected by the combination or repetition of operations and hence regarding one of these operations as a whole we have quantity introduced into the elective equation. The language of quantitative mathematics is independent of the particular nature of the unit. The language of analysis in general is therefore independent in its forms of the particular nature of the operation represented by the operating symbol<sup>(12)</sup>.

Nowhere in Boole's published work are we to find such a

penetrating analysis of the dissimilarities in the foundations of logic and mathematics. Moreover, we notice his emphasis on the concept of unity which played a determinant role throughout his work in both these subjects. Their link and independence through the different analysis of the unit is remarkably exposed. For Boole this sort of account consists the philosophical ground upon which he based his investigations.

The passage from "Claims" (1851) that follows below is less analytical. At once we perceive the difference between personal thoughts and an account prepared for a lecture:

All correct reasoning consists of mental processes conducted by laws which are partly dependent upon the nature of the subject of thought. Of that species of reasoning which is exemplified in Algebra, the subject is quantity, the laws are those of the elementary conceptions of quantity and of its implied operations. Of Logic the subject is our conception of classes of things represented by general names; the ultimate laws are those of the above conceptions, and of the operations connected therewith. Let these two systems of thought be placed side by side, expressed as they admit of being, in the common symbolical language of mathematics, but each with its own interpretations -each with its own laws; and together with much that is obviously common so much indeed, as to have fostered the idea that Algebra is merely an application of Logic, there will be seen to exist real differences and agreements hitherto unnoticed, but not without the influence on the course of human thought<sup>(13)</sup>.

The material quoted so far gives evidence of Boole's standard views on logic and its link with the science of number. Above all we have to remember that number, as a concept, is not to be confined to arithmetic only, but it can be combined with every system of language which is employed for the purposes of general reasoning independently of the subject under discussion. At this point I recall a quotation from "Claims" (1851, 194-5), provided in 7.1, where Boole viewed mathematics as "universal reasoning expressed in symbolical forms".

### 8.3 Modifications in Boole's logic: 1848-1853.

We now proceed to observe various modifications which started getting place in Boole's system around 1848-49. Up to the notes  $N_7-N_{27}$  the index law was presented in the initial form

$$(83.1) \quad x^n = x$$

[see (73.10),  $N_9$ ]. Soon after it was reduced to the form

$$(83.2) \quad x^2 = x$$

["Sketch", 141]. In LT the reason for doing so was given in a footnote. Let  $x^3 = x$  be in the form  $x(1-x)(1+x) = 0$ . Since  $1+x$  is uninterpretable, equation  $x^3 = x$  admits of no interpretation in his system analogous to that of  $x^2 = x$  and hence it is discarded<sup>(1)</sup>.

The operational character of elective symbols gradually fades away. In MAL we noticed that Boole hinted upon the analogy between elective symbols, as  $x$ , and differential operations, as  $d/dx$ , more than once [7.3, 7.5]. Also, in the notes  $N_7-N_{27}$  the operational element is apparent. In fact, he draws an analogy between elective symbols and discontinuous integrals regarded as multipliers in the calculation of definite multiple integrals. Moreover, these multipliers are subject solely to the values 0 and 1<sup>(2)</sup>.

However, in [COL, 126],  $x_1$  or  $x$  is associated with the class  $X$ , whereas in LT,  $x, y, \dots$  stand merely for classes instead of operational symbols which select elements from these classes [LT, 29, 47, 66-71]. A turning point was "Sketch". There,  $x, y, z$  "are names expressive of qualities or attributes, and as such are used according to the laws of naming" ["Sketch", 145]. But in the first appendix we read "Thus the adjective, and every attributive expression, whether single or many-worded, possesses a limiting, or as I have termed it elsewhere [MAL] an elective power" ["Sketch", 162]<sup>(3)</sup>.

Another change hinted at already in MAL [7.4, 7.6], concerns the primary forms for A and E. Instead of  $x(1-y) = 0$  and  $xy = 0$  [(74.8)] we now have their solutions  $y = vx$  and  $y = v(1-x)$  respectively. As a consequence, a different reasoning for their justification had to be introduced and thus  $v$  appeared as necessary in universal propositions and not mainly in particular

ones<sup>4</sup>. The reason for doing so was symmetry [see (74.9), (76.10)]. But the former system (74.8) is often regarded as superior to the latter (76.10): see [COL, 128-0; "Sketch", 148; LT, 61]<sup>5</sup>.

Solution of elective equations was to be carried out from 1848 onwards only by means of his general method [(77.38), (77.39)]. Boole avoided any reference to the method studied in 7.5, or to the analogy between elective and differential equations he had perceived in MAL [see COL, 131-134; "Sketch" 153; LT, Chapter 6]. The influence of his mathematical background, in particular of his general method in analysis, was not as obvious, as it was in MAL.

A similar effect is to be observed in connection with syllogistic deductions. In COL [135-140] they are carried out on lines similar to those in MAL [7.6]. But from then onwards the premises of a syllogism were viewed as elective equations developable and solvable according to the general method. This had as a result the distinction of two cases only, but also the introduction of much more complicated formulae [see "Sketch", 145-153; LT, 232-239; 8.6].

A most crucial theme in Boole's logic is the operation inverse to multiplication. In basic logic we saw that division was excluded from the fundamental operations in his calculus. The same attitude, even more emphatically as we will see in 8.5, was to be followed in LT. However, through the general method, division was implicitly effected as in example (77.40) <sup>6</sup>. But only in LT do we come across the notation  $x/y$  or  $x/x$  so we postpone any discussion on division to the section 8.5.

Of interest is to notice <sup>however,</sup> Boole's attempt to incorporate division as a logical operation in his notes  $N_7-N_{27}$ . He gives a list of the fundamental operations  $xy$ ,  $x+y$ ,  $x-y$  including (83.3)  $x^{-1}y = xy + \overline{v1-x1-y}$ , with  $y(1-x)=0$ . He then proceeded to state the axioms of inference [(73.13), (73.15)]. He added also the following axiom, absent in earlier or later works:

(83.4) "Equal inverse operations of division give indefinite results which are equal on the proper determination of the indefinite function" [ $N_9$ ].

Further below in these notes , under the title "of Inverse operations", he wrote briefly:

It has been remarked that beside considering the results of direct operations we can propose to determine the nature of a subject upon which the performance of a given known operation shall produce a given known result. It would appear that question -is in its very nature indefinite [...] . It is like the inquire into cause. If a particular state of things as cause produce a particular effect we cannot from this alone be sure that there exists no other state of things which will produce the same effect<sup>(7)</sup>.

The first evidence we have on an interaction between logic and probabilities is in "Sketch". On this matter Boole held correspondence with the British scientist and astronomer J.W.Lubbock. On 16 February 1849 he announced to him that he had recently made a fundamental step in the theory of probabilities. In his next letter he wrote: "I have found that all questions of pure logic are reducible to the applications of one general method however numerous and however complicated the premises<sup>(8)</sup>". In LT Boole would dedicate 6 chapters on the application of logic to probabilities. This subject is considerably studied recently, and, as it is beyond the scope of this thesis, we will not delve into it [see Hailperin 1976, chaps. 4-5; 1988].

Our study of this period will end with few remarks on the concept of time. A first hint upon the notion of duration was given by Boole in the notes N<sub>7</sub>-N<sub>27</sub>. At page N<sub>12</sub> he wrote "It is clear from the above [The first quotation in 8.2] that the idea of number is introduced by the repetition of an operation as in successive periods of duration. Or is equally introduced by the consideration of individuals of the same kind and defined by a common name simultaneously existing". In ["Sketch", 146] he was more explicit:

Accordingly I have in the work above referred to [MAL], interpreted the symbols  $x, y, z, \dots$ , as here expressing the cases in which those elementary propositions are true [...]. And without stopping here to assign the reason upon which that interpretation is founded, I shall simply state that it consists in regarding the symbols  $x, y, z$  as representing the times in which the elementary

propositions to which they refer are true [...]. The law whose expression is  $x^2=x$ ,  $y^2=y$  etc will equally be satisfied, and the numerical values 0 and 1 will be equally admissible with this system of interpretation -the former as the representative of the nothing of time or never: the latter as the Universe of time, which when unlimited is Eternity, when limited the duration to which our discourse refers.

Thus, Boole dropped around 1849 the idea of "cases" or "conjunctures of circumstances" [see 7.6] for the notion of time for which a proposition is true. De Morgan must have had objected towards Boole's new approach as we see from Boole's letter dated 23 February 1854: "Do you now admit the validity of my theory of Secondary Propositions [Hypotheticals] and their connexion with Time?" [Smith 1982a, 65].

According to Jourdain [1910, 351] Boole's approach was regarded as a curious metaphysical ~~view of the role of~~ time, and it "must be considered irrelevant to logic just as Sir W.R.Hamilton's idea that "algebra is the science of pure time" is irrelevant". Hailperin showed recently that Boole's early approach in MAL was superior to that in [LT, 159-184] which was based on the notion of time<sup>(?)</sup>.

8.4 LT: the new grounds of Boole's basic logic and general method in logic.

From a very first glance at LT we notice that its main difference with MAL lies in the increased emphasis paid to the general method of logic and in the decreased interest in Aristotelian syllogism. The order of exposition is reversed and the center of attention considerably altered. For, in MAL, Aristotelian logic, treated mainly by what we called "basic logic" [7.2. (6)], covered almost two thirds of the book, whereas in LT it occupied a single chapter, the 15th, and was investigated by means of a slightly modified and greatly enlarged formal method. In MAL the mathematical treatment of examples of traditional logic had led to the invention of the formal method. In LT it was the formal method that was suggested as the ultimate

method of logic and the examples from Aristotelian logic that followed served solely as one possible field for an illustration of its applications.

Boole sounds in LT much more authoritative and confident than in MAL. It had been not anticipated by him, while composing his earlier work, that every logical (elective) equation admitted of solution and interpretation. But, having discovered that this was so, he was totally certain that what he had created was the ultimate calculus of deductive reasoning, by means of which the laws of thought were expressed in mathematical form<sup>(1)</sup>. In fact, the form of this calculus, claimed Boole, was unique; for it was dictated by the laws of thought which rendered this mode possible forbidding any other form. Now, why "the ultimate laws of Logic are mathematical in form", wrote Boole, "is a question that lies "beyond the reach of our faculties" [LT, 11].

To justify the necessity of his general method, which was to occupy most of the material in LT devoted to logic, he strongly denied that all inference can be reducible to conversion and syllogism. Still, had this been the case, he wrote, a general method would have been absolutely indispensable. Complex examples of logical deduction, as well as difficult questions in the theory of probabilities, demanded the power of a general method which in the sciences of mathematics - "the most perfect examples of method known" - "constitutes its chief office and distinction" [LT, 10-11].

Boole put forward his views on language<sup>(2)</sup>, and then he went on to define "sign" as "an arbitrary mark, having a fixed interpretation, and susceptible of combination with other signs in subjection to fixed laws dependent upon their mutual interpretation" [LT, 25]. He then defined the language of his calculus in a formal way which was absent in MAL. He constructed a list of literal symbols,  $x, y, z$  which represent "things as subjects of our conceptions", i.e. classes; of three signs of operations,  $+, -, \underline{x}, \wedge$  "by which the conceptions of things are combined or resolved so as to form new conceptions", and of one sign of identity" =<sup>(3)</sup>. In the end of this list he added that: All these symbols of logic "are in their use subject to definite laws, partly agreeing with and partly differing from the laws of the

⊗ [x sign of multiplication] 554

corresponding symbols in the science of Algebra" [LT, 27].

Signs  $x, y, z$  were called "descriptive", operational signs  $+$ ,  $-$ ,  $\times$  were those of "mental operations", and identity was the sign of "relation". By means of the first class of signs we express a thing, quality or circumstance belonging to it, by the second we collect parts into a whole or separate a whole into parts, and finally by the third we may form propositions [LT, 27, 32, 34].

Drawing from the constitution of ordinary language, Boole formulated the fundamental laws of logic (73.5), (73.8) and (73.9) (in other words, the commutative, distributive and index laws). Boole provided rules for the formulation of logical terms drawing by analogy from language. For example he wrote: "We are permitted, therefore, to employ the symbols  $x, y, z$  etc, in the place of the substantives, adjectives, and descriptive phrases subject to the rule of interpretation, that any expression in which several of these symbols are written together shall represent all the objects or individuals to which their several meanings are together applicable, and to the law that the order in which the symbols succeed each other is indifferent" [LT, 29-30] <sup>(4)</sup>.

A main difference with the earlier presentation at this point was that the concept of the universe of discourse was not yet formally introduced. Moreover, Boole introduced the laws that follow which were omitted in MAL:

$$(84.1) \quad x+y=y+x \qquad (84.2) \quad x-y=-y+x$$

and

$$(84.3) \quad z(x-y)=zx-zy$$

[LT, 33-35; see also (73.12)]. Boole now emphasized that  $+$  means "and" or "or" and is used to join distinct (disjoint) classes. On this basis subtraction can be defined. Transposition is also introduced. Let  $x$  stand for stars,  $y$  for suns and  $z$  for planets. Then  $x=y+z$  and by transposition  $x-y=z$ . Implicitly it is held that  $x-y$  can be formed only if the class  $x$  includes the class  $y$  [LT, 33-35]. In other words  $x-y$  equals, by definition,  $x(1-y)$ .

In chapter 4 it was observed that the expression "Things which are either  $x$ 's or  $y$ 's" has two symbolical forms depending on whether  $x$  and  $y$  are exclusive or not. These expressions,



respectively are

$$(84.4) \quad x(1-y)+y(1-x)$$

$$(84.5) \quad xy+x(1-y)+y(1-x) \text{ or } x+y(1-x)$$

[LT,56] '='. Thus, in Boole's basic logic,  $x+y$  stood either for (84.4) or for (84.5). However, within formal logic,  $x+x$  did not stand for  $x+x(1-x)=x$  (as would have followed from (84.5)). We discussed this point in length in 8.2. In fact, as we will notice in our study, though both basic and formal (or general) logic are consistent within themselves, inconsistencies do arise when we regard the latter as based upon the former.

Boole mentioned the rules of inference (73.13) in words and proceeded to examine the inverse of (73.16),

$$(84.6) \quad x=y \rightarrow zx=zy.$$

Here, more emphatically than in MAL, he claimed that algebraic division has no formal equivalent in logic. Moreover, he wrote: "I say no formal equivalent, because, in accordance with the general spirit of these inquiries, it is not even sought to determine whether the mental operation which is represented by removing a logical symbol,  $z$ , from a combination  $zx$ , is in itself analogous with the operation of division in Arithmetic" [LT, 36-37]. Thus, despite admitting the formula (83.3), i.e.,  $x^{-1}y=xy+v(1-x)(1-y)$  with  $y(1-x)=0$

in his notes  $N_7-N_{27}$  around 1848, as the 4th fundamental operation in logic, Boole emphatically excluded logical division—that mental operation which is commonly termed "Abstraction"—as an operation of "basic logic". Still, (83.3) would appear later in the book as the result of the application of his "general method" for the solution of logical equations [see (85.11)].

At the end of chapter 2 on signs and their laws he speculated over the index law,  $x^2=x$ . This "special law" is satisfied only by two numerical symbols, 0 and 1, observed Boole. Hence, he wrote, "instead of determining the measure of formal agreement of the symbols of Logic with those of Number generally, it is more immediately suggested to us to compare them with symbols of quantity admitting only of the values 0 and 1" [LT,37].

Consequently Boole suggested in [LT, 37-8] the conception of an algebra

in which the symbols  $x,y,z,\dots$  admit indifferently of the values 0

and 1, and of those values alone. The laws, the axioms, and the processes, of such an Algebra will be identical in their whole extent with the laws, the axioms, and the processes of an Algebra of Logic. Difference of interpretation will alone divide them. Upon this principle the method of the following work is established.

Indeed, upon a slight modification of the above principle [see (84.11)], he would justify his general method founded upon the theorem of development (84.13). How far, though, is this principle a novelty of LT? And how far is Boole's algebra of logic analogous to an algebra of 0 and 1?

In the above form this principle is indeed absent in MAL. However, the proof of the development theorem (77.9) was implicitly based upon it, as well as the possibility to formulate meaningless term such as (77.38) or to solve elective equations by substitution, [7.5, (5)]. Moreover, certain principles of symbolic algebra in general were made use of in MAL, and the possibility to substitute these general variables by the numerical values 0 and 1 was more than once taken advantage of. But now Boole is more conscious of these possibilities and is willing to make the most out of them.

In his paper [1873] on Boole, A.J.Ellis noticed that there can be no effective correspondence between the interpretation of the symbols of these two algebras. Boole's calculus admit of three distinct possibilities: all, some or none (1, v and 0) whereas the algebra he draws from by analogy has only two cases, 1 and 0. Though Boole is often unclear about his definition of v, he mentions that "some" includes "all" but not "none" [LT, 124] ('<sup>4</sup>).

In the third chapter of LT the fundamental laws of logic,  $xy=yx$ ,  $x^2=x$  and  $x+y=y+x$ , are confirmed by the study of the operations of the human mind on lines similar to those in the first chapter of MAL [LT, 44-45; 7.3]. Surprisingly the distributive law,  $x(y+z)=xy+xz$ , is missing here. The operation of election makes a rare appearance in LT in the following sense: "as the word men directs us to select mentally from that Universe all the beings to which the term "men" is applicable" [LT, 42-43]. By universe of discourse he refers to "the ultimate subject of the

discourse"; "If that universe of discourse is the actual universe of things, [...] , then by men we mean all men that exist" [LT, 42]. Thus, this notion is restricted to existing objects only [compare with MAL, 15; 7.3, (1)].

Faithful to the principle stated in the end of his previous chapter, Boole went on to define the logical symbols 0 and 1 by analogy with the respective numerical symbols of the algebra of 0 and 1. Once more, both the style and order of introduction of these logical symbols differ substantially from those employed in MAL. Since 0 in algebra satisfies the law

$$(84.7) \quad 0y=0$$

for any number  $y$ , 0 in logic should stand for that class which satisfies (84.7) regardless of the class represented by  $y$ . "A little consideration will show that this condition is satisfied if the symbol 0 represent Nothing". In fact "Nothing" apparently stands for what we call the empty class ( set). He then went on to observe that "Nothing and Universe are the two limits of class extension, for they are the limits of the possible interpretations of general names, none of which can relate to fewer individuals than are comprised in Nothing, or to more than are comprised in the Universe" [LT, 47]'.<sup>2</sup>

Similarly, since the number 1 satisfies

$$(84.8) \quad 1y=y$$

"it appears that the [logical] symbol 1 must represent such a class that all the individuals which are found in any proposed class  $y$  are also all the individuals  $1y$  that are common to that class  $y$  and the class represented by 1. A little consideration will here show that the class represented by 1 must be "the Universe", since this is the only class in which are found all the individuals that exist in any class" [LT, 47-48]. Hence, what was previously in MAL defined as universe, 1, which therefore obeys the law (84.8) in the form  $y1=y$ , or (73.1), now it is interpreted as such through this law of arithmetic due to Boole's principle.

However, he did not bother to include

$$(84.9) \quad 1 \neq 0$$

as an axiom of his logical calculus. He most probably implicitly held that this is so, since in arithmetic (84.9) is obviously

true. It is to be reminded though, that he was quite emphatic about (84.9) in MAL [see (77.27), (77.28)]. Next he defined the contrary or supplementary class of  $x$  as  $1-x$ , and, writing the index law in the form  $x-x^2=0$ , based on the laws of transposition and combination, he deduced as a theorem

$$(84.10) \quad x(1-x)=0$$

proving thus the following proposition [LT,49]:

That axiom of metaphysicians which is termed the principle of contradiction, and which affirms that it is impossible for any being to possess a quality, and at the same time not to possess it, is a consequence of the fundamental law of thought, whose expression is  $x^2=x$ .

R.L.Ellis wrote soon after reading LT in 1855 a short paper, now included in his Mathematical Writings [1863], commenting admiringly upon Boole's work. Among other remarks he denied that (84.10) expressed in virtue of antecedent conventions the principle of contradiction. He rightly based this objection on the fact that contrary to ordinary language, where independently of this principle we can express negation, in LT there is no other means of expressing "not red" than by  $1-x$ ,  $x$  denoting red. Now the interpretation of this symbol  $1-x$  seems to me to be given by the principle of contradiction, and therefore I should rather say that the equation  $x(1-x)=0$  is interpreted by that principle than that it expresses it. In accordance with this view the equation  $x^2=x$  would appear to be independent of the principle of contradiction" [Ellis 1863: 394]. This remark seems to have escaped the notice of contemporary commentators on Boole. It was, however, emphasized by Harley in his Report in 1870<sup>(8)</sup>.

The third chapter of LT ended with the remark that the index law can not be expressed as an equation of a degree higher than the second due to the uninterpretability of terms such as  $1+x$  or  $-1-x$  [LT, 50, fn]. As a consequence of  $x^2=x$  we can perform, noticed Boole, "the operation of analysis and classification, by division into pairs of opposites, or, as it is technically said, by dichotomy". Somewhat prophetically he added:

"Now if the equation in question had been of the third degree, still admitting of interpretation as such, the mental divi-

sion must have been threefold in character, and we must have proceeded by a species of trichotomy, the real nature of which it is impossible for us, with our existing faculties, adequately to conceive, but the laws of which we might still investigate as an object of intellectual speculation.

He thus called equation (84.10) "the law of duality" [LT, 50-51] ('?').

Basic logic, and thus the preliminaries for the foundation of the general method established in chapters 5-6, is completed in the fourth chapter of the book. Now categorical propositions are called "primary" ones and hypotheticals "secondary" propositions [LT, 52]. Categorical proposition A is no longer introduced as  $x(1-y)=0$ , but as  $y=vx$ . Accordingly, on lines similar to COL [8.3, (4)]  $v$  was introduced as the symbol for "some" when A is read "All Xs are some Ys". He thus wrote that  $v$  is "the symbol of a class indefinite in all respects but this, that it contains some individuals of the class to whose expression it is prefixed". Moreover, he explicitly stated what was implicitly assumed in MAL, that  $v$  obeys the index law [LT, 61, 63; 7.4 (4)].

Formal logic is introduced in chapter 5. Boole lays down the conditions of valid reasoning by symbols which we have presented in 7.2 [LT, 69]. According to these conditions the formal processes of reasoning are independent of the interpretability of the symbols [for his argument drawn from trigonometry see 8.2 text and (10)]. Hence, he argued, we may regard logical symbols as quantitative ones during the intermediate steps of logical processes.

The principle introduced earlier [LT, 37-38] is now accordingly reformulated as follows [LT, 69-70]

We may in fact lay aside the logical interpretation of the symbols in the given equation; convert them into  
(84.11) quantitative symbols, susceptible only of the values 0 and 1; perform upon them as such all the requisite processes of Solution; and finally restore to them their logical interpretations.

Without bothering to provide numerous applications of basic logic—as he had done in MAL—Boole aimed <sup>at</sup> establishing the

foundations of his formal logic by means of which he was to show that the following problem is resolved in all its aspects [LT, 10]:

Given a set of premises expressing relations among certain elements, whether things or propositions: required explicitly the whole relation consequent among any of those elements under any proposed conditions, and in any proposed forms.

(84.12)

We know that the main tool for this method of formal logic is the development theorem (77.9) or,

$$(84.13) \quad f(x) = f(1)x + f(0)(1-x).$$

Boole's respective process in LT differs once more from that followed in MAL. His new process of development, rests upon two issues: dichotomy and the principle (84.11), both of which are founded upon two distinct forms of the index law;  $x(1-x)=0$  and  $x^2=x$  respectively.

Given any two classes  $y$  and  $x$  in logic, it was argued that  $y$  can be divided in two portions with respect to the property  $x$ , according to the principle of duality, (84.10). This mental division, he claimed, is anteceded of all knowledge of the constitution of the class derived from any other source". Given further information, i.e. that the  $Y$ 's of property  $x$  are also defined by a property  $u$ , etc., we have

$$(84.14) \quad y = ux + v(1-x)$$

[LT, 70-71]. This form was used in MAL for the solution of elective equations but had lacked any explanation, most probably because Boole had not been explicit about the duality law [see (75.2)].

Now, (84.13), which provides the development of a logical function is to be deduced formally from (84.14) which represents the development of a class on logical ground. Boole defines hence  $f(x)$  as "any algebraic expression in  $x$ " and calls it "developed" if it can be reduced to the form

$$(84.15) \quad f(x) = ax + b(1-x).$$

To prove that every function  $f(x)$ ,  $x$  logical symbol, can be developed it suffices to assume (84.15) and determine the coefficients  $a$  and  $b$ . Based on the principle (84.11) he successively replaced  $x$  in (84.15) by 0 and 1 and obtained the theorem

(84.13) [LT, 72] (1847):

We can see that now, due to principle (84.11), Maclaurin's theorem is redundant. "To some it may be interesting to remark", wrote Boole in a footnote, "that the development of  $f(x)$  obtained in this chapter, strictly holds, in the logical system the place of the expansion of  $f(x)$  in ascending powers of  $x$  in the system of ordinary algebra". He then presented the proof earlier introduced in MAL [(77.5)–(77.9)] ending by saying "This demonstration in supposing  $f(x)$  to be developable in a series of ascending powers of  $x$  is less general than the one in text" [LT, 72–73 fn].

This last remark may be one basic reason why Boole dropped the early proof for that based upon (84.15). Moreover, the total absence, in what follows in text, of any discussion about analogy between solution of logical equations and differential ones shows that Boole was not in LT under the same strong influence of his early papers on analysis as he was in MAL. He confined only to taking advantage of any analogy possibly drawn between his algebra of 0 and 1 and his calculus of logic. However, the principle (84.11) seems not to be a novelty peculiar to LT as we saw above in text. Thus, the latter proof might had been apparent to him as a possibility while composing MAL. If that were the case, then Boole must had preferred then the strictly mathematical proof aiming to render the theorem more valid to the mathematicians to whom MAL was primarily addressed [MAL, 69; see 7.7].

## 8.5 LT: the study of division in Boole's logic

As we saw in 7.7, division, in the form  $x/y$ , was not explicitly apparent in MAL. In this section, drawing mainly from LT, we will comment upon the operation of logical division and the coefficients  $1/0$  and  $0/0$ . To further elucidate our account we will also draw from material written by Boole around 1855 and his textbook [1859] on differential equations.

Of most importance for the sound establishment of Boole's formal method is chapter 6 on interpretation. In this chapter his aim is to show how the general problem (84.12) can be fully resolved. Denoting by  $V$  a function in  $x, y, z, \dots$  he distinguished three forms of logical equations:

(85.1)  $V=0$ , (85.2)  $V=1$  and (85.3)  $V=w$ .

However, since (85.2) is a case of (85.3) a separate study of it was regarded as unnecessary [LT, 82; 85-86; 86]. The first case -[(77.1)]- tackled by means of theorem (77.20) is reduced to a series of denials,  $t=0$ , for every constituent  $t$  whose coefficient  $a$  does not vanish. Hence "The interpretation of these results collectively will constitute the interpretation of the given equation" [(85.1)]. Attention has to be called here to the fact that in the form (85.1) the function  $V$  is assumed to involve such logical symbols so as no fractional coefficients may arise. "Fractional combinations indeed only arise", wrote Boole, "in the class of problems which will be considered when we come to speak of the third of the forms of solution above referred to [(85.3)]" [LT, 82]. This clarification was only implicitly assumed in MAL.

Up to this point the exposition is almost identical with that in MAL [7.7]. The solution of the general problem (84.12) amounts now to the solution and interpretation of equation (85.3) [LT, 87]. What follows is new. Developing any given logical equation with respect to  $w$  we obtain an equation of the form

(85.4)  $Ew + E'(1-w) = 0$ ,  
 $E, E'$  linear functions of  $x, y, z$ . Solving (85.4) algebraically (assuming implicitly throughout the principle (84.11)) Boole arrived at

(85.5)  $w = E' / (E' - E)$ ,  
hence at the form (85.3) [LT, 87].

In order to tackle (85.5) Boole assumed the second member to be developed with respect to the variables  $x, y, z$  according to the generalization of (84.13) [see (77.13)]. He also introduced the known theorem (77.29) due to which we can equate to 0 every term whose coefficient does not satisfy the index law,  $w$  being the sum of the remaining terms [LT, 90-91; MAL, 66; 7.7]. So, since according to this last proposition all numerical coefficients of the form  $n/m$ ,  $n \neq 0, 1$  and  $m \neq 0$ , vanish leaving behind the interpretable equations  $t=0$ ,  $t$  of the form  $x(1-y)(1-z)$  etc, only the coefficients  $0, 1, 0/0$  and  $1/0$  can appear. Thus (85.5) is reduced to

(85.6)  $w = 1A + 0B + \frac{0}{0}C + \frac{1}{0}D$ ,



A, B, ... the remaining constituent terms, and finally, by means of the interpretation for  $0/0$  and  $1/0$  given below, (85.6) is reduced to the equations

(85.7)  $w=A+vC$  and (85.8)  $D=0$ ,

$v$  an indefinite class symbol [LT, 92].

It now remains to provide the interpretation of the coefficients in (85.6). In the case of 1, which stands for the universe, we take without limitation the whole of class A. In the case of 0, or "Nothing", no part of the class B to which it is prefixed must be taken. Also, as for the logical symbols 0, 1 he had sought their interpretation by analogy from the respective arithmetical ones [see (84.7), (84.8)], similarly for  $0/0$  Boole observed that as in arithmetic it represents an "indefinite number", in logic it must stand for an "indefinite class" [LT, 88-89].

Moreover, he regarded  $0/0$  as indicating that "all, some or none" of the class to whose expression it is affixed must be taken and for convenience replaced it by  $v$  subject to the index law [LT, 90]. Thus (85.7) would be read as follows: The class  $w$  contains the whole of class A and an indefinite amount, all, some, or none, of the class C. It is thus implied that the solution of (85.5) is not unique. Coefficients of rational form appear only in case (85.3), as mentioned above, which is treated by means of (77.29). It remains to examine then whether  $0/0$  obeys or not the index law. Boole's answer is not a straight forward one: The symbol  $0/0$ , he wrote, "does not necessarily disobey the law we are here considering [ $x^2=x$ ], for it admits of the numerical values 0 and 1 indifferently. Its actual interpretation, however, as an indefinite class symbol, cannot, I conceive, except upon the ground of analogy, be deduced from its arithmetical properties, but must be established experimentally" [LT, 91-92].

Thus Boole is no more clear in LT about  $0/0$  than he was in MAL. He certainly devotes to it more ample discussion than before but shows his overall uncertainty by claiming that  $0/0$  "must be established experimentally" without clarifying at all what is meant by that. Moreover he falls into certain inconsistencies. First he replaces  $0/0$  by  $v$  which in MAL and in basic

logic in LT was regarded as a non-empty class, meaning "some", possibly "all", whereas in the course of the general method it is totally indefinite, and so could also mean "none". Secondly, he regards  $v$  as obeying the index law but sounds uncertain as to whether  $0/0$  obeys it or not. Thirdly, it is obvious that formal logic, as based upon these three possibilities, can not possibly be strictly analogous to the algebra of 0 and 1.

About the coefficient  $1/0$  he is less uncertain. Algebraically it is the symbol of infinity. "Now the nearer any number approaches to infinity (allowing such an expression), the more does it depart from the condition of satisfying" the index law [LT, 91]. Thus any symbol, apart from 1, 0 and  $0/0$ , indicates that the constituent to which it is prefixed must be equated to 0 [LT, 92]. Hence we have (85.8). In other words, class D is not merely excluded from  $w$  but it is an impossible, an empty class. Thus the classes to which  $0/0$  and  $1/0$  are attached to have been interpreted (see further comments in (4) below). But what about the classes  $0/0$  and  $1/0$  themselves? Do they signify logical operations and if so which? Boole provides no clues in LT. These two symbols are in fact peculiar artefacts of Boole's formal method. Division, as a logical operation, is strictly excluded from basic logic. Moreover, often  $0/0$  is not replaced by  $v$ , most probably because for him it meant something more complicated than the symbol  $v$  as introduced in the first part of this work.

Omitting the particular example through which division was for the first time introduced in LT, we proceed to discuss Boole's general procedure in it. Let us take the equation (85.9).

$x = yz$ ,  
 $z$  to be determined. Before proceeding any further we notice from (85.9) that, division  $x/y$  to be effected,  $y$  has to be part of the class  $x$ , in analogy with the other inverse operation, subtraction (see discussion following (84.3)). Developing  $x/y$  we have

$$(85.10) \quad z = \frac{x}{y} = \frac{1}{1} xy + \frac{0}{0} x(1-y) + \frac{0}{1} (1-x)y + \frac{0}{0} (1-x)(1-y).$$

[LT, 87]. According to the solution of the general problem (85.5) the above is reduced to the equations

$$(85.11) \quad x/y = xy + v(1-x)(1-y) \quad (85.12) \quad x(1-y) = 0.$$

The interpretation of  $x/y$  is therefore a class which consists of all  $xy$ , none of  $(1-x)y$ ; and an indefinite amount of  $(1-x)(1-y)$  together with the condition that the class  $x(1-y)$  is empty [see Venn 1881, 205, 208, 211-12].

How Boole viewed division in logic is an essential question which cannot seek a totally satisfactory answer. But it is clear that apart from his notes  $N_7-N_{27}$  [(83.3)], he did not consider division as the inverse operation of combination in logic, he did not seek its most general interpretation as a class but only accepted it as a necessary outcome of his formal method in the solution of certain problems. To be able to further justify this opinion we have to seek explanations in his more general statements about inverse operations. The best source for this is his textbook on differential equations (1859). In the 16th chapter on symbolical methods Boole talks about inverse "procedure" instead of "operation". In a way he denies that the inverse of an operation stands indeed for an operation. "The inverse procedure is thus presented", he wrote, "as one, the effect of which the direct operation simply annuls. This is its definition" [1877, 386; see also 8.8].

But even this last point is absent in LT. That is, Boole did not consider the converse argument seeking to prove the correctness of his result for  $x/y$  as in formula (85.10). He only aimed <sup>at</sup> obtaining its logical interpretation according to the particular problem under question. Thus, even if he is unclear about the definition of the inverse operation of  $d/dx$  in his book on differential equations, he is even less specific -almost indifferent we could say- about the analogous definition of the inverse of the operation  $xy$  in logic.

Venn noticed that (85.11) can also be written in the form  $x/y = x + v\bar{x}y$  [ $y \bar{x}$  he denoted  $1-x$ ]. "The test of the correctness of this result", he wrote, "is found, as in the case of other inverse operations, by simply performing the direct process of it, and seeing whether we are thus led back to our original starting point. Thus if we multiply this expression,  $x + v\bar{x}y$  by  $y$ , it must yield  $x$ . It is true that at first sight we seem to get a different result, viz,  $xy$  instead of  $x$ ; but the difference is soon found to be apparent only, in as much as  $xy$  and  $x$  are in this

case the same" [Venn 1881, 72]. Moreover, (85.11) can be reduced by means of (85.12) to the form

$$(85.13) \quad x/y = x + v(1-y)$$

This form is found in [Venn 1881, 204 fn; Hooley 1966, 115; Styazhkin 1969, 194]. The latter based his elucidation of Boole's process of division upon that form; see (3) below].

Quoting Boole [1859, 377] "it is the office of the inverse symbol to propose a question, not to describe an operation. It is, in its primary meaning, interrogative, not directive", Venn wrote in a footnote: "It surprises me that one who had so clearly stated the nature of an inverse operation in mathematics should never have proposed, so far as I know, any corresponding explanation in Logic" [1881, 70]. Comments are also to be found in [Ven Evre 1977, 365, 377 fn 10; Hailperin 1981, 184]. Venn was among the first logicians who attempted to shed light on Boole's most obscure point in formal logic, division [Styazhkin 1969, 197]. He tried to define  $0/0$  and  $1/0$  as logical operations and the study of his [1881] is still one of the best sources for reference in order to view Boole's work in the spirit of his time (on a direct, unproblematic, reception of Boole's process of division see Young 1865).

However, what Venn provided in his [1881], as explanation for Boole's peculiar symbols  $0/0$  and  $1/0$ , partly coincides with Boole's own account in an unpublished essay, entitled by Rhees as "Logic and reasoning" <sup>and</sup> hereafter cited as LR, written after the publication of LT<sup>(2)</sup>. In this essay, referred to by Boole as a "letter", he tries to present a non-algebraic expression of his formal method. Illustrating the logical operations of composition and addition with examples, such as "red flower" and "men and women" respectively, Boole proceeds to mention that "To these operations there exist two others which are respectively inverse" [LR, 221]. This is the second instance, besides his notes  $N_1 - N_2$  [8.3], where Boole admits the existence of the logical operation of division. He wrote in [LR, 221]:

viz, as inverse to aggregation, that process by which, from the conception of a whole by the subtraction of one of its parts we form the conception of the remainder, and as inverse to composition, that by which from a given conception, we ascend to some

higher conception, from which by a given act of composition, the conception given may be formed. The operation commonly called abstraction is in its formal character a particular case of this. When from the conception "red flowers" we ascend to that of "flowers", we arrive at a conception which, by composition with "red", gives "red flowers."

This quotation, similar in spirit to Boole's views on the inverse of multiplication in his  $N_1 \cdot N_2 =$ , shows that Boole -contrary to what he had invoked in LT- was aware of the justification of his result as in (85.11). It is strange that he kept such crucial details for his personal notes and correspondence, while he had been so much more clear in his account of  $(d/dx)^{-1}$  in his [1859]. Probably in LT, taking for granted that his readers would have been able to see beyond his cryptic statements, he was eager to illustrate as fully as possible the results of his formal method to a variety of problems of logic and probability; whereas, in his notes, partly gathered as a sketch for a book to be published on the philosophy of logic addressed to non-mathematicians [8.7], he provided more detailed and careful explanation.

Next Boole tried to define in simple terms the classes 0/0 and 1/0. We read in [LR,221]:

Now the processes of the Laws of thought lead in certain cases to a symbolic form which is inverse in character and may be directly interpreted into the question: What class is that which, in composition with the conception of Nothing, leads to the conception of Nothing? In other words, what class is that from which, if we mentally separate nothing, we obtain as the separated product, nothing? It is plain that the answer to this is any class. If from any class of things we take nothing, we obtain nothing. Now the conception of any class is, in other words, that of an arbitrary or indefinite class, and this is the ground of the category of the indefinite [0/0].

In similar terms the class 1/0 which denotes the category of the impossible is defined as the class "from which, if we mentally separate Nothing, we obtain as the separated mental

product, the conception of the Universe" [LR, 222]. These definitions coincide <sup>10</sup>large with Venn's respective ones in his [1881, chapter 9, 205]. Most probably Venn was unaware of Boole's manuscripts of that time and thus of the quotations given above from LR.

Nowdays Boole's method of division is rigorously remodelled, the most complete account provided by [Hailperin 1976, chap.2, 1981]<sup>(3)</sup>. However, as both Hailperin and Hooley declared, despite the attempt to present Boole's method in a lucid way, it was impossible to explain how he had managed to provide sound results while using nonrigorous processes. We read: "Of course, our explanations are markedly different from Boole's. In view of the many, varied, and involved techniques we have had to use to accomplish what Boole did we can only stand in wonder at the genius in Boole's ingenuity" [Hailperin 1976, 108; see also Hooley 1966, 118]<sup>(4)</sup>.

## 8.6 Elimination, reduction and Aristotelian logic as treated in LT.

It is for the first time in LT that Boole observed a substantial difference between elimination in logic and elimination in algebra. In the former, due to the index law, we can eliminate from a single equation an indefinite number of symbols. [LT, 99-100]. The method he introduced is as follows: Let  $f(x)=0$ ,  $x$  to be eliminated. We expand  $f(x)$  with reference to  $x$ , solve the equation  $f(1)x+f(0)(1-x)=0$  for  $x$  and  $1-x$  respectively, and then substitute the values of  $x$  and  $1-x$  given by  $x=f(0)/(f(0)-f(1))$  and  $1-x=-f(1)/(f(0)-f(1))$  in the fundamental equation  $x(1-x)=0$ . Hence we have

$$(86.1) \quad f(1).f(0)=0.$$

[LT, 101]<sup>(5)</sup>

Boole provided two more proofs for the elimination formula (86.1). The one is on lines similar to algebraic elimination [LT, 102]. The other is of a "half logical character". Boole reduces  $f(1)x+f(0)(1-x)=0$  to the equations

$$(86.2) \quad f(1)x=0 \text{ and } f(0)(1-x)=0.$$

By solution and development we obtain.

$$(86.3) \quad f(1) = \frac{0}{x} = \frac{0}{0} (1-x) \quad \text{and} \quad f(0) = \frac{0}{1} - x = \frac{0}{0} x.$$

Through this demonstration, noticed Boole, we afford to interpret the latter equations in logical terms: "1st whatever individuals are included in the class represented by  $f(1)$ , are not  $x$ 's" and "2nd whatever individuals are included in [...]  $f(0)$ , are  $x$ 's". (LT, 102-103). Whence, there are no individuals in the class  $f(1)f(0)$ , hence we have the formule (86.1). Followed a generalization of (86.1) for two variables in the form:  $f(1,1)f(1,0)f(0,1)f(0,0)=0$  (LT, 103-112).

As an example take "All men are mortal" given by  $y=vx$ . Giving to  $v$  successively the values 0 and 1 and multiplying the equations thus derived we obtain  $y(1-x)=0$  read as "Men who are not mortal do not exist" (LT, 104). First we see that, contrary to what had been assumed in <sup>the</sup> basic logic of either MAL or LT,  $v$  can obtain the interpretation "none". Secondly, we see that Boole correlated the two forms for  $A$  in exactly the reverse order from that given in MAL (where the former was deduced from the latter as its solution) [see 7.5, 8.3].

In the end of chapter 7 on elimination Boole affirmed that the equation (86.1) admits of a remarkable interpretation. Let  $f(x)=0$  imply a proposition regarding "men". Equation  $f(1)=0$  expresses the original proposition under the assumption that men made up the universe, while  $f(0)=0$  expresses what the original equation would become if men did not exist. Based upon this example Boole claimed in (LT, 113) that (86.1) expresses that: "What is equally true, whether a given class of objects embraces the whole universe or disappears from existence, is independent of that class altogether, and vice versa" He then added that, as in the case of theorem (84.10), "we see another example of the interpretation of formal results, immediately deduced from the mathematical laws of thought, into general axioms of philosophy".

In the next chapter Boole proposed two methods for the reduction of a system of logical equations to one, so that his general method for the interpretation of a single equation can be applied. First he presented the method of indeterminate multipliers which he had discovered in MAL. The demonstration now offered for its justification is more simple and elegant than that

given in 1847<sup>(2)</sup>. The second method of reduction introduced was regarded as superior to the former which had "the inconvenience of rendering the subsequent process of elimination and developments when they occur, somewhat tedious" [LT, 115]. The latter method was based on the following observations. Boole proved that when the coefficients in the developments of  $V_1, V_2, \dots$  are positive then the system

$$(86.4) \quad V_1=0, V_2=0, \dots$$

has the same logical import as

$$(86.5) \quad V_2+V_2+\dots=0.$$

In the case of negative coefficients the system (86.4) is equivalent to equation

$$(86.6) \quad V_1^2+V_2^2+\dots=0$$

[LT, 120-122]<sup>(3)</sup>.

As an example of his application of theorem (86.6) take the proposition

$$(86.7) \quad X=Y,$$

where  $X$  and  $Y$  each satisfy the index law. The above is written as  $X-Y=0$ , whence the form  $V_1=0$ . By squaring we have

$$(86.8) \quad X(1-Y)+Y(1-X)=0.$$

The left-hand side is interpretable since both terms satisfy the index law and are exclusive [LT, 123]<sup>(4)</sup>.

Once more we notice Boole's preference for modes of demonstration closely related to the algebraic principle (84.11) rather than processes peculiar to analysis, as with the case of indeterminate multipliers. This remark holds true also for the proof of the development theorem (84.13) and the solution of elective equations.

In the short 10th chapter<sup>en</sup> titled "Of the conditions of a perfect method", Boole undertook to extend certain ideas he had only briefly hinted at in MAL [7.7]. Proud of his formal method he cared in rendering it "perfect". By that he did not mean "perfection only which consists in power, but of that also which is founded in the conception of what is fit and beautiful" [LT, 150]. Indeed, as he stated in the end of that chapter, no particular practical advantage was derived out of his perfect method. What was essential for him was to render the forms of the method "suggestive of the fundamental principles, and if possible



of the one fundamental principle, upon which they are founded" [LT, 150]. Now, this one fundamental principle can be no other than the index law in the form

$$(86.9) \quad x(1-x)=0.$$

He then went on to say in [LT,151]:

Were brevity or convenience the only valuable quality of a method, no advantage would flow from the adoption of such principle. For to impose upon every step of a solution the character above described [(86.9)], would involve in some instances no slight labour of preliminary reduction. But it is still interesting to know that this can be done, and it is even of some importance to be acquainted with the conditions under which such a form of solution would spontaneously present itself.

It is of importance to mention these views for they are <sup>representative</sup> not only of Boole but also of many mathematicians of that period. Brevity was one of the advantages of the calculus of operations, but symmetry, elegance and unity were not less important. The poet and the philosopher is intertwined <sup>with</sup> the logician and the mathematician in Boole's persona [see also 7.1, (2)].

Boole recalled the basic forms of equations

$$X=vY$$

$$(86.10) \quad X=Y$$

$$vX=vY$$

and their transformations in the form  $V=0$ , the latter satisfying the law (86.9) [see (86.8)]. This possibility could be generalised as follows. Let  $V=v+v'+v''+\dots$ , where  $v, v', v'', \dots$  are functions of  $x, y, z, \dots$  which obey (86.9). Then  $V$  can also be written in the form

$$(86.11) \quad V=v+(1-v)v'+(1-v)(1-v')v''+\dots$$

thus satisfying the index law [LT, 152-3] <sup>(5)</sup>. Thus "All logical equations then are reducible to the form  $V=0$ ,  $V$  satisfying the law of duality". But would it not had been a higher degree of perfection if equations always presented themselves in such a form? [LT, 153]. The reason why this is not always so "is a consequence of the fact that our premises are not always complete, and accurate, and independent". Setting aside the first two points, he went on to define independence. "A system of

propositions may be termed independent, when it is not possible to deduce from any portion of the system a conclusion deducible from any other portion of it" [LT,153-4].

Next he proved that if  $V$  in the equation  $V=0$  satisfies the law (86.9) and if the expression of any symbol  $t$  of that equation be determined as a developed function of the other symbols, then the coefficients of the expansion can assume only the forms 1,0,0/0,1/0 [LT,155]. Boole argued that the first three forms appear independently of the condition (86.9) for the function  $V$ . However, the terms to which 1/0 is attached may receive any other value, except 1,0,0/0, when the condition (86.9) is not satisfied. To render the method uniform he decided to "change any coefficient of a development not presenting itself in any of the four forms referred to [...] into 1/0, regarding this as the symbol proper to indicate that the coefficient to which it is attached should be equated to 0. This course I shall frequently adopt" [LT, 156].

Finally Boole proved that if  $V$  satisfies the index law, the result of the elimination of any symbols from  $V=0$  can be obtained by developing the given equation with reference to the other symbols, and equating to 0 the sum of those constituents whose coefficients equal 1 [LT,156-157]. Thus, the conditions of a perfect method consist of the following:

1. That the propositions are of the ordinary kind, implied by the use of the copula is or are, the predicate being particular,
2. That the terms of the proposition are intelligible without the supposition of any understood relation among the elements which enter into the expression of those terms, and
3. That the propositions are independent.

He ended the chapter with the remark: "Considered both in their relation to the idea of a perfect language, and in their relation to the processes of an exact method, these conditions are equally worthy of the attention of the student" [LT,158].

Omitting Boole's new theory for hypotheticals - chapters 11-15- we will proceed to illustrate his critical approach towards the validity of philosophical arguments by Clarke and Spinoza as in chapter 13. We will quote his procedure to prove "That unchangeable and independent Being must be self-existent". The

premises are:

1. Every being must either have come into existence out of nothing, or it must have been produced by some external cause, or it must be self-existent.
2. No being has come into existence out of nothing.
3. The unchangeable and independent Being has not been produced by an external cause.

Let  $x$ =Beings which have arisen out of nothing,  $y$ =Beings which have been produced by an external cause,  $z$ =Beings which are self-existent and  $w$ =The unchangeable and independent Being. We thus have

$$(1) \quad x(1-y)(1-z)+y(1-x)(1-z)+\overset{z}{(1-x)(1-y)}=1 \quad (2) \quad x=0 \quad (3) \quad w=v(1-y).$$

Eliminating  $v$  from (3) we have (4)  $wy=0$ .

"Whenever as above the value of a symbol is given as 0 or 1, it is best eliminated by simple substitution". Thus, the elimination of  $x$  from (1) gives

$$(5) \quad y(1-z)+z(1-y)=1 \quad \text{or} \quad (6) \quad yz+(1-y)(1-z)=0.$$

Adding (4) and (6), and eliminating  $y$ , we get  $w(1-z)=0$ , hence  $w=vz$ . This last equation is interpreted as "The unchangeable and independent being is necessarily self-existing" [LT,194-5] 'e'.

We will conclude this section with few comments upon Boole's treatment of Aristotelian logic as in his chapter 15 of LT. First he mentioned the well-known four forms of categorical propositions, A, E, O and I, and then referred to De Morgan and Hamilton in connexion with the enrichment of this system with another four forms [LT,227]. He expressed the eight fundamental types of propositions in his symbolic language as follows:

All Y's are X's	$y=vx$
No Y's are X's	$y=v(1-x)$
Some Y's are X's	$vy=vx$
(86.12) Some Y's are not-X's	$vy=v(1-x)$
All not-Y's are X's	$1-y=vx$
No not-Y's are X's	$1-y=v(1-x)$
Some not-Y's are X's	$v(1-y)=vx$
Some not-Y's are not-X's	$v(1-y)=v(1-x)$

[LT,228] 'f'.

Conversion was effected by the general method as follows. Take "All Y's are X's" denoted as

$$(86.13) \quad y=vx.$$

Eliminating  $v$  we have

$$(86.14) \quad y(1-x)=0.$$

The latter, solved with respect to  $1-x$ , gives

$$(86.15) \quad 1-x = \frac{0}{0} (1-y)$$

interpreted as "All not-X's are not Y's". This is an example of "negative conversion" [LT,229].

Through elimination the existential import of  $v$  in (86.16) is dropped in (86.14) and  $0/0$  is no more replaced by  $v$  in (86.15). Hence, we notice once more Boole's inconsistency in his use of  $v$ . In MAL this inconsistency was restricted due to the fact that he had regarded as a system of the fundamental forms of propositions (74.8) instead of (86.12). In another case, as below, the existential import is apparent throughout. Let equation (86.13). Multiplication with  $v$  gives

$$(86.16) \quad vy=vx$$

read as "Some Y's are X's" [LT,229; Häilperin 1976, 78-9].

Listing the conclusions of negative conversion, Boole formed the following group of equivalent forms

$$(1) y(1-x)=0 \quad (2) (86.13) \quad (3) 1-y = \frac{0}{0} x + (1-x) \quad (4) x=y + \frac{0}{0} (1-y)$$

for the negative conversions of (86.13). For example (3) was read as "Things that are not-Y's include all things that are not-X's, and an indefinite remainder of things that are X's" [LT,230]. Whereas in MAL negative conversion was simply effected by changing  $X$  into not- $X$  and vice-versa. For example,  $xy=0$ . "No X's are Y's, was written in the form  $x(1-(1-y))=0$  interpretable into "All Xs are not-Ys" [MAL,28-29]. Thus, his list-according to the rules provided -was more straight-forward:

"All Xs are Ys" was equivalent to either of the following:

"No Xs are not-Ys"

$$(86.17) \quad \text{No not-Ys are Xs}$$

"All not-Ys are not-Xs."

[MAL,30].

Syllogistic deductions were made more cumbersome than in MAL on simpler lines. The advantage of Boole's formal method lay in the possibility of now generalizing his former cases into two

$$(86.18) \quad \begin{array}{cc} vx=v'y & vx=v'y \\ \text{and} & \\ wz=w'y & wz=w'(1-y) \end{array}$$

depending on whether the middle-terms were of "like" or "unlike quality". Assuming proper interpretations to the symbols  $v, v'$  etc all possible varieties with reference to quantity can be represented.

His procedure has as follows: Suppose we have the system

$$(86.19) \quad \begin{array}{cc} (1) vx=v'y & (3) vx(1-v')=0 \\ \text{or} & \\ (2) wz=w'y & (4) wz(1-w')=0 \end{array}$$

Eliminating  $y$  from (1) and (2) we have

$$(5) w'vx=v'wz$$

Now, it remains to express  $x$ ,  $1-x$  and  $vx$  in terms of  $z$  and  $v, v', w, w'$  so as to include all the possible forms of the subject of the conclusion.

Boole ~~does not~~ mention in LT any of (3), (4) and (5) and gives directly the values of  $x$ ,  $1-x$  and  $vx$ . Details are provided in his Annotated copy of MAL edited by [Smith 1982b] <sup>(9)</sup>. In order to find  $x$  Boole writes  $w'v=t$ ,  $v'wz=t'$  so that (5) becomes

$$(6) tx=t', t''=t, t't''=t'$$

Hence, by development we have  $x=tt'+q(1-t)(1-t')$ ,  $t'(1-t)=0$

Replacing  $t, t'$  with their equivalent and taking under consideration formulae (3) and (4) he obtains the value for  $x$  [Smith 1982b, 4-8].

In the Appendix of this copy <sup>(9)</sup> a second method was suggested worth of mentioning. Solving (2) for  $y$  we have

$$(7) y=ww'z+q(1-w')(1-wz) \text{ with (4) holding true.}$$

Hence, from (1) we have

$$(8) vx=v'(w'wz+q(1-w')(1-wz)).$$

Boole solves (8) in respect with  $x$ , expands the coefficient of  $z$  with reference to  $v$  and  $w$  and taking under consideration (4) he finally obtains the same result as before [Smith 1982b, 33-34]. The procedures are long and complicated, and on similar lines  $1-x$  and  $vx$  are determined separately. Omitting the details, we will solely give the result for  $x$  as given in LT which coincides with that provided in that copy with the only exception that now  $q$  is replaced by  $0/0$

$$x = [vv'ww' + \frac{0}{0} [vv'(1-w)(1-w') + ww'(1-v)(1-v') + (1-v)(1-w)]]z + \frac{0}{0} [vv'(1-w') + 1-v](1-z)$$

[LT, 233].

Boole next provides the conditions and rules of inference for both cases in (86.18) and having done so he apologises for the needless complexity of his process of investigation, which is apparent only in the formulae that express  $x$ ,  $1-x$  and  $vx$  in both cases. He agrees that the same results, i.e. the rules of syllogistic inference, could have been obtained with greater facility but it was his object, he claimed, "to conduct the investigation in the most general manner, and by an analysis throughout exhaustive. With this end in view, the brevity or prolixity of the method employed is a matter of indifference" [LT, 237].

Having argued so, he illustrates the generality of his method which can provide deductions that are beyond the scope of traditional syllogistic by replacing  $v$ ,  $x$  etc in the system (86.19) with qualities that express colour of pieces. The result is a 9-line sentence that reminds us of De Morgan's games with words in his obscure paper on relations [1860a]. Boole was proud of his method but his treatment of Aristotelian logic was far less successful than in his earlier account in MAL, which was less elaborate but more straight-forward.

Summing up what he had presented so far in LT in respect with his general method in logic, Boole offered as a conclusion what he had hinted at in his introductory chapter. Scholastic logic is not a science, he wrote, "but a collection of scientific truths, too incomplete to form a system of themselves, and not sufficiently fundamental to serve as the foundation upon which a perfect system may rest". He is sceptical about the real utility of traditional logic's mnemonic rules and ends up with "As concerns the particular results deduced in this chapter, it is to be observed, that they are solely designed to aid the inquiry concerning the nature of the ordinary or scholastic logic, and its relation to a more perfect theory of deductive reasoning" [LT, 241-2].

## 8.7 Boole's speculations on symbolical procedures in logic in late 1850's.

Up to the early 1850's Boole was mostly interested in vindicating symbolical methods through applications in problems of analysis -such as in the solution of the Laplace equation [5.8]- and in logic [MAL and LT]. His logical writings were mainly addressed to mathematicians and logicians with a considerable knowledge of symbolical algebra and analysis. Gradually the scene changes and he becomes more interested in the foundations and philosophy of symbolical methods rather than in their applications.

Boole had meant to present his material in LT within a philosophical frame-work missing in MAL. There were indeed certain instances, such as in connection with his "theorem" (84.10), where -somehow unsuccessfully [see 8.4, (8)]- he had hinted at the metaphysical interpretation of symbolic formulæ. In the introductory chapter he stressed that the principles of symbolical methods are not mathematical but founded in the very nature of language [LT.6]. In that chapter Boole included in summary his earlier investigations in connection with symbolical methods in logic and mathematics [see sections 8.2, 8.3, 8.4; LT, 5-13].

In the concluding chapter of LT Boole contemplated the concepts of induction, infinity<sup>and</sup> unity as well as the role of philosophy in the domain of science in general. He stressed the role of intuition in processes of discovery and claimed that the isolated study of different domains of science inhibits man's possibility to come close to the "Truth" which, indirectly and in an obscure way, he related to God. Boole's epistemological views are worthy of attention and will be briefly discussed in 8.9. However, they shed no light whatsoever in his symbolical treatment of logic as exposed in the main part of his book [8.4-8.6].

Indeed, soon after composing LT, Boole realized that his philosophy of logic was hidden beneath excessive symbolism and thus decided to write a sequel to it where the theory of logic would be outstripped of its mathematical veil. We are left with a series of manuscripts written during the period 1855-1860 in which he dealt with the nature of logic, the doctrines of concep-

tion, judgment and reasoning as well as with the use of symbolism in logic [Hesse 1952, 63-64]... These manuscripts are drafts for the book on philosophy of logic which he had intended as a sequel to LT <sup>but</sup> which was never ~~finished~~ [7.1 text and (24)]. The book was not completed for a variety of reasons: illness, indecision as to what to include from his various drafts and premature death. Further study of these manuscripts suggest another plausible reason, that is, difficulty to express within ordinary language what he was accustomed to state with ease by means of symbols<sup>(1)</sup>. As a result, despite his claims of outstripping logic of its symbolical veil, Boole would refer to certain symbolical formulae and would devote a considerable part of these manuscripts on a discussion of the apparent independence of symbolical procedures from mathematical notions.

Considerable study has been carried out in our century on Boole's manuscripts <sup>(2)</sup>. In what follows we will stress his views on the nature and value of symbolical procedures in general, aiming to illustrate in 8.8 his unpublished views through his published respective account in his treatise on differential equations written in 1859. [not by Boole]

We start with the typescript introduction W<sub>3</sub> [see 7.1, (24)]. There he admitted that algebraic procedures had undoubtedly been a source of difficulty for readers of LT. However, he claimed, the employment of these procedures was not arbitrary but rested upon the analogies proved to exist between the operations of thought in logic and the algebra of 0 and 1. This analogy, he went on, does not consist in likeness of the operations or of the subjects, but, "as every strict and proper analogy does - in likeness of relations. Agreement in formal laws is the only ground upon which any connection of the method between Logic and the science of number (algebra of 0 and 1) is possible" [W<sub>3</sub>, 2].

An important question arises next: how far this analogy is indispensable in the full development of logic? Boole says that this analogy proved essential to him mainly as a means to discover methods. In fact, real isolation and independence in the study of the departments of the domain of truth is hardly possible. However, under the belief that this analogy was not essential, he would present the results to which he was led by it.



using purely logical devices [W<sub>3</sub>, 2-3].

The import of these introductory statements is also evident in some other manuscripts written around 1855-56, cited earlier as LR [see 8.5, (2)]. In connection with the symbolical procedures in LT Boole poses the following question: "What is the logical import of the processes there employed? Analogies, mathematical, and otherwise, being cast aside, what doctrine of the intellectual operations remains concealed beneath the forms themselves?" [LR, 212]. With this question Boole tackles directly the philosophical doctrine of symbolical procedures in general. Before we present his conclusion on this matter in LR, let us first give a sketch of his general speculations in these manuscripts.

First he distinguished between the highest conception of logic, "as "conversant about all Thought which admits of expression; whether that expression be effected by the signs of common Language or by the symbolic language of the mathematician", and the secondary one which consists of the logic of class [LR, 212-213]. Then, on lines similar to those followed in N<sub>7</sub>-N<sub>27</sub>, he perceived a close connection between the conception of number with that of class [LR, 214]. He went on to discuss the three fundamental operations of conception, judgment and reasoning and their laws. He claimed that the laws of identity, contradiction and of excluded middle cannot be regarded as alone fundamental but form only part of a system of logic [LR, 214-216; Rhees 1952, 25].

Providing an example of traditional syllogism, Boole made the following statement in [LR, 217-218]:

I think I should myself have been at first disposed to conjecture that the validity of inference depends upon the formal laws of operations alone. I would add that it is not upon considerations such as these the proposition as involved in the symbolism of my treatise rests. It is upon the fact which is established: that the formal laws of all conceptions of class are those which are common to the two limiting conceptions of Universe and Nothing, i.e. to the two conceptions which express simply the ideas of existence and non-existence.

Boole observed that a logical conclusion is always in the form of a necessary proposition modified by means of the premises. There are four categories of modification studied in 8.5: the universal, the indefinite, the non-existent and the impossible [LR, 220-222]. He went on to compare these four categories with those distinguished by Kant, the universal, the particular and the singular and speculated on whether his own scheme "can be established independently of the interpretation of symbolic forms" [LR, 224-226].

Finally, we are presented with his overall conclusions which partly answer the initial question mentioned above [LR, 226-7]. We present his main points in summary listing them, for reference purposes as:

- (87.1) The basis of all logic is the possibility of apprehending the laws of general conceptions. These conceptions may be only the unattainable limits of processes of abstraction. Unable to picture them we can only study and contemplate their laws "And these laws contemplated and applied constitute the immediate subject matter of all Logic".
- (87.2) These conceptions are expressed by signs whose laws express the laws of the conceptions. Hence they are arbitrary in character but fixed in interpretation and law.
- (87.3) Intellectual procedure consists in elementary operations of thought by which conceptions are combined and compared and by which propositions are expressed and inferred.
- (87.4) Every intellectual operation implies a subject upon which it is performed, and a result to which it leads. Given the former we can determine the result by a direct procedure based upon formal laws only. When we seek to determine the subject, the inverse procedure involved depends upon our knowledge of the general nature of the subject conception and upon the specification of this knowledge through a knowledge of the general results of the direct procedure.
- (87.5) "Operations may be subject to conditions of possibility

as well as to formal laws, when possible. But the same formal laws may belong to different sets of operations and in the one set, the conditions of possibility may be wider than in the other. In this case, we may conduct our reasoning about the sphere of the one by transferring them to the sphere of the other".

- (87.6) "The question whether the intellectual procedure of thought is entirely dependent upon formal laws, -so that we may not only neglect the conditions of possibility of the operations in the actual sphere of the reasoning, but also the consideration whether there exist any other sphere in which operations subject to the same laws exist, but not limited by the same conditions of possibility, -still remains".

The most important of the points raised above is (87.5), where Boole for the first time speculates on formal conditions of possibility. In another paper, cited here as "Extracts", he observes "that the meaning of words is not always wholly independent of the form of expression in which they occur". For example, we cannot express "X's and Y's" unless X and Y are distinct classes. Hence, "it is that the forms of language, which is but the outward expression of thought, impose conditions of interpretability upon the symbols which they connect" ["Extracts", 232-3] <sup>(3)</sup>. So, while up to LT Boole was mainly interested in the interpretability of results, now he also pays attention to the conditions of possibility of formal expressions. Based upon the index law he infers that "the equation  $xy=0$  is the condition of interpretability of the operation of adding  $y$  to  $x$  in either system [Logic or dual algebra], and it is a condition formally expressed" [Rhees 1952, 21] <sup>(4)</sup>.

On similar lines  $y(1-x)=0$  is the condition of possibility for expression  $x-y$  and  $x(1-y)=0$  respectively the condition for  $x/y$ . Prior to this, he noticed that no such condition is involved in composition. It is of interest to notice here, that the condition for division (which he called abstraction) was not derived by means of the development formula as in LT [(85.10)-(85.12)]. Representing  $x/y$  by  $w$  he inferred  $x=yw$  and multiplied both sides with  $1-y$ . Due to the index law,  $x(1-y)=0$  was obtained [Rhees

1952, 22].

In some other manuscripts, cited in [Hesse 1952, 70], Boole distinguished between operations of extension, + and -, and operations of intension, . and /. His concept of class is prominent throughout all his manuscripts. Once more, regarding division as an operation, he called abstraction the inverse of composition, adding that "This is the only operation which introduces class members not contained in the initial class concepts". It has to be noticed that, neither in MAL, nor in LT did Boole accept division as an operation. Moreover, this is the only instance where he points out the distinction between operations of extension and intension which is important for their proper interpretation<sup>(5)</sup>.

We end with few more comments on division in ["Extracts", 242-3]. Boole distinguished between the symbolical equations

$$(87.7) \quad x = yz \quad (1)$$

$$(87.8) \quad y = x/z \quad (2)$$

and

$$(87.9) \quad y = 1xz + 0z(1-x) + \frac{1}{0} (1-z)x + \frac{0}{0} (1-z)(1-x) \quad (3)$$

commenting as follows:

In (1), which is the symbolical expression of the premiss, the concept  $y$  whose explicit definition is sought appears in composition with  $z$ . From this connexion it is freed by the inverse operation of abstraction. The equation (2) expresses this fact, and shews that it is by abstraction of the concept  $z$  from the concept  $x$  that the definition of  $y$  must be obtained. Equation (3) exhibits the result of the abstraction as obtained by the process of development, that process depending not upon the meaning of the symbols  $x$ ,  $z$ , but only upon their formal laws.

From the above citation Boole seems to regard (87.8) as the formal definition of division. However, this is not clear, (87.8) is rather the formal expression of the operation of division than its proper definition. For, the complete definition of division -according to point (87.4)- would have included, besides (87.8), the formula

$$(87.10) \quad zz^{-1}x = x,$$

which involves the earlier notation  $z^{-1}$  given in (83.3). Formula (87.10) was mentioned in the distinction of symbolical equations involving the inverse of differentiation in his [1859]. [see (88.3) below] '4'.

The above account shows evidence of the prominence of symbolical procedures in his work. To dispense with them in logic led, in fact, to a deeper speculation upon their nature '7'. To a great extent these observations clarify his rather obscure account in LT. But it was only with his textbook on differential equations in 1859 that Boole presented to the public the outcome of his philosophical and educational concerns in connection with symbolical procedures. We will present the most characteristic instances of his account on this matter in the next section.

#### 8.8 Boole's views on the nature and value of symbolical methods in mathematics in [1859].

As we mentioned in the previous section, up to the early 1850's Boole was mainly interested in illustrating the power of operator algebras through a diversity of applications within branches of analysis and logic. By mid 1850's he undertook to delve into a study of the nature of symbolical methods in general in an attempt to make out how far such methods were indispensable in the presentation of his theory of logic. His study was not completed. However, in the most elegant way he makes use of his speculations on this matter in his exposition of symbolical methods for the solution of differential equations in his [1859].

Boole's textbook Differential equations [1859] has been repeatedly cited in our thesis. However, this is chronologically the proper place to enter into a discussion of its instances which show evidence of his foundational study of symbolical procedures '1'. The best sources for such information are the "Preface" of this book and chapters 16-17 devoted solely on symbolical methods. However, despite his deep pedagogical concerns, important omissions will be noted, particularly in connection with his study of inverse operations. We start our account with some interesting passages from the "Preface".

Symbolical procedures, according to Boole, demand a con-

siderable familiarity with abstractions and a skill in using known methods which historically preceeded the employment of symbolical means. The student is above all advised to follow the historical sequence of discovery and deal with symbolical methods only when ready to delve into their nature. For, "Of the many forms of a false culture, a premature converse with abstractions is perhaps the most likely to prove fatal to the growth of a masculine vigour of intellect" [1877, vi].

Symbolical methods depend primarily upon certain inverse operations. Even the most accomplished in the use of symbols may be confronted with serious problems, particularly when it comes to the interpretation of a solution which can be defective or redundant in certain cases. In such instances, the student is advised to "throw aside his abstractions and resort to homelier methods for trial and verification—not doubting, in so doing, the truth which lies at the bottom of his symbolism, but distrusting his own powers" [1877, vii].

We may ask, like Boole in his introduction W<sub>3</sub> [8.7], "how far are symbolical methods indispensable in analysis"? Boole is not very precise on this point in the "Preface". As we shall see below, there are specific cases where indeed symbolical methods afford the only plausible way to integrate a differential equation. What he says, though, in the beginning, is that, apart from their "convenient simplicity" and "condensed power", these methods are chiefly valuable in revealing connections of language with thought. The passage from [1877, vii-viii] that follows reminds us of earlier statements given in his introductions to MAL and LT:

The question of the true value and proper place of symbolical methods is undoubtedly of great importance. Their convenient simplicity—their condensed power—must ever constitute their first claim upon attention. I believe however that, in order to form a just estimate, we must consider them in another aspect, viz as in some sort the visible manifestation of truths relating to the intimate and vital connexion of language with thought—truths of which it may be presumed that we do not yet see the entire scheme and connexion. But, while this consideration vindicates to them a high position, it seems to me clearly to define that position. As

discussions about words can never remove the difficulties that exist in things, so no skill in the use of those aids to thought which language furnishes can relieve us from the necessity of a prior and more direct study of the things which are the subjects of our reasonings. And the more exact, and the more complete, that study of things has been, the more likely shall we be to employ with advantage all instrumental aids and appliances<sup>(2)</sup>.

Once more, we see De Morgan's early "doctrine", of making the student's grasp "more intellectual and less mechanical" revive here. The fear of mechanical abuse of formal procedures was stressed by Boole in both MAL and LT. We proceed now to chapter 16 where Boole introduces symbolical methods. The chapter opens with the following definition[1877, 381]:

The term symbolical is, by a restriction of its wider meaning, applied more peculiarly to those methods in Analysis in which operations, separated by a mental abstraction from the subjects upon which they are performed, are expressed by symbols in whose laws the laws of the operations themselves are represented.

Thus  $du/dx$  is written symbolically in the form  $d/dx u$ , the symbol  $d/dx$  denoting an operation of which  $u$  is the subject. In thus expressing an operation by a symbol, in studying the laws of that symbol, and in founding processes and methods upon those laws, we introduce no strange or novel principle of Language; for it is the very office of Language to express by symbols the procedure of Thought.

He then introduces the distributive, commutative and index laws according to which  $d/dx$  operates, proving next that any rational and integral function of  $d/dx$  is employed in accordance with these laws [1877, 383-385]. The next step is to define the inverse of the operation  $(d/dx+a)$ , where  $a$  is a constant. Let the result of the direct operation on subject  $u$  be  $v$ :

$$(88.1) \quad (d/dx+a)u=v.$$

If  $u$  is sought, "analogy suggests the notation"

$$(88.2) \quad u=(d/dx+a)^{-1}v,$$

where  $(d/dx+a)^{-1}$  represents the inverse procedure in question "only in its inverse character". What it conveys in fact is

expressed by

$$(88.3) \quad (d/dx+a)(d/dx+a)^{-1}v=v.$$

"The inverse procedure is thus presented as one, the effect of which the direct operation simply annuls. This is its definition" [1877, 386].

Based upon his study of linear equations with constant coefficients by non-symbolical methods, Boole provides the result of the operation at the right-hand side of (88.2) to be the following:

$$(88.4) \quad u=e^{-ax}\int e^{ax}vdx$$

[1877, 388]. We now notice that (88.1), (88.2) and (88.4) are equivalent to (87.7)-(87.9) provided in his manuscripts for the definition of division in logic. Now, it is clear that the equivalent for (88.3) was missing in his earlier study for the definition of  $x/z$ . This omission is important, for as we saw above, it was exactly through (88.3) that he defined the inverse of differentiation in his textbook [see 8.7].

His present exposition is characterized by elegance and clarity. Boole gives specific examples and proceeds very gradually from the particular to the general, following exactly the reverse order of that in his early researches, as in his [1844]. However, in many aspects he becomes cryptic and certain steps are omitted in a similar manner as he had done in MAL and LT. It is surprising to notice that despite the richness of the material included in his textbook and the elegance of exposition, he ~~does not~~ enter into a detailed study of inverse operations.

We notice, for example, that he implicitly makes use of the property  $(fg)^{-1}=g^{-1}f^{-1}$  as well as of the property which says that if  $f$  is distributive, then  $f^{-1}$  is distributive too, where  $f, g$  stand for  $d/dx$  [1877, 389, 404]. Boole must have been aware of Murphy [1837] or Carmichael [1855] where such properties are discussed in full. However, the absence of such references is striking and seems contradictory with his educational concerns. The properties of inverse operations are studied only partially and this must have been a source of difficulty for the student.

Having given some basic examples of integration by symbolical methods, Boole goes on to discuss "Forms purely symbolical".



The following passage gives evidence of the influence that his study of the nature of symbolical methods in his manuscripts on logic had upon his views in this work. We read [1877, 398-9]:

In any system in which thought is expressed by symbols, the laws of combination of the symbols are determined from the study of the corresponding operations in thought. But it may be that the latter are subject to conditions of possibility as well as to laws when possible. And thus it may be that two systems of symbols, differing in interpretation, may agree as to their formal laws whenever they both express operations possible in thought, while at the same time there may exist combinations which really represent thought in the one but not in the other. For instance, there exist forms of the functional symbol  $f$ , for which we can attach a meaning to the expression  $f(m)$ , but cannot directly attach a meaning to the symbol  $f(d/dx)$ . And the question arises. Does this difference restrict our freedom in the use of that principle which permits us to treat expressions of the form  $f(d/dx)$  as if  $d/dx$  were a symbol of quantity? For instance, we can attach no direct meaning to the expression  $e^{hd/dx} f(x)$ , but if we develop the exponential as if  $d/dx$  were quantitative, we have  $e^{hd/dx} f(x) = (1 + hd/dx + \dots) f(x) = f(x+h)$  by Taylor's theorem. Are we then permitted, on the above principle, to make use of symbolic language; always supposing that we can, by the continued application of the same principle, obtain a final result of interpretable form?

Now all special instances point to the conclusion that this is permissible, and seem to indicate, as a general principle, that the mere processes of symbolical reasoning are independent of the conditions of their interpretation. In the few instances we may have occasion to employ, verification will be easy. We take occasion to notice that, whatever view may be taken of this principle, whether it be contemplated as belonging to the realm of a priori truth, or whether it be regarded as a generalization from experience, it would be an error to regard it as in any peculiar sense a mathematical principle. It claims a place among the general relations of Thought and Language.

We thus see Boole stressing once more the independence of

symbolical reasoning from the conditions of the interpretation of the symbols. In other words, symbolical methods are not peculiar to mathematics<sup>(3)</sup>.

In chapter 17 he exposed his general method as in his [1844]. Having provided the solution of the equation.

$$(88.5) \quad \frac{d^2u}{dx^2} - \frac{i(i+1)}{x^2} u + q^2 u = 0,$$

which is the general form of the earth-figure equation, he went on to determine, according to his discussion of "Forms purely symbolical" cited above, the solution of the partial differential equation

$$(88.6) \quad \frac{d^2u}{dx^2} - a^2 \frac{d^2u}{dy^2} - \frac{i(i+1)}{x^2} u = 0.$$

Comparing the forms of equations (88.5), (88.6), he claimed that the solution of the latter could be obtained from the former by purely symbolical substitutions; that is it suffices to change  $q$ ,  $c_1$ ,  $c_2$  in the solution of (88.5) into  $a d/dy$ ,  $\phi(x)$  and  $\psi(x)$  respectively, where  $\phi$  and  $\psi$  are arbitrary functions [1877, 424-426]. Having done so Boole goes on to explain why the arbitrary functions in the solution of (88.6) have to be placed strictly after the operator  $e^{ax d/dy}$  and not before.

This distinction had been needed in the former case; for  $e^{qx}$ , where  $q$  is a constant, commutes with constant  $c$ . However, in the latter case the "reason why the arbitrary function  $\phi(y)$  must be placed after  $e^{ax d/dy}$  and not before it, is that, in the derivation of the exemplar form, the arbitrary constant takes its place after, and not before  $e^{qx}$ ", that is,

$$(88.7) \quad (d/dx - q)^{-1} 0 = e^{qx} (d/dx)^{-1} 0 = e^{qx} c.$$

So, transposition can not be effected in the latter case since  $y$  and  $d/dy$  are not commutative. He went on to say: "The principle here illustrated, and which is a very important one, is that all conclusions founded on community of formal laws should stop short of interpretation. The form should be kept distinct from the matter". Thus, "there is perfect analogy between the theorems

$$(d/dx - q)^{-1} 0 = e^{qx} (d/dx)^{-1} e^{-qx} 0$$

and

$$(d/dx - a d/dy)^{-1} 0 = e^{ax d/dy} (d/dx)^{-1} e^{-ax d/dy} 0.$$

but not between the theorems.

$$(d/dx - q)^{-1} 0 = ce^{qx}$$

and

$$(d/dx - ad/dy)^{-1} 0 = e^{-x} \int a dy \phi(y)$$

because in the formulation of the latter interpretation has been employed" [1877, 426].

Boole's final remark at page 427 is worth citing:

The above example is one in which Monge's method of solution would fail, except for the particular case of  $i=0$ . And this gives occasion to the remark that symbolical methods are not, as they have sometimes been supposed to be, valuable only as abbreviating the processes of analysis. There are innumerable cases in which they afford the only proper mode of procedure<sup>4</sup>.

To a certain extent this conclusion had proved equally true for logic. Traditional syllogism had a restricted area of applications. In both MAL and LT he had pointed out instances which could not be treated by Aristotelian logic. Had he had succeeded in translating all the results of his formal method within ordinary language he would have proceeded to a publication. Symbolical methods might not always be necessary but there are instances where indeed they are indispensable once their power, beyond conciseness, elegance and efficiency in calculations, is illustrated.

In the last years of his life Boole was particularly interested in the study of singular solutions. According to the testimonies of his wife, he viewed this subject from a metaphysical angle [7.1; M.Boole 1972, 69]. In fact, we notice in his textbook on differential equations an instance of his duality doctrine, which had been of fundamental importance to him in MAL and LT, in connection with his study of singular solutions.

Chapter 8 on singular solutions opens as follows: "In the largest sense which has been given to the term, a singular solution of a differential equation is a relation between the variables which reduces the two members of the equation to an identity, but which is not included in the complete primitive" [1877, 139]. Boole stressed next the peculiar property of a singular solution that although it "is not included in the complete primi-

tive, it is still implied by it". He then introduced positive and negative elements as follows [1877, 140]:

It is important that the two marks, positive and negative, by the union of which a singular solution of a differential equation of the first order is characterized, and by the expression of which its definition is formed, should be clearly apprehended. 1st. It must give the same value of  $dy/dx$  in terms of  $x$  and  $y$ , as the differential equation itself does. This is its positive mark, a mark which it possesses in common with the complete primitive, and with each included particular primitive. 2ndly: It must not be included in the complete primitive. This is its negative mark. Upon the analytical expression of these characters the entire theory of this class of solutions depends.

Discussing the last chapter of MAL in 7.7, we had noticed a similar remark by Boole about the positive and negative foundation of the theory of categorical propositions. By the former (positive mark) he implied that there exists a universe of conceptions within which each variable of the proposition either represents or does not represent a given class. By the latter he implied the restrictions we have to impose upon the variables when we solve an elective equation [7.7; MAL, 77-78]:

Boole's dualistic tendency in both logic and mathematics further justifies our claim that he viewed logic and mathematics as two independent branches of a universal calculus of symbols. This calculus of symbols is based upon certain formal laws which are dictated by the laws of thought. Symbolic procedures do not depend upon interpretation of the symbols and this property he claimed repeatedly not to be peculiar to mathematics but related to the connection between language and thought [see the passage quoted above from "Forms purely symbolical" and references in (3) above].

Through abstraction from mathematics Boole discovered the laws of thought and, based upon the property of symbolical procedures (mentioned above), he applied mathematics to logic. He then delved further into the nature of symbolical procedures expressing his mature conclusions which are now illustrated in his textbook on differential equations. In the course of this approach

certain philosophical or religious beliefs are involved. Particularly the duality law

$$(88.8) \quad x + (1-x) = 1$$

is the symbolical expression of Presocratic philosophy that revived through the ages<sup>(=)</sup>.

M.Boole called (88.8) the "Mystical equation". She held that chapter 8 on singular solutions "contains much genuine metaphysical truth expressed in mathematical terminology" [M.Boole 1972, 68]. This seems a bit exaggerated, but we have seen other instances too, drawn from the "Preface" and chapters 16-17 above, in which Boole's philosophical consideration of the relation between language and thought is prominent.

We could perhaps correlate the positive and negative marks that characterize a singular solution of (88.8), in the following sense. Let A be the collection of all <sup>proper</sup> subclasses of 1. Then A is the complete solution of (8.8). Now  $x=1$  satisfies (88.8) -the positive mark of 1- but it does not belong to A -the negative mark. However, 1 is implied by A in the same way as the singular solution of an equation is the envelope of the curves of all other solutions. Another correlation would be to say: there is a concept 1, which has the positive mark (x) and the negative one (1-x) which together make up the universe 1. This analogy is due to [Grattan-Guinness 1985, 64].

So, once more we have an instance of analogy between differential and elective equations, an analogy which most probably Boole had constantly in mind while working on his logic. The tendency towards unity had puzzled him a lot. In [1844] he had tried to put the basis of a "General method in analysis" uniting under its realm all possible aspects of differential and integral calculus. Few months before he died he wrote enthusiastically to his wife "I have made out what puts the whole subject of Singular Solutions into a state of Unity" [M.Boole 1972, 68-70; 1931, 252-3]. We have no clues about what Boole had meant by that. The fact is that the pursuit for unity had motivated largely his researches in both mathematics and logic<sup>(=)</sup>.

M.Boole recalls that her husband often tried to translate equation (88.8) in words. "But many circumstances combined to hinder his publishing any word versions of it". She also informs

us that to Boole's great joy, Père Gratry the Oratorian formulated the truth that this equation expresses "in language on which George Boole felt he could not improve" [1972, 63].

In 1879 M.Boole wrote a book on the Mathematical psychology in Gratry and Boole. In a rather naïve way, she presents her own naturalistic psychological interpretation of Gratry's philosophical study of the foundations of the infinitesimal calculus together with instances from Boole's mathematical writings focusing on his singular solutions. She exploited Gratry's and Boole's writings in order to prove that the doctrines on which they were based could be applied in elementary education and mental hygiene<sup>(2)</sup>.

The last section of this chapter will be devoted to a brief study of Boole's and Gratry's epistemological views. In an attempt to point out what was in Gratry's Logique [1855] that Boole appreciated so much, we will have an opportunity to stress certain similarities and contrasts in the philosophical writings of these two men.

#### 8.9. On the epistemologies of Boole and Gratry; similarities and contrasts.

In a rather concise and obscure manner Boole presented his broader views on the philosophy of science in the last chapter of LT. Mary Boole's claim, that Gratry's Logic [1855] expressed in a lucid language what her husband had meant to say in the concluding chapter of LT, motivates a comparative study between the epistemological views of these two men. In fact, what renders this study interesting is that apparently they were both unaware of each other's work while composing their works on logic<sup>(1)</sup>.

Gratry studied traditional logic as it prevailed in the 18th century [1944, 267-350]. Similarities with Boole are occasionally perceived only in his ample, almost poetic, discussion on induction which covers the last chapter of LT. At the same time certain contrasts will be noticed, as their mathematical backgrounds differ a lot<sup>(2)</sup>. The issues discussed by both men could be listed for convenience in the following order:

- i) Scientific induction,

- ii) The nature of mathematical truth,
- iii) The form-matter issue,
- iv) The doctrine of infinity and unity, and
- v) The comparative science.

Juxtaposition of passages from Gratry [1944] and Boole's LT will bring to light their common, unlimited pursuit of "Truth", the similarity of their views on the role of induction in science, their belief that mathematical truths are eternal and independent from experience, their tendency for abstraction, generalization and unity, as well as, their common speculations on the role of the philosophy in science. Strongly concerned with education, they both claimed that the isolation of the study of different branches of science is disastrous in its advancement. In fact, Gratry will be more emphatic than Boole on the latter issue and under a Leibnizian influence, he will urge the study of the "comparative science".

During our study we will have an opportunity to notice some contrasts in their choice of examples. For, while Boole's mathematical background was Lagrange's algebraic calculus, Gratry's own was that of <sup>mathematical</sup> analysis. Moreover, Gratry had attended Cauchy's lectures as a student at the Ecole Polytechnique in the 1820's; he thus perceived the shaky foundations of Lagrange's calculus and was very critical <sup>of</sup> the latter's avoidance of the infinitesimal calculus [1944, 86, 425-7, 565].

In addition, there is one more issue to be mentioned. As will be seen, Gratry's discussion is often characterized by strong religious sentiments. It is more than probable thus, that "Boole saw Gratry's mystical and religious attitude towards logic and philosophy as a perfect complement to his own precise and mathematical treatment of these subjects". Religion will not be discussed in this section; I would like, however, to mention that Gratry sounds more optimistic and confident, while Boole is more vague and sceptical upon religious implications<sup>(3)</sup>.

Gratry was very fond of Leibniz. His very first chapter opens with Leibniz's remarks on the usefulness of logic. According to Leibniz, the art of logic can not develop "without the aid of the most profound parts of mathematics". Gratry believed after Leibniz that this applied to the infinitesimal calculus. For, "It

was by reflecting on the algebraic and geometrical method of the infinitely small that we ourself first understood the existence of the chief process of reason, of which elementary logics to this day do not speak, or indicate only vaguely" [1944, 113-4; 18-19 fn 7].

Which is the "chief process of reason" then? According to Gratry it is "induction". The "syllogism" or "deduction" is given very little attention in his book. Induction, perceived in its scientific and mathematical form, is an indispensable tool for discovery. Deduction is consequently useful as a method for proof<sup>4</sup>. So now we will tackle our first issue, that of scientific induction. This issue is principally manifested in both Gratry and Boole by Kepler. Let us see first in brief Gratry's views.

In his discovery of Mars's orbit, Kepler first mastered geometry, next gathered the facts, and finally eliminated "the variable and accidental connections between terms, in order to grasp the constant and simple relation of all terms [...] the ellipse". Scientific induction, holds Gratry, is "an affair of tact and genius". But, above all, it requires "clear reason, which if rightly developed, includes in itself ideas, abstract forms [...] and laws to which it seeks to reproduce phenomena, which, in fact tend naturally thus to reduce themselves" [1944, 79-80].

Boole distinguishes on similar lines between general propositions of science, derived by induction from a collection of observations, and those of necessary truth as in arithmetic, geometry and logic [LT, 403-4]. At page 402 we read his own description of scientific induction:

The study of every department of physical science begins with observation, it advances by the collation of facts to a presumptive acquaintance with their connecting law [...] and finally, the law of the phenomenon having been with sufficient confidence determined, the investigation of causes, conducted by the due mixture of hypothesis and deduction, crowns the inquiry.

Boole speculated next on "what is the nature of scientific truth, and what are the grounds of that confidence with which it



claims to be received". In Kepler's case, his conclusion was larger than his premises. "No principle of merely deductive reasoning can warrant such a procedure" [LT, 402-3]. He then went on to discuss the principle of "Order" in nature as the basis of inductive reasoning. Certain philosophers, he wrote, claim that we can infer only from particular to particular truths. On that he commented as follows:

Now whether it is so or not, that principle of order or analogy upon which the reasoning is conducted must either be stated or apprehended as a general truth, to give validity to the final conclusion. In this form, at least, the necessity of general propositions as the basis of inference is confirmed, —a necessity which, however, I conceive to be involved in the very existence, and still more in the peculiar nature, of those faculties whose laws have been investigated in this work.

We now proceed to a discussion of induction in mathematics and logic as a process of generalization based on abstraction and next we will focus on the nature of mathematical truths. Grätry claimed that men instinctively practice the dialectical process of induction in prayer, in poetry and science. "It seems to us that the five words "perception", "abstraction", "generalization", "analogy", and "induction", taken together, suitably related and adapted, reproduce all of the process that we want to describe" [1944, 373].

Perception is "the idea which the impression of an object produces in us". While impression relates to the soul, perception relates to the object. [1944, 373-4]. Abstraction "deals with subjects, leaving accidents out of the account". A classical example of abstraction in geometry forms the concept of the ellipse defined algebraically by  $a^2x^2 + b^2y^2 = a^2b^2$ . By means of "the algebraic language, all the individual conditions are blank, are indeterminate and abstracted: there remains only the pure and universal idea of the ellipse, although the sentence also indicates the inevitable existence of individual characters" [1944, 375-6].

Through abstraction we achieve generalization since the idea of the ellipse represents an infinite number of ellipses while we

have thought of only a finite number. Moreover, in such abstractions we perceive finally a simple "unity" or "law" which "reigns in the midst of the infinite variety of points of a geometrical form, whether abstract or individual". This last operation is more than "a generalization property speaking; it is an induction founded on an analogy" [1944, 376-7]. Finally, "induction", supported by a single particular case, "affirms the universal with composite certainty". It is upon such certainty, wrote Gratry, that the infinitesimal calculus is founded [1944, 378].

In some manuscript notes that formed part of the sequel to LT, Boole defines the inductive process which equally applies to mathematics and logic as follows:

For the most part we are only able to arrive at a knowledge of the universal by means of that of the individual and the particular. By studying Particular manifestations of thought we ascend to its general laws; and this we do not so much by comparing particular forms and instances and selecting the truth which is common to them all, as by some deeper faculty of insight enabling us, when contemplating some general truth manifested under particular forms or conditions, to perceive how far such conditions are necessary and how far they are accidental<sup>(3)</sup>.

Discussing the nature of mathematical truths, both men drew on geometry. Boole wrote that in geometry we "perceive the truth of the general axioms in the very act by which we deduce the special conclusions" [LR, 214; 8.5, (2)]. Talking of geometrical figures [LT, 405], he wrote:

It seems to be certain, that neither in nature nor in art do we meet with anything absolutely agreeing with the geometrical definition of a straight line or of a triangle [...]. Although the perfect triangle [...] exists not in nature, eludes all our powers of representative conception, and is presented to us in thought only, as the limit of an indefinite process of abstraction, yet, by a wonderful faculty of the understanding, it may be the subject of proposition, which are absolutely true. The domain of reason is thus revealed to us as larger than that of imagination.

Gratry, while discussing the concept of the ellipse [1944,

391)), wrote:

Mathematical truths are, indeed, eternal, immutable, and have their reality in God himself. Is there anywhere in created nature a perfect sphere, with an absolutely simple center and an infinite periphery, in the sense that it would be composed of an actual infinity of infinitely small elements? Not at all. This ideal, absolute geometry subsists only in God; and it is in God, indirectly through us, that we see the mathematical truths.

The similarities in these two passages are striking. Both men believed strongly that mathematical truths exist independently from experience. References to Plato are to be found in connection with this issue in both works [LT, 404; 1944, 36-7, 86, 432]<sup>4</sup>. From the previous quotations we also see that they viewed induction as allied with abstraction and generalization as well as with the concepts of unity and infinity. Before we proceed to a discussion of the latter issues we will see an example from the infinitesimal calculus by means of which we have one more illustration -beside the earlier geometrical one- of Gratry's form-matter issue.

Assume we have a curve and two points on it whose position can vary indefinitely. Analysis, in other words the infinitesimal calculus, decomposed the given complex of the relation of these points into an indefinitely variable element,  $\Delta x$ , and the invariable,  $f'(x)$ . By the inductive process upon which the infinitesimal calculus is based we can eliminate the variable element (difference) in order to retain the perfectly fixed (differential). This elimination is based in the assumption that all scattered points are collected into one. Drawing on Cauchy, Gratry wrote [1944, 81-3]:

Thus one studies the curve outside space, dispersion and quantity, in that ideal simplicity, where according to the words of a great geometer, the whole curve is assembled in one point, to the mind's eye. All of the qualities of the curve are seen in this point [...]. This is truly the model of the whole inductive process.

Gratry argued, by providing numerous geometrical examples,

that the infinitesimal calculus, being an application of one of the two essential operations of reason to mathematics, is indeed rigorous. He regreted that often rigour is connected only with syllogistic demonstration [1944, 418-8]. Moreover, Gratry attacked Lagrange's "holy horror of the infinite": perceived even in the very title of his [1797] '7'. "The ideas of limit and of infinitely small shaped the primitive existence of geometry", he wrote [1944, 69-70]. But how did Gratry and Boole view infinity? Boole has very little to say on this subject. In [LT, 419] we read:

We can never be said to comprehend that which is represented to thought as the limit of an indefinite process of abstraction. A progression ad infinitum is impossible to finite powers. But though we cannot comprehend the infinite, there may be even scientific grounds for believing that human nature is constituted in some relation to the infinite. We cannot perfectly express the laws of thought, or establish in the more general sense the methods of which they form the basis, without at least the implication of elements which ordinary language expresses by the terms "Universe" and "Eternity". As in the pure abstractions of Geometry, so in the domain of Logic it is seen, that the empire of Truth is, in a certain sense, larger than that of Imagination. And as there are many special departments of Knowledge which can only be completely surveyed from an external point, so the theory of the intellectual processes, as applied only to finite objects, seems to involve the recognition of a sphere of thought from which all limit, are withdrawn.

Gratry shares Boole's view. In a footnote he wrote [1944, 419, fn3]:

To know and to analyse the infinite is not to understand the infinite. The infinite is incomprehensible, like God himself. Man can and ought to know God, and yet is unable to understand him. We shall never understand the infinite, even though we should succeed in knowing some clear truths about the nature of the infinite and its relations to the finite. That is why the incomprehensible side of the infinitesimal calculus always exists as such, even when the student see in it, as we do, the simple and rigorous application of

the one of the two operations of reason.

Up to this point we had a chance to notice multiple similarities between the views of these two logicians. Grätry admired Leibniz and drew on his work, Boole was also delighted to come across the latter's work late in his life [7.4, (4)]. Both were Platonists seeking for the absolute truth, Grätry through induction based on "clear reason", Boole by means of his "laws of thought". In fact, Boole had meant to write on induction in his sequel to LT, as we saw in the passage quoted in (5) above. But according to [Hesse 1952, 64] nothing more was written by him on this matter. So, this must be a plausible reason why Boole felt that Grätry covered him through his lucid and ample discussion on induction which was enriched with numerous examples.

A basic contrast between them is the choice of examples and the fact that Boole omitted any illustration whatsoever of the issue of infinity. While Grätry drew constantly on the calculus, Boole remained silent<sup>(e)</sup>. However, one common example from analysis is traced; that of an inscribed polygon in a curve. In both cases the similarity lies in the emphasis paid on the often neglected "law of continuity". It is of interest to see the different language used, as well as the different context within which their discussion are carried. We first quote from Grätry's book.

"Does any reader care to insist and maintain", asked Grätry, "that from the polygon to the curve there is continuity, identity [...]? He would be making a great error [...] it is not always possible to infer from polygons to curves [...]. Further, [...] the abyss between the finite and the infinite exists in any case between the curve and the polygon [...]. And should anybody want to maintain that the infinite (the infinitely large and the infinitely small) is only a particular case of finite quantity, that too would be absolutely false" [1944, 64-65].

With an example of a semi-convergent series Grätry tried to make "the abyss which separates the infinite from the finite understandable". As a result he stressed that "it is not possible to infer except where there is continuous and indefinite convergence". Then Grätry went on to show that when the inscribed

polygon approaches the curve certain elements of it are analogous to properties of the curve while others are not. He added one more example in order to illustrate the "singular" properties of the infinite; he referred to a finite series with a certain property which does not hold at the limit<sup>(9)</sup>.

Speculating on the analogies and contrasts from the series to its limit, Grätry came to the following conclusions:

To determine in general what the analogous properties are and what the contrasting, would be achieved by a theory of geometrical induction, and would greatly advance the general theory of induction. Quite probably a general theorem exists in the science of the future which will fix these general conditions of induction. I think of this theorem as analogous to the beautiful theorem of M. Cauchy who, after having shown that the Taylor series, one of the types of series in equation with its limit, can often lead to error; established the conditions of its exactitude in a single statement.

Then followed Cauchy's theorem on the <sup>condition for</sup> convergence of the Taylor series for  $f(x+h)$  [1944, 65-67 fn 10].

In his textbook on finite differences [1860, 137] Boole stressed that "a careful analysis of the meaning of the word limit will shew us that it is not true that every result of the Calculus of Finite Differences merges when the increments are indefinitely diminished into a result of the Differential Calculus".

Examining the same example Boole distinguished between two senses, a more and a less complete, in which a curve can be said to be the limit of a polygon. In both cases every side of the polygon tends indefinitely to coincidence with the curve. But in the more complete sense  $\Delta y/\Delta x$  in the polygon tends to  $dy/dx$  in the curve, whereas in the less complete sense the linear elements of the polygon "do not tend to coincidence of direction with the curve" [1860, 138]. Grätry has included a very similar discussion when he asked "But is the curvature the limit of the successive angles which form the sides of the polygon?" [1944, 66]. For, the two cases that Boole distinguished depended upon the state of the angles of the polygon.

Finally, Boole meant with this example to illustrate the principle of continuity which is evident in the first case where all the angles of the polygon tend to  $n$  as their limit [1860, 138]. However, the discussion -strictly within the sphere of finite difference and differential equations- involves no reference to induction, metaphysics or to Cauchy's theorem.

Drawing on the above discussion of the polygon -curve example<sup>(1)</sup> we have an opportunity to point out some further similarities between the interests of these two logicians. Grätry, like Boole, was also interested in singular properties of mathematical entities. Boole, having a strong background in differential equations, dealt extensively with singular solutions [8.8]. Grätry's background in infinite series urged him to contemplate the paradoxes lying in the foundations of the infinitesimal calculus [see (9) above]. In fact, in the same footnote where he referred to Cauchy's theorem, Grätry appealed to Abel's speculations on the shaky foundations of analysis. How can one apply operations to infinite series as if they were finite, Abel had wondered. Grätry exclaimed consequently his desire that "some great logician or a geometer" resumed this research [1944, 66-67, fn 10].

Another point we notice is Grätry's desire for a unique inductive theory via which the theory of infinite series could be successfully tackled. We remind here the reader of Boole's search for a universal method in analysis via which one could deal with the whole of differential and integral calculus. Moreover, both men viewed their aims from a metaphysical, psychological angle. Grätry sought to study the psychological element lying in the foundations of analysis; Boole, based on his metaphysical doctrine of duality, aimed to render the study of singular solutions in a state of unity [8.8]. A rather naïve interpretation of their psychological and metaphysical doctrines is [M.Boole 1897, 44-45].

We now proceed to the last two issues of our study, the doctrine of unity and the role of philosophy in science. Grätry wrote in [1944, 322]:

Every mind must work towards unity, as the human spirit taken in its entirety must tend toward unity and work toward its own

necessary centralization, toward the reciprocal communication of the parts in the whole.

And in his extensive chapter on Kepler, [1944, 387] Grätry wrote that Kepler saw the idea of the ellipse "as the most perfect symbol of unity, uniformity, regularity, simplicity in plurality and the most perfect harmony".

We are familiar by now with Boole's own views on the doctrine of unity [7.1, 7.7, 8.2, 8.7, 8.8]. In his last chapter of LT we see a historical review of the doctrine of unity in various theories on lines similar to those followed in "Claims" [7.1, 8.2; LT, 412-415]. The result of Boole's speculations is encapsulated in the following quotation [LT, 415-6]:

The attempts of speculative minds to ascend to some high pinnacle of truth, from which they might survey the entire framework and connexion of things in the order of deductive thought, have differed less in the forms of theory which they have produced, than through the nature of the interpretations which have been assigned to those forms. And herein lies the real question as to the influence of philosophical systems upon the disposition and life [...] . Herein too may be felt the powerlessness of mere Logic, [...] to resolve those problems which lie nearer to our hearts, as progressive years strip away from our life the illusions of its golden dawn.

Thus, mere logic or philosophy can not form a sufficient basis of knowledge. Neither does, wrote Boole, the study of mathematics only. The cultivation of the mathematical or deductive faculty is only a part of intellectual discipline. "The prejudice which would either banish or make supreme any one department of knowledge or faculty of mind, betrays not only error or judgment, but a defect of that intellectual modesty which is inseparable from a pure devotion to truth" [LT, 423].

In the lengthy "Introduction" to his Logic, Grätry had emphasised that there is only one art of reasoning and not three peculiar to physics, metaphysics and mathematics as certain writers argued [1944, 16, 90]. And in the chapter of his treatise he stressed that the exclusive methods of philosophy is



the main cause of our error [1944, 164]. The true method consists "in the unity of all sources of knowledge", he wrote. And drawing once more on Leibniz, he said:

The mind is a peculiar element, a substance with a surprising nature. I urge you to study comparative science; I ask you, for that purpose, to study everything: theology, philosophy, geometry, physics, physiology, history<sup>(11)</sup>.

We thus see that both men believed that the laws of thought are universally true and equally applied to all sciences. Moreover, they held that exclusive methods and the isolated character of scientific teaching is a main cause of error in the development of science [1944, 164, 552; LT, 424]. But what about the role of philosophy in science? Both regretted the inability of philosophy "to keep pace with the advance of the several departments of knowledge, whose mutual relations it is its province to determine" [LT, 424; see also 1944, 35, 94-5, 550-2].

While Boole is rather vague on this matter, Gratry provides the solution: "There you have the strictly scientific term which it is indispensable that we should introduce into philosophy" [1944, 167]. Drawing on Cauchy's theorem on convergence in [1944, 66, fn 10] Gratry wrote "If philosophers worked in the same fashion, learning to preserve scientifically and patiently after the truth they also would make discoveries".

Our final remark will be devoted to the interaction between logic and mathematics. In different ways, as we saw, both men believed that the latter science is indispensable to the former. But while Boole regarded mathematics as a rigorous enough discipline, Gratry, aware of its paradoxes, prophetically perceived the necessity of a logical inquiry into its foundations. In the following quotation from Gratry's letter to his "illustrious master and friend" Cauchy, we see an instance of De Morgan's belief that mathematicians should be logicians and vice-versa<sup>(12)</sup>. Gratry wrote in his "Introduction" [1944, 57]: "In logic I am your equal. I have been preoccupied with logic all my life, as you have been with mathematics; but perhaps I have studied mathematics more than you have logic". We had the opportunity to notice another instance, where Gratry commenting upon

Abel's letter expressed his desire that a logician undertook the study of infinite series. His desire was somehow fulfilled by Cantor's set theory and the consequent growth of mathematical logic.

## Chapter 9

Concluding survey: looking back and glimpsing ahead.

### 9.1 Introduction

Between the mode of generation and the fruit,  
there is an enormous difference<sup>(1)</sup>.

Paul Valéry.

If there is an "enormous difference" between the way scientific theories are conceived and their final outcome in immediate applications, then we might say that there is often hardly any link between their mode of generation in previous centuries and what survives from them today. Every mathematician in our century is acquainted with the names of Babbage, Herschel, De Morgan and Boole, but what actually survives from their contributions to mathematics and logic is not only rather limited, but also hardly representative of their inquiries and, in some cases, misleading.

In current textbooks of set theory, elementary or advanced, Boole and De Morgan are familiar to us due to what came to be known as De Morgan's laws and Boolean algebra [for example, Halmos 1970, 17-18; Jech 1978, 144, 159]. Students are ignorant either of Boole's connection with differential operators or of De Morgan's with the theory of relations—subjects covered in the curriculum of most 20<sup>th</sup>-century universities<sup>(2)</sup>—let alone of the educational and philosophical concerns which had played such a vital role in the generation of their work. Moreover, the idea circulates that Boole rather than Babbage is—after Aristotle and Leibniz—one of the father figures of computers [7.1, (22)].

As a result of recent historical studies, many gaps in our knowledge are filled and certain misinterpretations of the fruit of the work of our four main figures are eliminated. We would like to stress, for example, the edition of Babbage's works [1989], Merrill's comprehensive book on De Morgan's logic of relations [1990] and Hailperin's paper "Boole's algebra isn't Boolean algebra" [1981] [see 6.4, (2) and 7.1, (21); 8.4, (6)]. However, what above all was missing so far was a detailed inquiry into the mathematical roots of their logics, and this omission motivated the shaping of this thesis which has involved, among

other things, a wider inquiry upon predecessors and contemporaries, covering a period of roughly seventy years.

Among the main outcomes of our study was to investigate in depth the pioneering work of Babbage and Herschel in functional and D-operator algebras respectively—which foreshadowed the symbolical methods put forward by Murphy, De Morgan and Gregory in the late 1830's— and to notice the degree to which these algebras influenced respectively De Morgan's and Boole's logics. In so doing we revealed several interesting minor or major contributions of neglected figures, as well as a most intricate network of influences between the cultivators of these algebras, differential equations and logic in the first half of the 19<sup>th</sup> century in England and Ireland, linking finally Boole's work on the calculus of operations with Cayley's and Sylvester's calculus of forms.

In this chapter we seek to evaluate the fruit of the work of our principal figures according to the following plan. Sections 9.2 and 9.3 cover a brief survey of the development of symbolical methods and their impact in England during the periods 1800–1839 and 1840–1860 respectively. In 9.4 we examine the degree of utility of these methods in the realms of mathematical sciences in the second half of the century, covering the period between the two crucial textbooks on differential equations, Boole [1859] and Forsyth [1885].

The rest of the chapter concerns the impact of Boole's and De Morgan's logics on English, German and American logicians. Section 9.5 deals with Boole's semi-followers, focusing on Jevons and Venn, while 9.6 touches upon the joint influence of Boole and De Morgan upon H. MacColl, C. Peirce and E. Schröder. In 9.7 we examine some interesting links between logic, D-operators and chemistry, with note taken of the work of the chemist B. Brodie in 1866–1877. Our survey concludes in 9.8 with a brief epilogue to serve as a basis for the future researcher<sup>(3)</sup>.

## 9.2 English mathematics and logic: 1800–1839; the main issues.

We begin with a systematic commentary upon the period 1800–1839, paying attention to the development of logic and mathematics at that time as studied separately in [6.2–6.5] and [chaps

2-3;4.1-4.2] respectively. To facilitate our discussion we divide this period in two subperiods: (1800-1830), and (1830-1839), providing also corresponding tables with the main dates, names, issues and publications upon which we will comment below.

Table 1  
Revival of logic in England: 1800-1830.

<u>Main works</u>	<u>Main issues</u>
Kirwan 1807	(i) Aristotelian logic viewed as an indispensable tool in the study of law, theology, chemistry, biology and mathematics
Whately 1826	(ii) Logic divided in "Art" and "Science"
Bentham 1827	(iii) Analogies perceived between algebra and logic

Table 2  
Importation and development of French "analytics" in England: 1800-1830.

<u>Main works</u>	<u>Main issues</u>
Woodhouse 1803;1809;1818 Brinkley 1807	(i) Educational reforms
Babbage } 1813-1818 Herschel }	(ii) Study of Laplace's <u>Mécanique Céleste</u>
Lacroix 1816 (translation) <u>Examples</u> 1820	(iii) Substitution of Lagrange's series-based calculus for Newton's fluxional calculus
Airy 1826	(iv) Generality of methods
Babbage 1827	(v) Notation

Table 3

The effects of the revival period on mathematics and logic: 1830-1839; foundational concerns<sup>(1)</sup>.

<u>Dates</u>	<u>Main works</u>			<u>Differential equations; physical astronomy</u>
	<u>Education</u> <u>Algebra</u> <u>Logic</u>	<u>COF</u>	<u>COO</u>	
1830	Peacock's <u>Algebra</u>			Whewell on Laplace's coefficients
1831	De Morgan's SDM			
1833			Murphy on distributive operations	Murphy's <u>Electricity</u>
1835	De Morgan's review of Peacock [1830]			
1836		De Morgan's treatise on the calculus of functions		Pratt's textbook; A.J. Ellis on the EFE
1837			Murphy on the COO-inverse operations	Greatheed on partial differential equations
1839	>> FNL Solly W.Hamilton		W.R.Hamilton on Herschel's theorem	Gaskin on the EFE; Gregory on the COO

Main issues: Clarifications in the instruction of algebra, logic and physical astronomy; a rigorous foundation of the calculi of operations and functions; new effective methods for the solution of differential equations other than series.

During the first decades of the century there was no significant contact between logicians and mathematicians. In this respect the former were more poorly equipped than the latter to achieve their aims though the necessity for revival and updating was equally strong. Moreover, they shared a common interest in the study of language and signs, as well as in the adoption of a systematic scientific approach. Logicians gradually restored the neglected role of Aristotelian syllogism, regarding logic both as a system of truths worth of study and development -as a science- and as an indispensable tool to avoid fallacies in the study of

other sciences -as an art [6.2 text and (2),(7),(8); 6.3 text and (2)]. For mathematicians, on the other hand, Lagrange's and Laplace's series-based methods, provided the powerful tools for the discovery of truths (as a science), and for the application of the results obtained to the mathematical sciences(as an art) [2.7, text above (27.1);2.8,(2);2.9,(4)].

In addition, we observe that generality of methods, condenseness and symmetry of language, unity and analogy were among the chief concerns for both parties on purely independent lines -with perhaps only a partial common influence from Condillac [2.9,(3);6.2,(5),(1);6.3,(1),(2)]. Compare, for example, the symmetry in G.Bentham's *partly* mathematical system (63.2) in his book [1827] and Babbage's concerns for symmetry and conciseness in mathematical notation in his paper on the influence of signs [1827; see 2.9 text and (7)]. We might also note Whately's and Babbage's nominalistic attitudes [6.2,(9) and 2.9,(8) respectively], or Kirwan's, Tooke's, J.Bentham's and Babbage's common pursuit of a universal language [6.2,(3),(4); 6.3,(1); 2.9,(1)].

Such similarities, partly due apparently to a more or less common philosophical background, are above all indicative of the common concerns-besides educational updating- that characterized the independent but simultaneous attempts for the revival of logic and the calculus during the period 1800-1830 [Tables 1 and 2]. Certainly the tendency to draw on analogy and to attain a relatively high level of abstraction and generalization is stronger in the case of the analysts, most particularly Babbage and Herschel. Foreshadowing Boole's interest in a general method in analysis, Babbage and Herschel regarded respectively the methods of the calculi of functions and operations as general and uniform means of discovering new truths and of treating problems of analysis, physics, geometry, chemistry and the calculus of variations [2.3-2.9].

The interesting and close collaboration of Babbage and Herschel, as well as the peculiar characteristics of their individual published and unpublished contributions have been discussed in chapter 2. We would like to underline here some of the most important issues. Under the influence of Lagrange and Laplace [1.5], both analysts drew on analogy between functional, differential and exponentation indices, and, accordingly, between functional, differential and algebraic equations [2.7]. The fact

that Herschel contributed considerably to the development of Babbage's favourite subject of functional equations, suggesting amendments and new techniques, and, above all motivating the latter's interest in generalization [2.4 text and (8),(13)], has not been stressed in previous historical works.

Slightly bolder <sup>and</sup> <sub>^</sub> more ambitious than Babbage, Herschel at once assimilated and applied Arbogast's method of separation of symbols in 1814 [2.3,(17)], and undertook -unsuccessfully [2.4, (5),(6)]- to integrate the general form of the first-order functional equation (24.15). It should be noted that he was the first British analyst to solve a differential equation (23.11) by symbolical methods (23.12). He also developed Laplace's work on finite difference equations [2.3,(8); 2.4,(4)] as in Mécanique Céleste, borrowing Laplace's method of generating functions, and even the title of the latter's paper [1809]; [see 2.3,text and (14)-(18)]. Finally, he superseded Babbage in applications by proving the utility of the latter's transform  $\varphi^{-1}f\varphi$  (25.1) outside the realms of functional equations, <sup>and</sup> <sub>^</sub> elaborating Bromhead's unpublished work on the calculus of factorials [2.8,(7) and (28.13)]. Like Laplace, who had claimed in 1811 that Arbogast's separation of symbols formed part of his own "calcul des fonctions génératrices" [1.7,text below (3)], Herschel asserted that his D-calculus "expanded will include your [Babbage's] calculus of functions as a particular case" [2.9,(2)].

For all <sup>of</sup> <sub>^</sub> Babbage's fear to separate symbols of operation from those of quantity, <sup>despite</sup> <sub>^</sub> Herschel repeatedly urging him to, he did view functions as entities just as Herschel viewed differential and finite difference operators as mathematical objects other than numbers or geometrical concepts [2.7,text,(3)-(4); 2.9 before (10)]. But they both showed a deep concern for successful notation and uniform general methods [2.3,(17),(19); 2.4,(5),(8); 2.5; 2.9] being largely indifferent to conditions of possibility of solution or rigour [2.4,(6); 2.5,(17); 2.7,(12)]. After Monge [1.4] they assumed that the inverse of a function always exists and is unique [2.4,(3); 2.6,(6)], and they committed several conceptual and methodological errors in both published and unpublished material [2.3,(13); 2.6,(1),(3),(12),(13); 2.7,(12); (27.16)(27.41)]. Surprisingly, they did not know of Servois's laws [1.6; 2.6,(3)].

Babbage and Herschel did not dwell upon foundational questions; they followed the subjects of their investigation



where they led "seizing every relation which presented itself, without inquiring whether the conditions of solution were possible or not. And they were right in so doing, the general interest of the science being considered" [De Morgan 1836, art.1; 2.9,(10)]. However, their successors, following directly or indirectly on the lines laid down by the Analytical Society [2.1,(1),(2)], were concerned both with developing and diffusing French "analytics" and with restoring many of the errors or omissions left in their work. De Morgan, their most ardent follower and reader of their often obscure work, declared that errors, ambiguities and open questions are but the best challenge for the researcher to make "a step in knowledge" and should not be regarded as a defect [2.3,(4); 6.9,(20)]. Babbage and Herschel knew that and were often enthusiastic in noting errors in each other's work [2.6,(12)].

The major figures of the early period 1800-1830 sought for the revival of mathematics and logic [2.1-2.3; 6.1-6.3], for general methods and a carefully constructed symbolic language relying on analogy regardless of rigorous demonstrations. By contrast, from the 1830's onwards analysts and logicians focused particularly on explaining first principles, establishing the foundations of the calculi of operations and functions, reacting against the inadequacy of existing textbooks and seeking links and mutual applications between diverse sciences. The line of the development of the new theories is much more intricate and multi-dimensional in the 1830's [Table 3] than in the first three decades, and the network of mutual influences much more subtle and vague. While De Morgan is a striking exception in often providing his readers with very detailed references as to his sources, we still know very little about the sources used by A.J.Ellis, Gaskin, Solly, Greatheed or Murphy, and it is by guesswork that we detect influences based on evident similarities [3.2-3.3;4.2;6.3].

As far as logic is concerned, we notice its first explicit links with the instruction of algebra and geometry in De Morgan [1831;1835a;1839; see 3.4;6.4-6.5], as well as the first attempt for its mathematization by Solly [1839;6.3,(5)-(9)]. We also notice Bentham's, Solly's and Hamilton's independent introductions of the quantification of the predicate, which was to play a subtle role in the shaping of De Morgan's and Boole's works on

logic [6.3,(13),(12);6.6]. On the other hand, we notice the similarly independent inquiries in the foundations of functional and operator algebras by De Morgan [3.5-3.6], Murphy [3.3] and Gregory [4.4] in the late 1830's, inquiries which paid special emphasis on the properties of inverse functions and Servois's laws and which played a most crucial role in the development of symbolical methods and in the rise of algebraic logic in the 1840's [4.5-4.8;6.5-6.7;7.1-7.7; see also 9.3].

The study of Laplace's Mécanique Céleste, a very decisive motive for the British reforms [2.1-2.3], as well as the reactions against Whewell's conservative "liberal education" [3.2, (5),(20)], formed the most basic components of the updating of the mathematical and physical sciences in the 1830's at Cambridge. Airy's Tracts [1826] and Babbage's, Herschel's and Peacock's Examples [1820] gave rise to the updated treatise by Pratt [1836] and Gregory's Examples [1841] -while new works like Murphy's Electricity [1833c] appeared. On similar lines Whately's Elements [1826] formed both a motivation and a partial basis for Bentham [1827], Solly [1839] and Hamilton's own expanded Aristotelian system (63.3). But while, once more, the attempts to revive interest in the study of logic were rather limited in their immediate impact, the educational reforms<sup>instituted</sup> at Cambridge University by people like Peacock, A.J.Ellis, Pratt, O'Brien, Gaskin, Thomson and Hymers, effected a remarkable switch<sup>to pure</sup>

<sup>mathematics from</sup> Airy's and Whewell's emphasis on mixed mathematics.

→ All these people, whether in the status of student, lecturer, moderator, researcher, or textbook writer, prepared the grounds for the work of Gregory, R.L.Ellis, Greatheed, Bronwin and Boole in the late 1830's and early 1840's, while other capable wranglers like Cayley and Sylvester appeared at that time whose pursuit of pure mathematics was to be highly decisive in England later on [3.2,(1)-(4),(20); 4.1-4.3; 5.10; 9.3-9.4].

Very few contributions effected during the transitional period of the 1830's -which links the work of the Analytical Society with the systematic development of D-operator methods and the rise of algebraic logic some years later- gained prominence later in the century. In many ways certain of Herschel's and Babbage's contributions survived longer than Murphy's on non-commutative operations or De Morgan's treatise on functions. For example, Herschel's expansion theorem (23.18) -together with

Hamilton's version of it in 1839 [see (23.30)]- played a most vital role in the development of symbolical methods at mid 19<sup>th</sup> century [(48.1), 4.8, (2)-(3); 5.2; 5.4, (12)], <sup>was</sup> featured in numerous textbooks from Hymers [1879] to Boole [1860]; [see 2.3, (21); 3.9, (2); 4.8, (2)], was remembered in Laurent [1890; 2.5, (1)], to be traced as recently as [Milne-Thomson 1951, 32]. On fairly similar lines, Babbage's transform  $\phi^{-1}f\phi$ , (25.1) -suggested by Maule [2.5, (3); 2.6, (9)]- was a key method in De Morgan's treatise on functions [1836; 3.5, (10); 3.6, (15)], which played a subtle role in the latter's logic [6.9, (13)], appeared in Boole [1860] and, independently of its original context, in Herschel's (28.13), Murphy's (33.51) and Gregory's (44.14) operator calculi, <sup>and is</sup> still prominent in our century [2.5, (1)]<sup>(2)</sup>.

However, as we argued earlier [4.1; 4.6; 4.8], if we did not have Murphy [1837] on non-commutative operations, or Gaskin's reform in 1839, we might had not have Boole [1844] or perhaps his Mathematical analysis of logic [1847a]. Similarly, as shown in [6.7; 6.9], without De Morgan's treatise on functions [1836] -as based on Peacock's Algebra [1830] and Babbage's transform (25.1)- we might had not have his logic of relations in 1860. We would like to stress that behind these three important works lay Murphy's concern for distributive operations in physical context in [1833b], Gaskin's interest in stimulating the students' familiarity with symbolical methods which gained little prominence with Whewell's emphasis on semi-geometrical, semi-analytical methods, and finally De Morgan's desire to amend, expand and systematize Babbage's and Herschel's work on functional equations under his peculiar educational and philosophical prism and the motivation of his review [1835b] of Peacock [1830].

Due to Whewell's obstructive role, the development of Cambridge mathematics and the diffusion of French "analytics" slowed down in the 1820's, to see a gradual revival in the 1830's. Perhaps the lack of prompt attention to the work of Babbage and Herschel at that time was also due to the unfavourable criticism of their work by P. Barlow [2.5, (18); 2.8, (8), (10)]. The development of the calculus of operations by Murphy, apparently on independent lines, revealed a very close connection between their concerns in the mid 1810's and Murphy's in the late 1830's [see 3.3, (6), text below (33.5), (33.62)-(33.67)]. Unfortunately Murphy's abstract work -though surprisingly accepted by Whewell-

gained little attention by his successors. his study of inverse, distributive operations being properly reproduced and acknowledged mainly by Carmichael [1855; see 5.7 and 9.3].

Maybe the development of either pure mathematics or logic might have been more rapid and substantial, or simply different, if certain published works, such as the Memoirs [1813], Solly [1839], De Morgan [1836] or Murphy [1837] had been less neglected by their contemporaries or successors. Moreover, several works left in manuscript form were not published: Babbage's unfinished sequel to his two papers on the calculus of functions [1815; 1816] and his papers on the philosophy of signs [2.7, 2.9, (8)], Herschel's planned book on algebra [2.8, (1)], Bromhead's calculus of factorials [2.8, (7)] and Murphy's never-produced sequels to his Electricity and his paper on the calculus of operations [3.3, (1)]. Among the reasons for this phenomenon are the lack of favourable acceptance or direct impact of their earlier work and the limited economical means for publication<sup>(3)</sup>.

### 9.3 The development of algebraic symbolical methods: 1840-1860; applications in Boole's and De Morgan's logics.

Deductive logic and algebraic methods saw rather slow and independent developments from the 1800's to the mid 1830's [9.2]. As soon as the calculi of functions and operations were more or less rigorously founded in the late 1830's [Table 3], operator (and to some extent functional) methods were rapidly developed and applied in the next two decades in symbolic algebra, differential, finite difference and functional equations, definite integrals, analytic geometry, the calculi of variations, vectors and quaternions, the theory of invariants, and above all logic. Following A.J.Ellis's hints in 1860, [5.10, (7)], we may say that the dual set of the notions of "operation" and "laws of operation" systematized by mid 19<sup>th</sup> century the most diverse branches of analysis, mechanics, geometry and logic into the corresponding "algebras" of the objects under study.

This movement was due to the fact that the pioneers in logic and analysis were no longer exclusively Oxford logicians or Cambridge analysts who worked in isolation. The major logicians, Boole and De Morgan, were not based in either university, and

several new figures appeared from Oxford, London or Dublin Universities who devoted their leisure to the pursuit of algebraic symbolical methods. Mathematicians working as lawyers, composers of music, theologians or linguists (as, for example Hargreave, Greatheed, Bronwin or A.J.Ellis respectively) were all equally attracted by symbolical methods and in particular by their effective application to equations prominent in physical astronomy like the EFE and the LE. Which were the qualities that united all these figures towards this common pursuit?

Many of the intellectuals discussed so far had an ardent infatuation with languages, grammar and poetry and so distinguished and exploited analogies perceived between common languages and that of algebra reviving in a new sense Condillac's dictum<sup>(2)</sup>. They further saw the beauty and symmetry offered by symbolical methods, qualities stressed by De Morgan in 1836 and principally sought after by Bronwin [(52.46), (52.48)], C.Graves [5.5,text, (5)], Carmichael [5.7,(9)], Spottiswoode [5.9,text, (1)] and Sylvester [5.10,text,(9)]. Boole was no exception in this respect, as his pursuit of a "perfect" method in LT reveals [8.6,text below (4)]. Initially, operator methods, as applied to differential equations in the late 1830's and early 1840's, were viewed as an improved alternative to the clumsy method of series [see Greatheed (42.13) or Gaskin,4.2,(8)] as well as a means to present known results in finite, elegant form. Moreover, they afforded rapidity, clarity and conciseness in both methods and results and were soon to be used as a means of invention, thus reviving once more the dictums of semiotic philosophers [1.8;2.9] expressed in De Morgan's treatise [1836;see 3.6,(9)].

Of all these properties, the latter is that which deserves ~~U~~ most attention. On the grounds that symbolical methods enabled mathematicians to see hidden analogies and to effect generalization through abstraction, we have the ingenious invention of new methods, the proof of new theorems, the search for general methods and above all the speculation of the proximity between mathematical and logical concepts and procedures that led to the rise of algebraic logic. None of these concerns were brand new with our mid-19<sup>th</sup>-century pioneers [see 9.2]. However, given Peacock's enlargement of the realms of symbolical algebra and its elaboration by De Morgan, as well as Gregory's establishment of the principles of the method of separation of symbols of opera-

tion from those of quantity, the figures of the 1840's were enriched with well founded principles and issues which their predecessors missed. Algebra formed from the late 1830's onwards a multidimensional<sup>nal</sup> and powerful "science of suggestion" through its quality par excellence of extension. If ~~there is one~~ key notion of this period, distinguishing it from the first decades, then ~~it~~ is definitely that of "extension", substantially differing from that of common and vaguely defined "generalization" (often based on induction) as De Morgan first pointed out in 1835 [3.4, text and (17)-(22)].

Though far from ignorant of the work of Cauchy, Fourier, and Poisson, English and Irish analysts, obsessed with algebraic methods, left very little scope for a radical updating of analysis on the lines followed by French analysts of the 1820's and 1830's as they drew more on Lagrange, Laplace, Arbogast, Servois, Babbage, Herschel and Peacock [1.1, (5); 1.7, (13); 3.1, (2)]. In this respect, this development was no more

a movement for the diffusion of new knowledge [Table 2]. Moreover, since this movement spread in various institutions and <sup>gained</sup> support <sup>from</sup> a wide diversity of figures, it ceased to be any more a reaction towards a conservative curriculum [9.2] but appeared as a one-side obsessional intellectual activity, a fascination with ~~complicated~~ -often meaningless-forms, with universal methods and with a curiosity to investigate and extend the boundaries of symbolic algebra and traditional logic.

It should be noted, however, that educational concerns are not to be underestimated. Many of the figures discussed so far were excellent teachers, and in particular De Morgan and Boole, grasped every opportunity to explain first principles and render their students' grasp of mathematics and logic "more intellectual and less mechanical" [3.5, (4)]<sup>(2)</sup>: witness the series of new textbooks, such as Gregory [1841], De Morgan [1842c], Carmichael [1855], Hymers [1858], De Morgan [1849c] and Boole [1859; 1860], some of which were to survive the century [3.9; 4.8; 5.10; 9.4]. To these we may add the classics LT and FL which not only impressed Boole's and De Morgan's contemporaries, but also influenced the following generations [see 6.4, (5); 6.7, (4) on FL and 7.1, (20); 8.4-8.9; 9.5 on LT].

As the material discussed so far on the theory and applica-

tions of symbolical methods during the period 1840-1860 covers two thirds of the thesis [chaps 4-8], we will restrict our discussion to instances relevant to differential equations and the calculus of functions and their effect on the rise of algebraic logic and the calculus of relations. For convenience we provide the reader with two tables which sum up the principal contributions produced during (1840-1850) and (1850-1860), proceeding next to a survey of Boole's and De Morgan's parallel and independent applications of the method of extension [see 9.1.(3)].

Table 4

Towards the peak of operator methods, symbolic algebra and the rise of algebraic logic: 1840-1850<sup>(3)</sup>.

<u>Main works</u>		
<u>Dates</u>	<u>Boole</u> (predecessors and followers)	<u>De Morgan</u>
1841	Hargreave on LE Boole on COO, COV, linear transformations R.L.Ellis on EFE Gregory's <u>Examples</u>	
1842		On the foundations of algebra; textbook on calculus
1844	Boole on a <u>general method</u>	
1845	>> on developments	
1846	>> on LE; Bronwin on COO	
1847	>> on a symbolical equation >> MAL  >> correspondence with Cayley	S <sub>1</sub> FL Dispute with Hamilton on the quantification of the predicate
1848	>> dispute with Bronwin >> manuscripts on logic >> COL Hargreave on EFE, LE	
1849	C.Graves on COO	On foundations; <u>Trigonometry and double algebra</u>
1850	Hargreave on COO	

Main issues: Generality of methods; extension of known methods and theorems in the calculus of operations and of Aristotle's system; the problem of uninterpretable terms within operator calculus and logic; analogies and differences between algebraic and logical concepts and procedures.

Table 5

The highlights in theory and applications of symbolical methods:  
1850-1860.

<u>Main works</u>		
<u>Dates</u>	<u>COO; logic</u>	<u>De Morgan</u>
1850	Jellett on COV Donkin on COO	S <sub>2</sub>
1851	Carmichael's $\nabla$ -calculus Bronwin on particular operator methods	Mansel's review of FL
1853	Hargreave on COO Carmichael on LE Spottiswoode on $\nabla$ -calculus	Manuscripts on COO
1854	Boole's LT	
1855	Carmichael's treatise Boole on philosophy of logic	
1857	Donkin, Graves and Boole on EFE, LE.	
1858	Hymers's textbook	S <sub>3</sub>
1859	Boole's textbook on differential equations >> >> on finite difference	
1860	A.J.Ellis on operations Greer on $\nabla$ -calculus Russell on non-commutative operations	S, S <sub>4</sub> on LOR "Logic"

Main issues: Assimilation and partial development of Boole's general method; a search for non-artificial and particular methods; extension of theorems in differential operators to ones in partial differential operators admitting of imaginaries; inquiries into the nature and limits of algebraic methods and their interaction with logic [on the abbreviations above see 9.3, (3)].

One of the most fundamental extensions attained was that of Gregory's method for the solution of linear equations with constant coefficients to Boole's general method in [1844]. By this method Boole effected for the first time the integration of binomial equations (46.1) with variable coefficients in a uniform, mechanical way, the solution obtained in finite symbolic form. As we pointed out in 4.6, his invention was motivated by the desire to extend the partial methods of his predecessors to integrate



the EFE, first in its "standard" form (32.11), consequently in its "general form" (42.13) [4.2.(7)] and finally, as one case of a wider class of ordinary differential equations. In the first two cases the trick was to see that the solution of the EFE depended upon that of the equation

$$(93.1) \quad (D^2 + k^2)^{m+1}u = 0, \quad D = d/dx, \quad u = u(x)$$

for  $m=0$  and  $m$  any integer respectively [see (46.28); 3.2; 5.2; (3)]. The third attempt was due to R.L.Ellis who, influenced by Gaskin's method of reduction [4.2], applied the series method in combination with Fourier's symbolical techniques so as to integrate any equation ultimately reduced to the form (43.1)

$$(93.2) \quad D^m u + q^m u = 0,$$

the EFE corresponding to the case of  $m=2$ ,  $k$ ,  $q$  being constants.

Urged by Ellis's ingenious scheme, Boole undertook to invent a method by means of which any equation reducible to the form (46.14)

$$(93.3) \quad D^n v \pm q^n v = X, \quad X = X(x), \quad v = v(x), \quad D = d/dx$$

could readily be solved in finite symbolic form. The basic tools applied to achieve his plan were the formulae (45.19)–(45.20) already known to Murphy and Gregory. However, not only the application of them was new with him, but also the fact that before making use of these formulae he deduced them from a "more general system" (45.12) "with a view to the maintenance of an unbroken analogy" [4.5 text and (8)]. The general theorem of development (45.12) was never mentioned by Boole's followers. However, its numerous consequences were to impress Boole and successors serving as opportunities for new extensions and contemplation of unexpected analogies [see 4.7; 4.5.(8); 5.2; 5.5; 5.10 below (2)].

For the purpose of our present survey we would like to stress the development (47.10)

$$(93.4) \quad f\left(\frac{d}{dx} + \frac{1}{x}\right)u = f\left(\frac{d}{dx}\right)u + \frac{1}{x}f'\left(\frac{d}{dx}\right)u$$

which always terminates at the second term "whatever maybe the form denoted by  $f$ ", and that of (47.14)

$$(93.5) \quad f\left(\frac{d}{dx} + x\right)u$$

It fascinated Boole by being analogous to Taylor's theorem after the first term: "a remarkable circumstance seeing that the

symbols  $x$  and  $d/dx$  are not commutative" [4.7,text and (2)]. For unknown reasons Boole never produced a second part of his paper on development [1845d] as planned [4.7,(1)-(2)].

Symbolical reasoning depends upon the laws of combination of symbols regardless of their meaning. English analysts distinguished

$$(93.6) \quad x(y + z) = xy + xz, \quad xy = yx, \quad x^m x^n = x^{m+n}$$

as the three fundamental laws of algebra, observing that they are also satisfied by the operators  $\Delta$  and  $d/dx$ . They drew on Servois [1814] -a paper unknown to Babbage and Herschel and apparently first acknowledged by Murphy in 1833 [see 3.3 below (8)]. In 1837 Murphy proved rigorously Taylor's theorem in its symbolic form based upon these properties, proving moreover that

$$(93.7) \quad e^{\theta}.e^{\theta'} = e^{\theta+\theta'}$$

for any commutative operations  $\theta, \theta'$  [see (33.22)-(33.25)]. Independently from Murphy, Gregory isolated the laws (93.6) [(44.7)] in order to fix the principles of the calculus of operations "on a firm and secure basis" in 1839 [4.4,text above (44.4)]. Like Murphy, he observed that since the binomial theorem holds for symbols of quantity with the properties (93.6), then it could apply for any symbols possessing these properties; however, he lacked the rigour and detail of Murphy's proofs [4.4,(2)-(3)]. But Gregory hastened to supply what Murphy had missed, that is to apply his results to the symbolical solution of linear differential equations with constant coefficients; according to his remarks, operations such as (44.6)

$$(93.8) \quad \left( \frac{d}{dx} \pm a \right)^n f(x)$$

are valid, and hence the symbolic solutions effected up to his time by Brisson, Français, Cauchy and independently by Herschel [1.6-1.7;2.3;9.2].

De Morgan was the next to distinguish the laws (93.6) as the three "characters of fundamental symbolical relations of algebra" (39.3) in 1840, incorporating next in his [1842c] Gregory's and Ellis's symbolical treatments [3.9,(2);4.3,(3)]. Gregory's Examples [1841] and De Morgan's textbook [1842c] formed two invaluable sources for Boole's paper [1844]. Boole also stressed the laws (93.6) in order to justify the symbolic form of Taylor's theorem (44.2) [4.5 text and (5)], but he soon perceived that

these laws were "the only laws of algebraic symbols devoid from the conception of quantity" (4); in other words, they not only served to extend the realms of algebra so as to embrace the calculus of operations but also to found his calculus of logic too. Thus in MAL [1847a], Boole handled his elective logical symbols as operators obeying the commutative, distributive and index laws  $x^n = x$ , an analogy raised much more distinctly in his letters to Cayley and the manuscript notes [see (4) above] that followed few months later, than in MAL [(73.2)-(73.11); 7.3, (2)-(7); 8.2; 8.3, (2)].

The fundamental problem in Boole's logic was to express a given problem in the form of an elective equation, to solve this equation and finally to interpret the final result in logical terms [7.5, (4); (84.11)]. Viewing his elective symbols in analogy with operators, he was aided in establishing his general method in logic. Based on his hints in MAL and letters to Cayley, we illustrated the close analogy between the solution of elective equations, such as

$$(93.9) \quad (1-x)y = 0 \longrightarrow y = vx,$$

and the corresponding differential equations

$$(93.10) \quad \left(1 - \frac{d}{dx}\right)y = 0 \longrightarrow y = \left(1 - \frac{d}{dx}\right)^{-1}0 \longrightarrow y = ce^x$$

[(75.2)-(75.7)]. Further, Boole exploited the former analogy by observing that "elective symbols combine according to the laws of quantity" and hence an elective function  $\varphi(x)$  could be expanded by Maclaurin's theorem, the result obtained being

$$(93.11) \quad \varphi(x) = \varphi(0) + [\varphi(1) - \varphi(0)]x = \varphi(1)x + \varphi(0)(1-x).$$

Boole compared (93.11) with (93.4) and was happy to note the similarity that both expansions stop at the second term [(77.5)-(77.11), 7.7, (5)-(6)].

Based upon the theorem (93.11) Boole built the general method for the solution and interpretation of elective equations which consisted principally of the two general theorems (77.20), (77.29). When the problem of the admittance of uninterpretable terms was put forward by Cayley, Boole hastened to explain to him that it was <sup>a</sup>legitimate as in the procedures of symbolical algebra, which, in fact, had furnished the grounds for his general method (5). The analogy (93.9)-(93.10) was not to form part of this method, and the fundamental theorem of expansion

(93.1) was to be proved in LT by simple and purely logical means<sup>(6)</sup>. The fact is, nevertheless, that the core of Boole's early logical scheme in MAL -which formed the raw model of LT- did depend upon the principles of symbolic algebra, borrowing methodological procedures of the calculus of operations [see 7.1-7.3;7.5;7.7].

Symbolic algebra served as a science of suggestion in logic for both Boole and De Morgan in the late 1840's. Boole drew directly on its laws and procedures, showing "that the forms and transformations of algebra can be fitted to meanings of the symbols which will make them express the forms and transformations of thought" [Neil 1865,91]. In so doing Boole relied upon the operational character of the symbols which obey the laws (93.6); preserving the form, he attributed non-quantitative matter, viewing the numerical constants that appeared in the intermediate steps as universal symbols "not solely confined to Arithmetic": "they are not numbers, but signs used in subjection to the laws of thought as manifested in language" (8.2,(11)). He further observed that "the language of analysis in general is [...] independent in its forms of the particular nature of the operation represented by the operating symbol" [8.2,(12)].

Thus Boole drew in fact on the form-matter issue with its origins in Arbogast's method of separation of symbols [1.6;1.8] extending the ability of symbolic language, as applied to mathematical symbols of operation, to symbols that represent operations of thought, totally altering the style of traditional logic which, beside viewing it as "incomplete", he regarded as "not sufficiently fundamental to serve as the foundation upon which a perfect system may rest" [LT,241-2; end of 8.6; see also 9.5]. On the other hand, De Morgan's system in FL is constructed upon the basis of syllogistic logic but "is widened in all directions by those suggestions regarding extension which operate habitually in a mathematician's mind, and is distinguished by the use of a symbolic language, which, though not mathematical, could never have been invented but by a mathematician" [Neil 1865,91].

Indeed, as De Morgan himself observed when, like Neil, he compared FL with MAL in  $S_2$ , his system is far from distinctly mathematical as Boole's was [6.7,text and (3),(13);6.9 text below (20)]. Nevertheless, in order to extend the forms of traditional logic he also made use of the form-matter issue, its origins

lying in Carnot's principle of the indeterminateness of algebraic forms [1.8;2.9] which he had applied in [1836] in order to show in steps how arithmetic was extended to algebra and the latter ascended to the calculus of functions and next to that of operations, matter excluded from form in each stage [3.6]. Just as Boole had viewed either the laws (93.6) or the process of repetition of unit [8.2,(12)] as non-peculiar to arithmetical entities, De Morgan saw the method of generalization via abstraction as a process of the mind independent of the matter effected upon (see 6.6,(9);6.5,(4);6.7). Thus in FL De Morgan enlarged the realms of traditional logic by rendering the copula "is" both abstract and general in its new form of "has relation to", the whole process consequently analysed in detail in  $S_2$  and  $S_3$  [6.5;(67.2);6.9].

Boole and De Morgan corresponded with each other both before and after the publication of their first major works, but they did not draw on each other's work [7.1,(8)]. Still, despite the slight connexion perceived between their systems, their deeper methodological concerns converged in the sense that they both acknowledged the rules and laws of symbolic algebra as tools for extension beyond <sup>normal</sup> mathematical structures. Moreover, they both speculated on the similarities between logical and mathematical concepts and procedures<sup>(7)</sup> and both attempted to produce logic devoid of excessive mathematical symbolism<sup>(8)</sup>. Fallacies and inaccuracies ~~were evident~~ in both systems and their philosophy was messy and unclear<sup>(9)</sup>, but our concerns have focused principally on the germs of their works and their consequent study of the correlation between mathematics and logic.

Around 1850 we notice the climax of symbolical methods best revealed in Hargreave [1850], Donkin [1850] and De Morgan [1849c;1850];[see 5.4;5.5;3.9 and 6.7 respectively]. Bronwin and Carmichael refuted on different grounds the possibility to attain a general, complete and genuine method in analysis in the early 1850's [5.10], while De Morgan, <sup>at</sup> a slower pace, delved into the theory of the calculus of operations in his manuscripts [3.9,(6),(13)]. Despite the fact that Boole's general method had few followers in the 1850's, the calculus of operations continued to grow with interesting results in the field of partial differential equations, the calculus of variations, analytic geometry and invariant theory [5.7-5.10;9.4].

Functional equations, on the other hand, were hardly at the forefront of research. After De Morgan [1836] they appeared only in his papers on the foundations of algebra [(39.9)-(39.14);3.9,(9)], and after Babbage they were symbolically treated by Gregory with some references in prominent textbooks [4.4;4.8]. However, foundational issues in De Morgan's calculus of functions had a subtle influence on his gradual development of the logic of relations, ultimately published in 1860. The very idea of combination of relations seems to be borrowed from composition of functions [6.9,(3)] while inverse relations were shown to have a role closely similar to invertible inverse functions [6.9,(14)]. A direct influence from the calculus of operations is not noticed in the construction of his enlarged system, though it is quite probable that he drew on it in the notation  $X..LY$ , as well as in the notion of quantified relation in  $X...LM'Y$  [see (69.12)-(69.14);6.9 below (10)]. Above all, what underlined De Morgan's logic of relations was his application of the form-matter issue; as he claimed in  $S_4$ , the transition from  $X...LY$  to  $Y..L^{-1}X$  "is a form of thought" and all his syllogistic inferences, such as (69.5) were formal [see 6.9 text and (19)].

#### 9.4 The impact and utility of symbolical methods in late-19<sup>th</sup>-century mathematics.

The status of operator methods soon after Boole's death is summarized in Spottiswoode's Report in 1865;

The calculus of operations, or of symbols, as it has been also called, whereby the symbols of operation are separated from those of quantity, has for some years been in use among analysts in this country. And although no very remarkable step has recently been made, or is perhaps to be expected, in this field, still some considerable progress has been effected towards completing the algebra, or laws of combination, of these non-commutative symbols<sup>(1)</sup>.

Spottiswoode also alluded to Jacobi's and Boole's "important contributions to the theory of differential equations, and in particular of those which occur in mechanics", claiming that in the deaths of these two analysts "mathematical science has sustained so great losses" [1865.5].

Operator methods of solving differential equations, studied so much in the 1840's and 1850's, were no more at the forefront of research in the second half of the century. The attention of the leading English analysts was now monopolized by the "new algebra", particularly of the theory of rational and integral functions which formed part of Cayley's calculus of "quantics" and Sylvester's calculus of "forms"<sup>(2)</sup>. Thus, as Boole's most eminent successors focused on invariants, matrices, ordinary equations and other related topics, exploring new aspects of algebra, there remained hardly any scope for any "remarkable step" to be made in the field of the calculus of operations seen as an autonomous field of pure mathematics.

This is briefly the general scene of the situation at the 1860's, as more or less discussed in 5.10. With Boole's death many aspects of his methodology and philosophy in employing a symbolical language faded away and gradually disappeared. Still, his memory remained alive till the end of the century. This section saves from oblivion some interesting instances which reveal an influence of Boole's work and concerns, and also seeks the reasons for the rather limited scope of this influence. We start by stressing the main links and similarities between Boole's, Cayley's and Sylvester's algebraic investigations. Next we explore traces of Boole's impact in reports, textbooks and research papers contributed from the 1860's till the 1880's, concluding with the last traces of his work as incorporated by his last semi-followers, Glaisher and Forsyth.

Cayley's and Sylvester's algebraic theories resembled, and even surpassed Boole's own, in being general and abstract; in fact "so general, so abstract, and so apparently removed from any practical applications [that they] could not fail to be regarded coldly by many whose attention had been principally directed to special problems in physics" [Spottiswoode 1865, 4-5]. Thus, this new trend in algebra was viewed by the traditional mathematicians of the 1860's in the same way as Whewell had regarded Babbage's and Herschel's analytics as useless four decades earlier [3.2, text and (4)], or as O'Brien and Carmichael had rejected Gregory's excessive symbolism and Boole's artificial techniques respectively in the 1840's [5.10, (3)-(4)] and 1850's [5.7, (8)]. But contrary to Babbage, Herschel and Boole [9.2-9.3], the new algebraists were not so ambitious to construct a method of

universal application in mathematics. Equally obsessed with their calculi of forms as <sup>were</sup> the former with functional and operator algebras, they sought unity of method via morphology, a term introduced by Sylvester and which, like a "comparative science", linked culture and science via analogies in form [5.10.(7),(9)].

Talking of "form"-as connected with the operational character attributed to this concept by Boole and De Morgan- in opposition to that of "matter"<sup>(3)</sup>, we would like to refer to Sylvester's interesting definition of it under a joint influence of its common mathematical use and of Darwin's morphology. In his paper on "syzygetic relations" [1853] we read in the vocabulary appended in the end: "Form.-Any function may be regarded as an opus operatum; the matter operated upon being the variables, and the substance of the operations being the form, which resides in the function as the soul in the body. A form is always common to an infinity of functions, but for greater brevity may be and frequently is called by the name of some specified function in which it is contained"<sup>(4)</sup>.

In this respect Sylvester's "Calculus of forms" encompassed a wide range of algebraic forms. Nevertheless, this calculus came to be mainly linked with a study of "invariants" and "covariants" on the lines engraved by Boole in his paper on linear transformations in the early 1840's [see (2) above]. As we saw in 5.10, both Sylvester and Cayley drew on Boole's early applications of partial differential operators. The study of non-commutative operators in general played a most prominent role in their work, and symbolic formulae already introduced by Murphy [1837] were reinvented in the 1860's [see (510.8)]. Moreover, Sylvester was to see beyond the mechanical utility of operators and to study them as a linguistic component linking them, like Boole, with grammar and logic. But as we saw in 5.10, his rather eccentric comments were only incorporated in footnotes and were apparently of no impact<sup>(5)</sup>.

Closer to Boole's spirit in applications was Spottiswoode who combined the calculus of operations with his theory of determinants in his [1853], paying, however, more attention to symmetry of forms than to the efficient solution of specific differential equations [5.9.(1)-(4)]. Moreover, after Boole's most ardent defender Russell, Spottiswoode was an exception in the early 1860's to contribute "towards completing the algebra, or



laws of combination of these non-commutative symbols" on Boolean lines [see (1)above].

How abstract and "removed from any practical applications" were pure mathematics? Incorporating under the latter's realms the calculi of quaternions, forms and operations, Spottiswoode claimed in his Report (1878):

That the Pure has outstripped the Applied is largely true; but that the former is on that account useless is far from true. Its utility often crops up at unexpected points; witness the aids to classification of physical quantities, furnished by the ideas (of Scalar and Vector) involved in the Calculus of Quaternions;[...]. The utility of such researches can in no case be discounted, or even imagined beforehand; who, for instance, would have supposed that the Calculus of Forms [ ...] would have thrown much light upon ordinary equations; [ ...] or that the Calculus of Operations would have helped us in any way towards the figure of the Earth<sup>(6)</sup>?

Since Kelland's personal acknowledgments to Gaskin, R.L.Ellis and Boole in 1858 for their solution of the EFE in finite form [5.10,(1)], Spottiswoode's general acknowledgment is a rare case among such statements of late-19<sup>th</sup>-century mathematicians on the value of Boolean operator solutions of the EFE or LE. For as we shall see below, these solutions were either ignored or regarded as non practical or unclear in form. For mid-19<sup>th</sup>-century analysts symbolical methods were an indispensable tool in the subject of differential equations. Let us examine the fruits of this obsessional study starting with Boole's views on these methods in his well-known textbook [1859].

It was after casting logic in algebraic form in 1847, that Boole started contemplating on the nature of symbolical methods motivated by Bronwin and Cayley on problems of fallacies and interpretation, as well as urged by his deeper belief that symbolic methods are not peculiar to mathematics. After publishing his LT, he attempted to compose a work on logic devoid of mathematical elements. He left his work in manuscript form, revealing his most mature views on the nature of symbolical methods which appeared together with his educational and philosophical concerns in his [1859]<sup>(7)</sup>. Cautious in promoting mechanical and artificial operator techniques, Boole stressed in his textbook that with few exceptions, on the whole these symbolical techniques are

far from indispensable; their chief value—other than providing condens<sup>ed</sup>ness and simplicity—was their property of revealing connections of language and thought [8.8,(2),(4)]. From this point of view the limited impact of symbolical methods (particularly in textbooks) partly reflected Boole's own rather reserved presentation in his [1859], reserved when seen in comparison with his unlimited enthusiasm in founding a general method in analysis in the mid 1840's (4.6,(10);5.2,(12);5.10).

Boole shared his predecessors' and contemporaries' admiration of the properties of the calculus of operations to offer rapidity, elegance, condensness, uniformity and symmetry of procedures and results. However, while he differed little from them in the actual employment of the calculus of symbols in mathematics, he was unique in exploring its metaphysical and linguistic aspects. For example, in no other treatise before or after Boole [1859] do we come across passages such as this one  
 from his discussion of  $f(d/dx)$ ;

In any system in which thought is expressed by symbols, the laws of combination of the symbols are determined from the study of the corresponding operations in thought [...]. Now all special instances point to the conclusion that [...] the mere processes of symbolical reasoning are independent of the conditions of their interpretation, [...]. We take occasion to notice that, whatever view may be taken of this principle, whether it be contemplated as belonging to the realm of a priori truth, or whether it be regarded as a generalization from experience, it would be an error to regard it as in any peculiar sense a mathematical principle. It claims a place among the general relations of Thought and Language<sup>(8)</sup>.

Boole's textbook was far from unknown to the textbook writers that succeeded him. However, no one seems to have paid attention to its underlying epistemology, and what comes as a big surprise is that writers who referred to it in their work totally ignored important aspects of his application of operator methods, such as his finite solution of the EFE and the LE. Let us have a brief look at Airy's Elementary treatise on partial differential equations [1866] and Earnshaw's Partial differential equations. An essay towards an entirely new method of integrating them [1871] which are the only textbooks to employ symbolical methods after Boole's death and before his major successor Forsyth

[1885].

Airy claimed in the "Preface" that "No attempt has been made to go into all the varied details, of methods and examples, which present themselves in the wide field of Partial Differential Equations considered purely as an Algebraical subject". However, in few equations, such as in the case of

$$(94.1) \quad \frac{d^2z}{dx^2} - a^2 \frac{d^2z}{dy^2} = a(x,y),$$

he employed both the standard method for its solutions and, consequently, that of separation of symbols, saying that "This [latter] theory is, in fact, merely a convenient form for exhibiting the indubitable results of legitimate algebra; but it sometimes serves to suggest new methods of treating equations which, when verified, are useful" [1866,25-6].

Recalling Whewell's influence on Airy towards "mixed mathematics" [3.2 text and (4)-(6),(15)], his statement above is a bold one. Nevertheless this was not a revolutionary switch with Whewell's death in 1866, for from the following statement it becomes evident that, for all Murphy's, Gregory's and Boole's attempts [3.3;4.4;4.5] to establish the calculus of operations upon rigorous foundations, Airy did not trust the legitimacy of the principle of separation;

This principle, as a purely algebraical and symbolical process, possesses very great power, and leads to very remarkable results. But the reader cannot fail to observe that it carries with it no evidence whatever for the validity of results, (such as is conveyed by the operations of quantitative algebra or by the steps [properly pursued] of the differential calculus), for which it must rely on subsequent verifications. As aiding the application of Partial Differential Equations to physical investigations it possesses little value. The further examination of it would therefore be out of place in this Treatise. The student who desires to follow it up will find much information in Boole's Treatise on Differential Equations, Gregory's Examples, and similar works<sup>(9)</sup>.

This statement comes as a surprise given Airy's apparent knowledge of Gregory [1841] and Boole [1859]. However, it seems that he did not study these works in depth, for after offering a small sample of simple equations like (94.1) solved symbolically,

he claimed:

We believe that scarcely any other equations than these, occurring in physical investigations, have been solved in a finite form. Several equations have been solved in the unsatisfactory form of infinite series, of which the convergence is not always assured; but it is not the object of the present Treatise to enter on them<sup>(10)</sup>.

We may excuse Airy for his incomplete knowledge of all the solutions of the LE (58.1), (58.12) -given by Boole and Donkin and incorporated in Boole [1859]- on the grounds that his treatise was "Elementary"; but we certainly cannot accept Earnshaw's ignorance when he wrote in the "Preface" to his [1871]:

Many equations will be found in this Essay integrated with ease in finite terms, which, as far as the Author is aware, were never integrated in finite terms before. In the last chapter, for instance, the following highly important equations will be found integrated for the first time in finite terms:

$$\begin{aligned} [(94.2)] \quad & \frac{d^2u}{dx dy} + cu = 0 \\ & \frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + cu = 0 \quad \text{and} \\ [(94.3)] \quad & \frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2} = 0^{(11)}. \end{aligned}$$

Earnshaw referred in the "Preface" to the "admirable and valuable" works of Boole, Gregory and Carmichael with which he had been "careful to interfere as little as possible" [1871,v-vil]. "In fact", he went on, "[the author] has consulted them only for such illustrative examples as fell within the scope of his Essay". Earnshaw claimed to have put forward a new symbolic method of solving partial differential equations by suitably reducing them to ordinary ones. Nevertheless, his methods hardly differed from those of his predecessors. And what is even more surprising is that he avoided to refer to their results. Omitting his solution of the LE (94.3) [see (11) above] we will provide as an example of his procedure his solution of the EFE (56.6).

In chapter 8 of his treatise Earnshaw reproduced Boole's method for the solution of binomial equations in a slightly different form, based upon the theorem (45.20) known since Murphy [1871,66-70;see 4.5-4.6]. By means of his theory he integrated the equation

$$(94.4) \quad d_t^2 u + cu = \frac{A(A-1)}{t^2} u \quad ; \quad d_t u = \frac{du}{dt} ,$$

by letting  $t=e^\theta$  and reducing it to the symbolic form

$$(94.5) \quad (d_\theta - A)(d_\theta + A - 1)u + ct^2 u = 0.$$

Next by the well-known method of successive reductions [4.2-4.3; 5.2;5.6] he arrived at the solution

$$(94.6) \quad u = t^A (t^{-1} d_t)^A w,$$

where  $w$  the integral of  $d_t^2 w + cw = 0$  or (52.7) [1871,91-2].

Earnshaw's method is very close to Williamson's assimilation of Boole's method for solving the EFE (56.14), and the result (94.6) is a slight alteration of Gaskin's solution (42.28) or (42.34). However, he mentioned none of his numerous predecessors on the EFE, nor did he even name the equation. Earnshaw's treatise, the only textbook to refer to Carmichael [1855] and to be devoted like the latter solely to symbolical methods, seems to have had very limited impact. We have traced it so far only in Glaisher's work [1872,137;1881,813]. Glaisher stressed that though solutions of the EFE of the form (56.15) or (42.34) have been given "more than once by different mathematicians", the "slightly modified form" (94.6) "seems scarcely to have been noticed" [1881,813].

With very few exceptions, most of the new contributions in the field of ordinary and partial differential equations focused on solutions provided in series form. In many ways solutions in series form were to be preferable in practice to finite symbolic forms and this seems to be the most basic reason for the rare appearance of the latter as the time passed by. Still Boole [1859], as well as the work of his predecessors and contemporaries, were not forgotten. Under the influence of the work carried out on second order ordinary differential equations in the 1840's - in papers with the common title "On certain differential equations" which focused on and around the EFE<sup>(12)</sup> - Cayley wrote a "Note on the integration of certain differential equations by series" [1869]. The paper opened with the following scholium:

There is a speciality in the integration of certain differential equations by series, which (though evidently quite familiar to those who have written on the subject—Ellis, Boole, Hargreave) has not, it appears to me, been exhibited in the clearest form<sup>(13)</sup>.

The first of his examples was but a transformed form of the EFE very close to Bronwin's form (43.27) [Cayley 1869,77-78]. In another paper "On Riccati's equation" [1868], Cayley applied the series method in order to integrate the Riccati equation in the form (14.4). His method seems to be very straight forward, and the partial results are mentioned as "readily identified" with Boole's solutions in [1859,98], where a more tedious method of a series of transformations were employed [Cayley 1868,351;see (14) below].

The Riccati equation was next to the EFE in the attention of our leading figures such as Murphy [3.3,(4)], De Morgan [4.3,(4)], Hargreave [5.3,(11)] and Boole<sup>(14)</sup>. In fact De Morgan was the first English mathematician to point out the close connection between the RE and the EFE in [1842c;see (14.3);4.3,(4)]. Based on all the work done on these two equations by his French and English predecessors, Glaisher contributed two interesting papers on the RE [1871;1872], producing the lengthy [1881], all papers repeatedly cited in our thesis [see 1.4,(4);4.2,(11);4.5,(9);4.8;5.3;5.6]. A most ardent and prolific defender of pure mathematics beside Cayley [4.2,(10)], Glaisher was as obsessed as Boole with the subject of differential equations. We may say that he was an exceptional case lying at the borderline between Cayley's strict preference for particular series solutions and Boole's for general symbolic ones. We may also add that he shared De Morgan's teaching and historical virtues, marking with his overall contributions an exceptional era in Cambridge University at the end of the century.

Glaisher showed a particular inclination towards definite integral solutions, and in his [1872,133-6] he did apply Boole's symbolical method in order to solve the RE as a transformation of the EFE [see 4.5,(9)]. His passionate inquiries in the RE since his graduation as second wrangler in 1871 culminated in his [1881], "his most significant paper" in the field of differential equations [Forsyth 1930,ix]. About half of the paper was devoted to particular series solutions of the RE and of other closely

allied equations [1881,759-782,819-22] and the rest to the numerous symbolic solutions of the EFE by Gaskin, Ellis, Boole and followers together with his own definite integral forms. In so doing he relied upon Cayley's work [1881,763,803,823], providing a most extensive bibliography of French and British analysts on the subject, and he did not fail to prove what his predecessors had not considered, the equivalence between the different solutions.

But the most vivid trace of Boole's influence was in Russell's English Cyclopedia article on the "Calculus of symbols" [1873], now forgotten like Glaisher's memoir. Opening with a brief historical survey on the calculus of operations -including Lagrange's theorem (15.3), Servois's laws and Gregory's work, the article focused on the development of Boole's algebra of non-commutative symbols by Russell and Spottiswoode. Saving from oblivion Boole's forgotten papers [1845d;1847c], upon which Russell had improved in the 1860's, it reproduced Russell's investigations on the multiplication and division between polynomials in  $n,p$ , operators combining according to the law (59.23) [see 5.9 text and (7),(8)].

Russell was the last of Boole's disciples as apparently his advanced attempts towards "completing the algebra" of non-commutative symbols [(1) above] remained without any impact. Russell's article paid attention to Boole's treatment of the "interesting rather than amiable group" of symbolic equations (58.17) [see 5.8,(11)], one case of which is the LE (58.1). This investigation had opened "a very curious chapter in symbolical algebra" in Boole [1847c;5.8,(11)] with consequences on Bronwin [5.4] and C.Graves, who together with Boole explored further the "boundaries of algebra" incorporating quaternions in the late 1850's [5.8,(18),(19),(20)]. This "curious chapter" closed for ever with Russell [1873]. But what about the fruits of it in applications?

As we saw in [(6) above] Spottiswoode acknowledged the practical utility of the calculus of operations in 1878 by reminding British mathematicians of the efficient application it had found in the theory of the figure of the earth. But looking at both the EFE and LE within their actual physical context, were their numerous symbolic solutions of any real utility or was the success of the calculus of operations limited to "pure"

mathematics? Airy was the first to express clearly the position in 1866 that separation of symbols "possesses little value" in physical investigations [see (9) above]. His views, partly due to lack of knowledge and partly to Whewell's influence, were nevertheless independently and indirectly justified by Cayley's emphasis on series solutions, as we saw above. But the failure of the calculus of operations in Boolean lines to meet the needs of physical astronomy is best revealed in Pratt [1871] and Todhunter [1875], the most representative treatises on the theory of attractions, the figure of the earth and Laplace's functions in the second half of the century.

In Pratt [1871, 114-5] we see the EFE solved by O'Brien's transform (32.23) (introduced in 1840) in the form (32.12) which was given in Airy [1826] and demonstrated by A.J. Ellis in 1836. As for the LE (13.6) we find Poisson's treatment along Laplace's lines with the following addition in a footnote:

For the direct integration of this equation, see two Papers in the Philosophical Transactions for 1841 and 1857, by Mr Hargreave and Professor Donkin respectively<sup>(15)</sup>.

A more detailed treatment of the theory of Laplace's functions on Murphy's lines was carried out by Todhunter, followed by a similar note as in [(15) above], including Boole [1859], concluding:

The result though very interesting theoretically has not hitherto been used in practical applications<sup>(16)</sup>.

Our conclusions on the "little value" of symbolical solutions are further proved by Forsyth's treatise on differential equations [1885], where it was particular solutions in series forms which were mainly sought after for equations such as the LE, the Legendre equation, Bessel's equation and others. Forsyth, senior wrangler in 1881, was well acquainted with Gregory [1841], De Morgan [1842c] and Boole [1859], which he recommended in his "Preface" together with the Todhunter edition [1865] of the latter's supplement [5.10.(2); 4.6, 10; 7.1.(25)]. In the 3<sup>rd</sup> chapter of his treatise he incorporated an improved version of Gregory's solution of linear differential equations with constant coefficients, and after Glaisher [1881; 4.2.(11)], he devoted a brief section on "symbolical solution" where he saved from



oblivion the contributions of Gaskin, R.L.Ellis, Boole and others on the EFE [4.6,(11)].

But what about Boole's general method? Compared with Airy and Earnshaw, Forsyth was much more well read on symbolical methods, but the fact that he restricted his account to Gregory's method with a passing tribute to Gaskin, Boole and others only on the EFE, shows that he passed over the core of Boole's textbook on operator methods and the LE. Forsyth was Glaisher's student, and it is evident from his textbook that his knowledge on the symbolic treatment of the EFE was principally derived from Glaisher [1881], as we saw in [4.6,(11)]. Besides, in the passage that follows from the excellent biography he wrote of his teacher, it becomes evident that Forsyth did not highly respect Boole's general method; praising Glaisher's "elaborate memoir" [1881] on the RE, he wrote in 1930:

The subject of differential equations, mainly ordinary equations and the integration particularly of ordinary linear equations in series, absorbed much of his activity in his most fruitful years. Down to the time of his earlier investigations, English progress in the subject had centred in formulae: "elegant" symbolic solutions had been accumulated by the diligent ingenuity of Cambridge mathematicians such as Gaskin and Leslie Ellis; and the method, so far as it was a method, had been systematised by Boole in his well-known volume<sup>(17)</sup>.

Forsyth's historical survey reflects what most late-19<sup>th</sup>-century and early-20<sup>th</sup>-century mathematicians would have said on Boole's general method had they been acquainted with the Cambridge movement of the late 1830's and early 1840's. Despite his negative comment, "so far as it was a method", at least Forsyth paid a tribute to the early pioneers of symbolical methods. The writers to follow focused solely on linear differential equations with constant coefficients with no tribute to Gregory and hardly any to Boole [see 4.1,(2);9.1,(2);9.8]. After Glaisher [1872] and Russel [1873], Boole's general method was not to feature again up to our days in connection with differential equations with variable coefficients. An interesting exception concerns Boole's application of that method to finite difference equations, acknowledged and improved in Milne-Thomson [1951]<sup>(18)</sup>.

## 9.5 The impact of Boole's Laws of thought: 1859-1881; Jevons's reaction and Venn's defence.

Just as Boole [1844] stimulated numerous variations upon his general method in analysis with happy consequences in the development and applications of symbolical methods within mathematics [9.3-9.4], his LT [1854] formed the grounds for a further study of algebraic logic and the development of abstract algebra. In both cases there were ardent disciples, fierce opponents and interesting commentators, and what is striking is the fact that though Boole's impact on the whole was eventually considerable, it was the peculiar features of his reasoning which were attacked or escaped the attention of his main successors<sup>(1)</sup>. The general method in logic, introduced in the last two chapters of MAL [7.7] and forming the core of LT[8.4-8.6], was not to survive any longer than did his general method in analysis. Moreover, its impact was not as immediate and extended as in the case of Boole's operator method. It is also worth noting that it was the LT rather than MAL to be taken over by late-19<sup>th</sup>-century logicians. The two works differ in many ways and certain merits of MAL over LT have been pointed out<sup>(2)</sup>. According to [Kneale 1948,173] "Towards the end of his life Boole said that he was dissatisfied with the exposition and arrangement of his Laws of Thought, and that he wished he had spent twice as long in working out the ideas first presented in his Mathematical Analysis of logic".

We can not conceive how far the Boolean impact on logic would have been different had Boole had lived beyond 1864. In this section we focus on the immediate reactions to his logic. First we comment upon Boole's impact on those close to him, such as De Morgan, M.Boole, Harley and R.L.Ellis, including instances from the writings of Gregg, A.J.Ellis and T.Young. Next we proceed to Jevons's criticism in 1863, concluding with Venn's defence in the late 1870's and early 1880's. We postpone to 9.6 a brief commentary on English, American and German logicians who worked under a joint influence of Boole and De Morgan in the last three decades of the 19<sup>th</sup> century. Our account will not follow a strict chronological order.

De Morgan was among the first to recognize the importance of Boole's work, defending it against Mansel's criticism in 1860 [6.7,(13); 6.9 text below (20)]. The two logicians had been in

close contact for many years and their work was shown to be underlined by certain common issues [9.3, text below (6)]. However, De Morgan did not follow Boole's work -there is evidence that he hardly read it properly [Corcoran 1986,69-70;6.9,text above (5)]- and in this sense he can not be called a follower. But he was able to perceive inaccuracies in Boole's syllogistic theory in MAL [see 7.6], and in the last years of their correspondence they communicated upon fundamental concepts of traditional logic, such as that of "universe" (which Boole had borrowed from him) and Hamilton's ambiguous "some" [Smith 1982a,101-4;6.6,(3)].

Mary Boole took over the educational and metaphysical import of her husband's logic. She called equation (88.8)

$$(95.1) \quad x + (1-x) = 1$$

the "Mystical equation" [8.8,(5)], and in her educational writings she tried to diffuse and explain Boole's philosophical and religious connotations, stressing particularly his obsession with singular solutions and the proximity of Boole's epistemology with Gratry's [7.3,(4);8.8,(6);8.9,(2)]. She was a rather eccentric advocate, interested in deploying Boole's doctrines for pedagogical reasons; for example, in her book Mathematical psychology in Gratry and Boole [1879] she tried to analyse Gratry's and Boole's unifying procedures and prove their utility in mental health [8.9, text below (10)]. Despite her naive tone, she delved deeply into Boole's metaphysics, and in fact better than any of his followers who set aside the last chapter of LT focusing on its mathematical aspects [8.8,(7);8.9]. For all her exaggerations, she is now considered as a reliable commentator, her work having recently attracted historians' interest [7.1,(4)].

The connection between religion and the genesis and development of Boole's logic has been touched upon only tangentially [7.1,(4);8.9,(3)]. Nevertheless, we would like to report a very peculiar application of Boolean logic to theology by the Dublin clergyman T.D.Gregg. Gregg suggested in 1859 an application of the principles of differential calculus to metaphysics "with a view to the attainment of demonstration and certainty in moral, political and ecclesiastic affairs". Apparently acquainted with both FL and LT, he drew on Boole's duality doctrine, formulating symbolic forms such as

$$(95.2) \quad ASX,$$

where A the apostolic element, S the doctrinal element and X the

body of the faithful, the whole form (95.2) denoting the church. Denoting further by "cex" the "general expression for an individual", he regarded his symbols as variables and expressed "the increase of David's educational excellence" in becoming from a shepherd boy, a king, by the formula

$$(95.3) \quad \frac{du}{de} = ce \frac{du}{de} + ex \frac{dc}{de} x + cx.$$

These instances are all drawn from De Morgan's amusing annotated account in his book A budget of paradoxes [1872,297-300], referred to en passant in Kneale [1948,172].

De Morgan's witty and ironic comments upon Gregg's work are enough to prove that the latter's attempts to mathematize theology on Boolean lines was wholly unsuccessful and invalid, and thus not worthy of any serious attention. However, Gregg's consideration of his symbols as variables calls attention to three other independent and widely individual applications of the issue of variation in logic, metaphysics and chemistry by Solly [1839], Gratry [1855] and Brodie [1866] respectively<sup>(3)</sup>.

Soon after Boole's death two ample biographies were published by Neil [1865] and Harley [1866a]. Both men tried to explain the principles governing Boole's logic defending it against charges of mathematization [7.1,(1),(13),(16)]. R.Harley, an intimate friend of Boole, devoted three reports [1866b; 1870; 1881] to a commentary of MAL and LT, pointing out the effects of Boole's logic on British and American scientists of that time. We would like to record an interesting instance from his [1870] which reveals Boole's influence on Tait's expansion of Hamilton's theory of quaternions: citing Tait's book on quaternions Harley stressed the following analogy:

It is curious to compare the properties of these quaternion symbols with the Elective symbols of Logic, as given in Boole's wonderful treatise on the "Laws of Thought", and to think that the same grand science of mathematical analysis, by processes remarkably similar to each other, reveals to us truths in the science of position far beyond the power of the geometer, and truths of deductive reasoning to which unaided thought could never have led the logician<sup>(4)</sup>.

Another interesting instance of Boolean influence recorded by Harley concerns R.L.Ellis's vague suggestions for a logic of

relations independently of De Morgan. Ellis's ideas were incorporated in his brief "Notes on Boole's laws of thought" published in 1863. Impressed by chapter 3 of LT, Ellis wrote: "No doubt everything stands in relation to something else, as the species to its genus, and consequently the symbolical language proposed is in extent perfectly general, that is, it may be applied to all the objects in the universe" [1863,391]. Ellis proceeded to formulate "inversio relationis" symbolically as follows: Let S be Shem, N Noah, f father, s son. Then

$$(95.4) \quad N = fS, \quad sf = 1.$$

Eliminating f between these two equations, we get

$$(95.5) \quad S = sN$$

[1863,392]. Harley's commentary upon Ellis's innovations is worth recording:

The author [Ellis] gave his reasons for believing that when [....] a calculus is devised for [the] symbolical solution [of inverse relations], it will be found that the processes involved in such a calculus formally coincide with the processes commonly employed in the solution of functional equations. He also pointed out that it was in this direction probably that Boole's method would be found to admit of extension—an extension analogous to that which Boole himself effected for the theory of the solution of differential equations by the invention of an algebra of non-commutative symbols<sup>(5)</sup>.

Replacing Ellis's symbols S, N, f and s by the more appropriate  $y, x, \phi$  and  $\phi^{-1}$  respectively, Harley formulated the implication (95.4)-(95.5) as

$$(95.6) \quad x = \phi y \longrightarrow \phi^{-1}x = \phi^{-1}\phi y = y,$$

concluding by remarking "De Morgan seems to be the only writer who has treated of such examples with any degree of fullness and ability" [1866b,5-6]. It is of interest to note that the only logicians to suggest a calculus of relations so far did so drawing (implicitly) on their study of functional equations; for Ellis, like De Morgan, had been involved with this subject in the early 1840's [2.5,(17);4.3 text above (6)]. And carrying a bit further Harley's intuitive analogy in (5) above, the reasoning of functional equations suggested the necessity for a symbolic calculus of relations, whereas that of differential equations [see (93.10)] facilitated the construction of an algebraic cal-

culus of deductive logic.

In fact Ellis was the first mathematician to point out Boole's omission of relations; however, his work remained without any impact. He is nowadays acknowledged in Bochenski [1970, 267]. We recall, moreover, that Ellis was the first to point out Boole's error in regarding the principle of contradiction as a consequence of the index law [(84.10), text and (8)]. As we saw in 8.8 this point was emphasized by Harley [1870,14] after Ellis [1863,394] and independently by Young [8.4.(8)] and Venn [1876,490-1].

G.P.Young (1819-1889) lived in Canada but was of Scottish origin and a student of Kelland. His little-known paper "Remarks on Professor Boole's mathematical theory of the laws of thought" [1865] is an interesting assimilation of Boole's work. Contrary to most of Boole's commentators, Young was an exception in accepting uncritically the coefficients  $1/0$  and  $0/0$  [8.5,(4)]. He was also an exception in delving deeply into the metaphysics of LT and in examining the validity of Boole's assaults on Aristotelian logic, proving also that "Professor Boole has been betrayed into observations by which his fame as a philosophic thinker must be seriously affected" [1865,166-172]. Perhaps Young's original criticism explains the lack of attention -and thus of appreciation- to Boole's philosophy by his successors in logic. Young was shrewd to rightly perceive where lay the value of LT when he wrote:

...Professor Boole is entitled to the praise of having devised a Method, according to which, through definite processes, it can be ascertained what conclusions, regarding any of the concepts entering into a system of premises, admit of being drawn from these premises. This Method depends on a Calculus, original, ingenious, singularly beautiful both in itself and in its relations to the science of Algebra, and capable (in hands like those of its inventor) of striking and important applications. In a word, the merit of the Treatise lies in that part of it which has nothing to do with the Laws of Thought, but which is devoted to showing how inferences, from data however numerous and complicated, and whatever be the matter of the discourse, can be reached through definite mathematical processes<sup>(6)</sup>.

Finally, two other commentaries should be noted, by A.J.Ellis

[1873] and Cayley [1871], devoted to two specific aspects only of Boole's logic. The former argued that given the three distinct possibilities all, some and none for the symbols of Boole's logical algebra one can not, as Boole had claimed, draw any analogy between that algebra and one which admits only of the two cases 1 and 0 [8.4, text above (6)]. Cayley's note concerned a minor point of Boole's syllogistic theory in COL [1848b; see 8.1, (1); 8.3] for which Cayley offered a more concise symbolic presentation. Summing up our account so far, and taking further under consideration the influential role of C. Graves [7.1, (9); 7.6, (6); 8.3, (4)] and Cayley [7.5; 8.2], we would like to note that with few exceptions, most of the figures mentioned so far were in some way connected with the development of the calculus of operations.

Despite their shrewd critical remarks on Boole's work before and after his death, these commentators hardly developed logic on his own or on other lines. Jevons was the first substantial critic of Boole, suggesting crucial alterations in his system which were partially followed by late-19<sup>th</sup>-century logicians. Like Bronwin and Carmichael in connection with operator methods, Jevons was largely impressed and influenced by Boole's work, his own system presented in Pure logic [1864] deviating considerably from the lines followed in LT by rejecting Boole's mathematical technicalities. On the other hand, Venn was, like Russell, a most ardent defender in his attempt to explain, diffuse and elaborate Boole's general method in logic, partly rejecting Jevons's charges, in his paper [1876] and basically in his Symbolic logic [1881]. We thus conclude our account with raising the most characteristic points of Jevons's and Venn's opposed approaches to Boole's calculus of logic.

W.S. Jevons, born in Liverpool in 1835, studied chemistry and metallurgy at University College London and after five years in Australia he studied mathematics under De Morgan. He became professor of logic at Owens College Manchester in 1866 and in 1876 professor of political economy at London. He wrote on inductive probability, attacking Mill's inductive logic, and he was the first to construct a reasoning machine. Jevons, best remembered as a pioneer in economic theory, deserves also a place in the history of logic. Jevons introduced himself to Boole in August 1863, enclosing in his letter an extract from his own work in which he refuted Boole's law (95.1) on the grounds that it is

incompatible with the law (82.1)

(95.7)  $x + x = x$

which he insisted including in Boole's system [Grattan-Guinness 1991a,24]. Boole argued why (95.7) is inconsistent with his system [8.2.(5)], and when he was sent few months later the proof sheets of Jevons [1864], he was reluctant to read them with the excuse that he was preparing a new edition of his differential equations. Though partly published by Jourdain [1913], the correspondence of the two logicians was never fully noted until Grattan-Guinness [1991a].

Jevons believed like Boole that the fundamental aim of logic is to find effective means of solution of logical problems based on an accumulation of exhaustive information. The system he proposed was closely analogous to Boole's, distinguished from the latter by the rejection of the calculus of 1 and 0 and its replacement by a method which he regarded as "equally powerful, and at the same time more simple, intelligible, and purely logical" [Harley 1866a,43]. Jevons omitted inverse operations limiting himself to addition, multiplication and negation. Instead of Boole's exclusive disjunction [(84.1)-(84.3)], he admitted of inclusive disjunction, and denoting (after De Morgan) with lower-case letters the opposite of a concept, he formulated as law of contradiction as

(95.8)  $Aa = 0$ .

Further, while Boole denoted the proposition "All horses [h] are (some) [v] animals [a]" by " $h=va$ ", Jevons, working with qualities instead of classes, would denote the same proposition by " $h=ha$ " [Nidditch 1962,44-48; Styazhkin 1969,203-5].

Like most of Boole's successors, Jevons tried hard to take errors out of his system and to strengthen the theory rendering it simpler and especially to reduce the mathematical links. He partly succeeded in so doing striking out the most peculiar aspect of Boole's system, the admission of uninterpretable numerical terms -an issue also attacked by Cayley in 1847 [7.5,8.2]. In fact, very little has been reproduced from his work until recently, and commentators (see (7) below) claim that despite his good intentions he did not avoid misconceptions and his system was less good and less complete than Boole's. Basically, two of Jevons's ideas were to be influential: the change of addition and the expansion of a concept  $x$  in "normal" form as in



$$(95.9) \quad x = x.(y+-y) = (x.y) + (x.-y) \text{ (?)}. \quad$$

We conclude our account with his four objections to Boole's system<sup>(e)</sup>. The first amounted to that "Boole's symbols are essentially different from the names or symbols of common discourse -his logic is not the logic of common thought". Thus Jevons insisted that addition should include non-exclusive terms, since in the common discourse we do not necessarily join logical contraries only. Upon these grounds Boole's operation of subtraction [(84.1)-(84.3)] can not be carried out.

According to Jevons's second objection, "there are no such operations as addition and subtraction [in the mathematical sense] in pure logic". In other words, Jevons refuted implicitly Boole's initial doctrine that the laws governing arithmetic, algebra and the calculus of operations, (93.6), are the only laws non-restricted to quantity and which, after the success of Boole's general method, proved to be indeed the laws of thought he had been investigating [see 8.2.(6)].

The third objection concerns what we have already mentioned, the fact that Boole's system "is inconsistent with the self-evident law of thought, the Law of Unity [(95.7)]". These three objections are variations on the same theme, the rejection of Boole's notion of addition. At this point it is worth recalling our study of Boole's defence of the foundations of his system fifteen years before Jevons's attack. Apparently motivated by Cayley's objections towards

$$(95.10) \quad \frac{1}{-x} + \frac{1}{-x} = x$$

on the grounds that the term  $\frac{1}{-x}$  is uninterpretable [7.5;8.2],

Boole expounded in his manuscript notes N<sub>7</sub>-N<sub>27</sub> the two fundamental operations of succession and aggregation [8.2, text above (11)]. Seven years later, in 1855, he was challenged by R.Latham to clarify why "x+x+.... n terms is not the same as "x"" by drawing (as Jevons was to do but on different lines) on the principles of common language [8.2,(2)-(5)].

As one would expect by realizing Jevons's anti-mathematical position, his fourth and last main attack concerned Boole's numerical symbols. In Jevons's words "the symbols 1/1, 0/0, 0/1, '1/0' are "obscure forms" which "establish for themselves no logical

meaning, and only bear a meaning derived from some method of reasoning not contained in the symbolic system". It was incomprehensible for Jevons <sup>that</sup> ~~that~~ <sup>to</sup> start. "from logical notions and self-evident laws of thought", Boole suddenly transmuted his formulae "into obscure mathematical counterparts" to finally arrive after "various intricate manoeuvres" to "forms arrived at directly and intuitively by ordinary [forms] of Pure Logic" [Jourdain 1913, 121].

Soon after composing LT, Boole had attempted to present the wider audience with a purely discursive logical work. The task proved to be very difficult, used as he were to the efficiency of symbolical methods and confident of his nearly "perfect" general method in logic which had relied heavily upon symbolical reasoning. He delved into a philosophical inquiry into the nature of reasoning with symbols, trying to point out that the issues upon which it rests are not peculiar strictly to mathematical principles. Among his concerns was to define the ambiguous coefficients 0/0 and 1/0 in purely logical terms. All these attempts for clarification, only partly satisfactory, remained unknown due to failure to complete this book and to De Morgan's advice on non publication [8.7.(1),(2),(87.1)-(87.6)]. As we pointed out in 8.5, Venn was the first logician to shed light on Boole's obscure points, especially division, and in fact his attempt partly coincided with Boole's account in his manuscripts entitled LR [8.5.(2)].

J.Venn was descended from a Devonshire family of intellectual distinction and rigid tradition. He entered Caius College, Cambridge in 1853 at the age of 19, graduated as a sixth wrangler in mathematics in 1857, and was soon elected fellow of his college. He took holy orders in 1858 and was appointed to a newly created post in moral science when he directed his attention to philosophy and metaphysics. He devoted himself to the study and teaching of logic between 1870 and 1900, contributing to both logic and probability. He is best remembered nowadays for his diagrammatic method of illustrating propositions by inclusive and exclusive circles, the so-called "Venn diagrams" (often confused with Euler diagrams [Kneale 1962,420]). He was a mountain climber, a keen botanist, and like many of the figures discussed in the thesis an excellent linguist.

Venn's first attempt to explain the procedures of the con-

troversial LT was in his brief paper "Boole's logical system" [1876]. We will touch upon the main issues discussed in this paper, providing references from the corresponding passages from his book [1881] some of which had proved very useful in our study of LT in 8.2-8.6. We will consequently conclude our account with an overall commentary on the merits and weaknesses of Symbolic logic [1881].

Venn focused first on discussing those characteristics of Boole's system which contributed to the view that he regarded logic as a branch of mathematics -implying, apparently Mansel's and Jevons's charges. The first is the doctrine of "expansion" which translated into the familiar logical term of "dichotomy" becomes much clearer than it appears in LT [1876,480-2;1881,192-200;7.5,(1);8.4]. Venn proceeded next to Boole's process of "elimination", once more arguing against the mathematical term and rendering Boole's procedure comprehensible by providing elementary examples [187,482-4]. In a more advanced context, Venn discussed elimination in his book [8.6,(1)], drawing epistemological similarities and differences between logical and mathematical elimination in its "Preface" [1881,xx-xxiv]. The last characteristic concerned the admission of uninterpretable terms. Venn confined at that stage to saying that the occurrence of a rational form in Boole's system is as "destitute of significance" as  $\sqrt{-1}$ , and thus it can be only by reasoning in analogy with mathematics that this employment of rational forms may be partially justified [1876,484-5]. But Venn [1881] found Boole's example from trigonometry as far from satisfactory [8.2,(10)].

In the rest of his paper Venn argued on the merits of Boole's expression  $x=vy$  over Jevons's  $x=xy$  (e.g. of better implying the indefiniteness required), showing however that they each was a consequence of the other [1876,487-9], and also touching upon disjoint addition. He concluded by the remark that, for all his deductive powers, Boole "does not seem to have possessed much of that, certainly rare, metaphysical faculty which distinguishes amongst elementary truths those which are really axiomatic" [1876,490-1] referring to the principle of contradiction [8.4,(8)].

Venn was the first to introduce the term "symbolic logic", regarding it together with mathematics as two branches of one

language of symbols "which possess some, though very few, laws of combination in common" [1881,xvii]. While the core of his Symbolic logic, chapters 2-18, is devoted to a gradual initiation of the reader to Boole's calculus of logic, the "Preface", chapter 1 and the last two chapters include very useful historical notes on the proximities and differences between Boole's symbolic calculus and those of his predecessors and successors. We have already made use of this extremely helpful source of reference in our study, but it is worth stressing some points.

Perhaps the most interesting feature of the "Preface" is that in his attempt to explain the utility of reasoning with symbols in logic, Venn appealed to the epistemological concerns of both De Morgan and Boole, stressing in particular the former's Trigonometry and double algebra [1849c]. The ability of symbolic reasoning to afford the procedure of extension [see 9.3 on our discussion of its importance] was discussed by Venn with special emphasis on the notion of the inverse operation  $f^{-1}$  and its import in De Morgan's inverse relations [1881,xxi;6.9.(16)] and in Boole's logical division [1881,70-72,204]. Claiming that neither Hamilton's, nor Jevons's works were satisfactory, Venn devoted his book to a defence of Boole's system from Jevons's attacks and to a restoration of the "many and serious omissions" traced in Boole's work. He further held that traditional logic deserves attention in education as easier for the beginner to understand, but it should be replaced later on by its extended symbolic form which is "as nearly free from all such accidents of speech as anything dealing with human thought can be" [1881,xxv-xxviii].

Venn referred to Jevons, Grassmann, C.Peirce, MacColl and Schroder for their use of non-exclusive addition<sup>and thus</sup> accepting the law of unity (95.7). However, he provided four reasons for preferring Boolean addition: we confine to mentioning the last "which seems to me [Venn] the strongest of all". Unless we adopt, argued Venn, all of Boole's principles of notation, then "none of those beautiful generalizations introduced by Boole, -such as the formula for Development and that for Elimination,- can (so far as has yet been shown) be admitted" [1881,385].

Venn was perhaps the only one of Boole's followers to hit at the heart of Boole's epistemological concerns when dealing with symbolical methods; the key-motto of Venn [1881] lies in the following phrase:

I have urged throughout that the principal merit of the Symbolic System consists in the speculative questions to which it introduces us; the nature and object of symbolic language as such, being one of the most important of these questions<sup>(9)</sup>.

Also echoing both De Morgan and Boole, he stressed, while discussing division in chapter 9 of [1881]:

The speculative advantages to be gained by really comprehensive logical theorems far outweigh any mechanical saving of trouble which they may secure. To understand the nature of an inverse operation as such; to generalize as far as possible familiar processes; to acquire an intelligent control of symbolic language, as distinguished from a mere mechanical facility in using it, -which can only be done by constantly interpreting its results, especially in limiting cases, and checking them by comparison with the results of intuitively evident processes- these and such as these are the great merits of a proper study of Symbolic Logic<sup>(10)</sup>.

Venn discussed inverse operations at different stages of his book, supplying clarifications missing in LT<sup>(11)</sup> and expressing his surprise why Boole did not define this notion properly in logic given his full knowledge of that definition in the textbook [1859,377; see 8.5 below (85.13);8.7;8.8]. Focusing on the only numerical multipliers  $1,0,0/0$  and  $1/0$  which appear in the development of  $x/y$ , Venn tried hard to show how each of them is equivalent "to a representative of a class, or a direction to take or leave a class" [1881,208]. He further touched upon the notion of inverse relation, in all cases drawing on the mathematical background of Boole and De Morgan [see 6.9,(16),(17)]. However, despite his general training as a mathematician, Venn seemed not particularly acquainted with the calculi of operations and functions. Perhaps, had he had been better acquainted with more details on the principles of symbolic mathematical methods, he might have produced much more satisfactory arguments in the course of explaining Boole's processes and defending them against Jevons's charges.

Compared with Russell in operator methods, Venn lacked his originality in building upon Boole's general method in logic and producing new work. He nevertheless was endowed by a rich historical and intuitive spirit and for all his weak points his

presentation in [1881] still remains the best supplement for a reader who wants to understand the key processes of LT. Besides, Symbolic Logic [1881] is rare in its combining at odd instances Boole's with De Morgan's concerns in symbolical reasoning. Unfortunately, like Russell, Venn had hardly any followers.

9.6 The development of algebraic logic and the logic of relations in late 19<sup>th</sup> century; Peirce and Schröder, the new pioneers.

While Venn ardently supported Boole's algebraic logic -supplying helpful clarifications- he did not develop it. His Symbolic logic [1881] together with Jevons's almost diametrically opposed Pure logic [1864] were not ignored by late-19<sup>th</sup>-century logicians, but their impact was limited. The more substantial -but rather indirect- outcomes of Boole's and De Morgan's logics delayed and are mainly recorded in the works of Peirce, MacColl and Schroder published between 1870 and 1900. It is beyond the scope of this section to delve in detail into these works. We wish merely to hint at the joint influence upon them of Boole, De Morgan -and to some extent Jevons- opening our account with a minor but representative instance from J.J.Murphy's work [see 8.5.(1)]. Once more we will not follow any strict chronological order, providing references mainly from secondary sources.

J.J.Murphy, born in Belfast in 1827, contributed to poetry, prose, sermons, meteorology, faith, and logic, his chief concern with the latter being symbolic notation. He did not produce any considerably original work to deserve a special notice in the history of logic; he was a commentator upon the works of his predecessors. Attracted by De Morgan's logic of relations he proposed a new scheme for the classification of relations [Venn 1881,403-4], and he was among the last generation of logicians to write "On the quantification of the predicate" [1883-1884], raising subtle differences between Hamilton's and Boole's conceptions of quantification. After the suggestion of G.B.Halsted, an American scholar who made valuable critical remarks on both Boole's and De Morgan's works on logic<sup>(1)</sup>, Murphy wrote "On the meaning of addition and subtraction in logic" [1885] where he endeavoured to show that the laws of logical addition and subtrac-

tion are closely analogous to those of logical multiplication and division. Assimilating Jevons's and Boole's opposed systems, Murphy argued that there can be a perfect symmetry between the following pairs of equations

$$\begin{array}{ll}
 (96.1) & x + x = x \qquad \qquad \qquad x - x = \frac{0}{0} x \\
 \text{and} & \\
 (96.2) & xx = x \qquad \qquad \qquad \frac{x}{x} = x + \frac{0}{0} (1-x)
 \end{array}$$

[Murphy 1885,14; Jourdain 1913, 128].

A delicate point raised by Murphy concerns the important distinction between "all" and "every", recalling De Morgan's emphasis on the difference between "cumular" and "exemplar" quantification [see 6.6 text and (12)]. This distinction becomes more interesting when we see that he applied it to Boole's system in order to interpret " $x+x$ " in terms of Jevons's law of unity (95.7) or (96.1). Murphy viewed Boole's symbol of unity as in his first fundamental law (73.1),

$$(96.3) \quad x = 1x,$$

as an operator to mean "all  $x$ ". Under this interpretation we can not possibly add an entire class to itself, hence " $x+x$ " has no meaning. "But if  $x$  is taken to mean any or every specimen of the class  $x$ , then the equation  $x+x=x$  asserts that if we add any substance to itself we have still the same substance. Add water to water, for instance, and we still have water. Perhaps the very simplicity of this interpretation has prevented its being seen" [Murphy 1885,13].

Murphy applied Boole's operator 1 as in MAL in his paper "On the transformations of a logical proposition containing a single relative term" [1889] in order to reformulate more conveniently De Morgan's theorems in the paper on relations  $S_4$  [1860a;see 6.8] in symmetrical symbolic form. We will confine here to the preliminaries of Murphy's notation. Let  $R$  stand for teacher; then " $X$  is all [the only] the teacher of  $Y$ " is denoted by

$$(96.4) \quad X = 1RY,$$

while its inverse " $Y$  is the pupil of none but  $X$ , or of  $X$  only" by

$$(96.5) \quad Y = 1^{-1}R^{-1}X.$$

Thus, when 1 stands before a relative term it means "only" when used as an adjective, whereas  $1^{-1}$  has this meaning when used as

an adverb. While

(96.6)  $1 = 1^{-1}$

is true in arithmetic, in the logic of relations, he stressed, it holds only if R is convertible [1884,133-4]. Finally, besides translating De Morgan's  $X.RY$  in equational form [see also (69.1)], Murphy further used the copula of inclusion "<" as in

(96.7)  $1X < RX$

to stand for "every X a teacher of Y" [1884,174]. Apparently Murphy's ideas had no followers, his named traced so far only in [Jourdain 1913; Venn 1881] as indicated above.

Had Boole had read Murphy's interpretation of the law (96.3) he would have rejected it on the basis that he had viewed it exactly the other way round [see 7.3,(2)], as (73.1)

(96.8)  $x = x1$ ,

x being the operator and not 1. As he stressed in his notes  $N_{7-}N_{27}$  "the  $x1$  in  $x1+x1+....$  refer to different or mutually exclusive entities so that we may have the possibility of aggregation" [8.2,(1)]. In other words, he viewed addition extensionally and not intentionally, as is revealed in his letter to Latham [8.2,(3)].

In any case, Murphy is a representative case of a logician who slightly developed algebraic logic under a joint influence of Boole, De Morgan and Jevons. It is a far more complicated task to illustrate a similar joint influence of our two pioneers in algebraic logic on their original successors MacColl, Peirce and Schröder. While the work of these logicians was carried out largely under the general spirit governing Boole's and De Morgan's works, it oscillated in many ways and in fact it was not produced initially under a direct impact of LT or FL. The new generation of algebraically orientated logicians rediscovered independently issues of propositional logic or logic of relations, incorporating on the way some aspects from Boole's and De Morgan's work and rejecting others. In general, it should be stressed that many aspects of Boolean algebra or modern logic are not in fact due to Boole or De Morgan [Grattan-Guinness 1991a,21; see also 6.9 text below (1);7.1,(21); 9.1].

As shown in Hailperin [1984], Boole provided in MAL the basis for a propositional logic. Hailperin drew on Boole's theory of secondary propositions formally reconstructing an algebra of elective operations, viewing both 1 and x as operators which



satisfy the law

$$(96.9) \quad 1x = x1 = x,$$

which he proved to be isomorphic to what today is known as a Boolean ring, a notion not then in existence [see 7.3.(2),(8); 8.3.(9)]. However, MacColl deserves to be acknowledged as a pioneer in the propositional calculus in 1877. He took the proposition as a real unit in symbolical reasoning, borrowing the symbols 0, 1 from probability to denote truth and falsehood. According to Bochenski [1970,309-10] the development that occurred between 1847 and 1877 in this area of logic was mainly due to Jevons and Peirce, but MacColl's approach marked the highest level of mathematical logic before <sup>G.</sup>Frege's independent and lucid introduction of propositional logic in 1879.

H. MacColl claimed that he read MAL and LT only after the discovery of his own system in 1877. His work presents many similarities with Boole's work but also substantial differences. We recall that Boole had used the symbols  $x, y, \dots$  in LT to represent not propositions but the times during which certain propositions are true [see 8.3.(9)]. MacColl introduced a new vocabulary extending the realms of symbolic logic but also oscillating from the standards of pure logic of his time, a fact which explains his neglect, though he arrived at conclusions now credited to other logicians. An essential novelty was his introduction of statements containing variables, a notion (linked with Peano) upon which lay the essence of mathematical logic at the turn of the century. He fully treated "the logic of functions or relations", the two concepts considered as synonymous, distinguishing between the symbols  $\phi(x)$  and  $\phi$ , the latter standing for "the form alone"<sup>(2)</sup>.

Known as a pioneer of pragmatism, C.S. Peirce contributed to symbolic logic in many and varied ways, his main papers published between 1867 and 1900<sup>(3)</sup>. Among his contributions we would like to stress his memoir "Description of a notation for the logic of relatives", published in 1870. His early papers in 1867 reveal Peirce's acquaintance with Boole's algebra of logic and De Morgan's FL. In fact it is recently argued that Peirce was aware at least of De Morgan's doctrine of the abstract copula, as in FL [6.5], when he began seriously working on the logic of relations in the late 1860's [Merrill 1978,275]. Peirce's originality in the realms of our survey lay in his modification of Boolean al-

gebra so as to accommodate De Morgan's logic of relations. Peirce himself declared that while composing his chief memoir in 1870 he was thinking of Boole and this is evident as the entire algebraic model there introduced is taken over from Boole. What is still, however, wanting of study, is the degree of De Morgan's influence in the shaping of that paper. A comparison reveals a strong Boolean influence which sharply differentiates Peirce's concerns about "relational-class composition" from De Morgan's "relational composition". Moreover, they differed substantially in the treatment of quantified relations [Merrill 1978,275-77,281]. According to Merrill, serious problems in Peirce's memoir might have been avoided had Peirce had taken De Morgan's work more seriously<sup>(4)</sup>.

Peirce defined relations as classes of pairs; let  $l$  denote "lover", then we may write

$$(96.10) \quad l = \sum_i \sum_j (l)_{ij} (I:J),$$

where  $(l)_{ij}$  equals 1 or 0 according to whether  $I$  is a lover of  $J$  or not respectively. On Boolean lines, relative terms were aggregated or compounded (disjunction taken non-exclusively as with Jevons)<sup>(5)</sup>. Peirce's ideas were taken over by Schröder in his Vorlesungen, published in three volumes between 1890 and 1905. Schröder extended Peirce's work in a very thorough and systematic way, the volume of his Vorlesungen devoted to the logic of relations being exhaustive in its account, indicating with its wealth of unsolved problems new directions for research [Tarski 1941,73-4].

Schröder was the last logician of the Boolean tradition in his attempt to present all aspects of logic in algebraic form<sup>(6)</sup>. A most prominent feature of his system was his systematic inquiry of the duality doctrine in algebraic logic, far more thoroughly than Boole, Peirce and others had done earlier. He built an axiomatic approach to the propositional calculus adjoining it to his calculus of classes, like Peirce, and developed after him the theory of existential and universal quantification foreshadowing the work of Peano, Frege and Russell<sup>(7)</sup>. Both logicians produced a heavily symbolic work on logic, their methods regarded by Bochenski [1970,380] as "so cumbersome and difficult that most of the applications which ought to be made are practically not feasible". Nevertheless, they neither drew on mathematical theories nor did they apply logic systematically to a branch of mathematics, with some minor exceptions concerning probability

logic and, in Peirce's case, abstract geometry too [Grattan-Guinness 1988b,75;1991b).

The work of Peirce and Schröder, lying in fact on the borderline between algebraic and mathematical logic, did not have many followers<sup>(8)</sup>. Independently of the direction engraved in their work, ideas of mathematical logic developed on the strength of the internal demands of mathematics itself, the first pioneer of this new generation being the German Frege<sup>(9)</sup>. Concluding we would like to hint at a Boolean influence upon the American mathematician E.V.Huntington and W.E.Johnson at the turn of the 20<sup>th</sup> century, as well as mention the Universal algebra of Whitehead in 1898, the first book after LT "to take seriously the algebra of logic as a field of every day mathematics and [...] to make clear the connections between this branch and other branches of Abstract Algebra"<sup>(10)</sup>.

#### 9.7 Links between chemistry, semiotics, logic and the calculus of operations; the case of Brodie, 1866-1877.

B.C.Brodie graduated from Oxford University in 1842, where he was appointed as Professor of Chemistry during the period 1855-1873. His most interesting research was on beeswax and graphite. An admirer of literature and poetry, Brodie led a rather conventional life, publishing work on a variety of topics until his death in 1880 at the age of sixty-three. Nowadays Brodie's name is linked with a debate which took place in the late 1860's in London about "the atomic hypothesis". He was one of the few chemists and physicists to develop an alternative approach to chemistry independent of Dalton's atomic theory. Brodie's radical proposal for a new system was the topic of his long memoir "On the calculus of chemical operations" [1866] which saw a second part as [1877], both papers published in the Transactions of the Royal Society of London. Unable to achieve his aim, that is to convince chemists to abandon association with atoms, Brodie succeeded in arousing a wide range of reactions by his contemporary physicists, logicians, chemists and mathematicians who tried hard to comprehend his ingenious, but far from defectles, system<sup>(1)</sup>.

Harley considered Brodie [1866] as an endeavour "to do for chemistry what Boole has done for logic, -to reduce it under the domain of mathematics" [1870,15], using the latter term in its "enlarged" sense [see 7.1,(1);7.2,(5)]. Further quoting Brodie [1866,788] about his project "to free the science of chemistry from the trammels imposed upon it by accumulated hypotheses, and to endow it with the most necessary of all the instruments of progressive thought, an exact and rational language", Harley claimed that "Sir Benjamin's system was evidently suggested by Boole's "Laws of Thought". Whether the soil into which he has transplanted Boole's ideas be congenial or not, remains to be seen" [1870,15]. Accordingly M.Boole considered Brodie's work as an outcome of her husband's LT when she wrote "the newest chemical notations has been founded on that used in a book written by a non-chemist and entitled "Laws of Thought"<sup>(2)</sup>.

It is true that Brodie's system "presents many curious and interesting analogies" [1866,787] with LT which was cited on various occasions in his memoir. In fact, the emphasis he paid on the operational character of his symbols recalls much more of MAL than of LT<sup>(3)</sup>. In any case we can regard it as a work produced partly under the influence of Boole's logic and in this respect its study might had been suitably incorporated in section 9.5. However, Brodie drew additionally on the recent development of symbolic algebra and geometry, alluding to people like Gregory and De Morgan, as well to Condillac's semiotics. In this respect Brodie's work comprises a most interesting study on the influence of signs more generally and their import on the progress of science, thus deserving a separate study.

In this section we will discuss the most important issues of Brodie's work set in in a wider epistemological frame in order to incorporate some remarks on the potential links between chemistry, algebra and logic during the 19<sup>th</sup> century. First we will draw some analogies between Brodie's concerns and those of his predecessors, proceeding next to a commentary upon his methodology introduced in [1866]. We conclude our survey with the reactions to his mathematical innovations, touching upon his second memoir, and hinting at some independent studies of Brodie's contemporary algebraists on analogies between algebra and chemistry.

Brodie was not the first chemist to be interested in logic

and algebra, nor the first scientist to perceive analogies between the three sciences. We have alluded to Lavoisier's early attempt to apply Condillac's semiotics to chemical notation [1.8, (15)], as well as to Kirwan's interest in the scientific import of logic and in the mutual utility between logic and algebra in 1807 [6.2, text below (5)]. Whately was the first logician to use heuristically the paradigm of chemistry in order to reinforce his arguments: in his Elements [1826,11-12] he conceived logic as an abstract analytic device which "is like using chemical analysis to examine the elements of which any compound body is composed" -(see also his Rhetoric [1836,77]). Whately's appeal to chemistry is further spotted in Solly's and De Morgan's works on logic as we shall see.

Solly drew on the experimental analysis of  $H_2O$  into  $H_2$  and  $O$  in order to show that "the most remarkable instance of confusion between synthesis and analysis arises from the circumstances that what is analysis considered objectively, is very frequently a synthesis if considered subjectively" [1839,40-43]. It is striking to note the nearly identical statement of M.Boole on this distinction: "Analysis is made by projecting the mind outwards, by the observation of outer facts, but the synthesis which completes the sequence takes place within"<sup>(4)</sup>.

De Morgan drew his first analogy between chemistry and logic in [FL,48-9] by displaying the difference between combination of chemical substances (as in  $H_2+O = H_2O$ ) and cumulation of gases which produce a mixed gas, as a heuristic example of the logical distinction between combination of ideas (as in "animal + rational = man") and mere cumulation of them. Later on he conceived of the "compound relation LM" as a new form (like water) in which the "components are lost in the compound" [S<sub>4</sub>,228-9]. In S<sub>3</sub> adopting a new terminology he wrote "if chemistry had been known as it is now, that which was called the metaphysical whole [(67.4)-(67.5)] would have been called the chemical whole" [S<sub>3</sub>, 98]. In fact by 1858 De Morgan had rejected the use of the sign "+" in syllogistic inferences applying it only for purposes of aggregation and not of composition [see 6.6, below (17)]. In S<sub>3</sub> we read:

The distinction of aggregation and composition is the most important distinction in the subdivision of Logic. Our knowledge does not suffice to define it by full description: we can only il-

illustrate it. To the mathematician we may say that it has the distinctive character of  $a+b$  and  $ab$ : to the chemist, of mechanical mixture and chemical combination: to the lawyer it appears in the distinction between "And be it further enacted" and "provided always"<sup>(5)</sup>.

Appending a footnote to this quotation, he claimed that "The chemist will some day be aware of the great mistake he has made in using the sign  $+$  to denote chemical combination" [S<sub>3</sub>.120.fn1].

Like De Morgan, Brodie also distinguished between  $x+y$  and  $xy$  to denote chemical aggregation and composition respectively: but unlike De Morgan, he viewed these two symbolical terms as equivalent in his system, in analogy with the equivalence between  $H_2+O$  and  $H_2O$ <sup>(6)</sup>. The latter notation was due to Berzelius in 1845, whom Brodie regarded as the "originator of our present method"; Berzelius had considered the letters he employed as representing certain weights of matter, and that "in the symbol of a chemical substance the sign  $+$  was to be understood as connecting every letter in the symbol, and was suppressed only from motives of brevity and convenience" [1866.783-4]. Citing the views of a French chemist on the unimportance of the specific language used to express the inventions of science<sup>(7)</sup>, Brodie argued passionately on the role of signs in the development of chemistry, displaying, as we shall see, a joint influence from the semiotic doctrines of Condillac and Degérando, as well as, from the operational character of algebraic symbols as applied in different ways by Gregory, De Morgan and Boole in mathematics and logic few decades earlier.

Brodie opened his paper by lamenting the slow evolution of symbols peculiar to chemistry in a De Morganian spirit by saying:

Even in the earliest times the attention of chemists seems to have been directed to the symbolic expression of the facts of their science, a method which had its origin in the mystic spirit of alchemy, and the subject has never ceased to occupy a prominent position in chemical philosophy. However, the development of our symbolic system has by no means kept pace with the general progress of the science<sup>(8)</sup>.

[Arguing next against Dalton's atomic theory [1866,782-787], he provided a brief and general description of his methodology before proceeding to define its foundational issues and formulae. In the spirit of the semiotic philosophy of the idéologues [1.8,(17),(18)], and especially of Degérando, Brodie asserted that his paper contained "a new method for the expression, by means of symbols, of the exact facts of chemistry, and for reasoning upon these facts by their aid", the symbol placed "in immediate relation with the fact, being indeed its symbolic equivalent or expression" [1866,787]. In fact he did acknowledge Condillac's views on the synonymous concepts of "language" and "analytical method" as influential in the shaping of his work [1866,788].

He further stressed that his method, "independent of any atomic hypothesis as to the nature of the material world", "may be regarded as a special application of the science of algebra, and in its construction I have been guided by the similar applications of that science to geometry, to probabilities and to logic". Apparently aware of the basic principles of symbolic reasoning, Brodie added:

The symbols which I shall have occasion to employ are of the same abstract character [....]. The conditions to be satisfied by such a method are few and simple. It is only necessary that every symbol should be accurately defined; that every arrangement of symbols should be limited by fixed rules of construction, the propriety of which can be demonstrated; and that the symbolic processes employed should lead to results which admit of interpretation<sup>(9)</sup>.

It is of interest to see how Brodie introduced the notion of operation in his symbolic calculus by drawing an analogy with symbolic geometry (and algebra) rather, than with the calculus of operations<sup>with</sup>, which we have reasons to believe he was not well acquainted ;

The object of this method may be considered to be the investigation of the laws of the distribution of weight in chemical changes, and the symbols here employed represent "weights" in the same sense as the symbols of geometry represent lines or surfaces. Now the symbol  $a$  in geometry, in its primary sense, may be regarded as the symbol of the operation performed upon the unit of length, by which a line

is generated, that is, of which the result is a line. In like manner the symbol  $a$ , as a chemical symbol, is to be regarded as the symbol of the operation performed upon a unit of space, by which a weight is generated, that is, of which the result is a weight. Symbols of operation have not hitherto been adopted in chemistry, and their introduction forms a distinctive feature of the present method, which I have hence termed "the Calculus of Chemical Operations"<sup>(10)</sup>.

Thus employing the symbols  $x, y, \dots$  as symbols of operations performed upon the unit of space, of which the result is "a weight", Brodie defined "aggregation" and "combination" of these operations, the symbol  $+$  signifying "mechanical mixture" and the symbol  $\cdot$  a "compound" weight. He claimed that the interpretation assigned to  $+$  and  $-$  "is strictly analogous to that which has been given to them in the arithmetical and logical systems" [1866, 795], referring in a footnote to [LT,32]. In brief, his symbols operate according to the laws of commutativity and distributivity, the numerical symbols 0 and 1, defined accordingly by

$$(97.1) \quad 0 + x = x \quad \text{and} \quad (97.2) \quad x1 = x,$$

being the symbols of "no weight" in aggregation and composition respectively. Brodie called 1 the unit of space, and the symbol  $\infty$  — which like 0 satisfies equation

$$(97.3) \quad yx = y$$

for any operation  $x$  — the symbol of the ponderable universe. He remarked that the limits 0,  $\infty$  of his calculus correspond to Boole's 0 and 1 [1866,800,fn].

The "fundamental equation" of Brodie's calculus was

$$(97.4) \quad xy = x + y.$$

Its explanation is simple :since  $xy$  is the symbol of a compound (single) weight [like  $H_2O$ ] composed of the same weights as those of the group  $x+y$  [like  $H_2+O$ ], then  $xy$  and  $x+y$  are symbolically equivalent as representing the same weights [1866,801; see also (6) above]. Brodie regarded the law (97.4) as occupying "a somewhat similar place in the chemical calculus to that held in [LT,31] by [...] $x^2=x$ " by expressing "a characteristic property by which the symbols are distinguished". He further added

The possibility of the existence of a class of symbols, other than the symbols of the logarithms of numbers, which should satisfy the



condition

$$f(x) + f(y) = f(xy),$$

was indicated by D.F.Gregory in his paper "On the Real Nature of Symbolical Algebra"<sup>(11)</sup>.

Replacing  $x, y$  by  $0, 1$ , it eventually follows from (97.4) that  
(97.5)  $0 = 1 = 2 = \dots = n$ .

Brodie admitted that "at the first glance" the equation  $0=1$  may appear "paradoxical". In truth, however, it is not "more singular or paradoxical that in chemistry 0 and 1 should be symbols denoting the same object, than that in geometry  $x^0$  and 1 should have the same interpretation" [1866,802]. Thus resolving this paradox to his satisfaction, Brodie proceeded to construct chemical equations from the data afforded by experiment. Among the most remarkable results of his calculus was the proof that certain elements are in fact compound bodies containing hydrogen. Contrary to his belief there was no experimental evidence to confirm this [Farrar 1964,172]. In what follows we present only a sketch of his procedure.

Brodie let a chemical function  $\varphi$  be the symbol of a compound weight

$$(97.6) \quad \varphi = x^n x_1^{n_1} \dots ;$$

where we have  $n$  weights of type  $x$ ,  $n_1$  of  $x_1$  and so on [1866.796]. He further called the symbol of a simple weight (i.e. of one which is not a compound of others) a "prime factor". Brodie's primary assumption was that the unit of  $H_2$ , denoted by  $\alpha$ , is a simple weight and thus undistributed in chemical reactions. If  $\varphi$ ,  $\varphi_1$ ,  $\varphi_2$  stand for a unit of  $H_2O$ ,  $H_2$  and  $O$  respectively we have that

$$(97.7) \quad 2\varphi = 2\varphi_1 + \varphi_2,$$

or, according to (97.6) that

$$(97.8) \quad 2\alpha m \xi^{m-1} = 2\alpha + \alpha^n \xi^{n-1}.$$

Taking further under consideration (97.4) we are finally led to the equation for the decomposition of water

$$(97.9) \quad 2\alpha \xi = 2\alpha + \xi^2.$$

Ultimately Brodie constructed lists of the possible combinations of the prime factors  $\alpha$  and  $\xi$  in (97.8) proceeding to results of practical value [Brodie 1866,808-814; Brock 1967,35-37].

At the end of his paper Brodie compared the atomic theory to a sort of "abacus" which simply facilitates calculations, whereas his new symbolic calculus "affords the same indispensable aid [...] but in a more truthful and effectual way". By means of this calculus "We are thus enabled to construct an accurate symbolic representation of the phenomena before us, on the fidelity of which we can rely". He proceeded, however, to claim that a symbol "should be something more than a convenient and compendious expression of facts. It is, in the strictest sense, an instrument for the discovery of facts [...]. Now as no symbolic system similar to the present has yet been devised, and as this system cannot be deduced from any existing system, every symbol not only makes an assertion but expresses a discovery as to the chemical properties of the substance symbolized". Brodie's epistemological analysis ended with a touch of metaphysics: for him the "simple weights" like  $\alpha$  or  $\xi$  "may be treated purely as "ideal" existences created and called into being to satisfy the demands of the intellect, to enable us to reason and to think in reference to chemical phenomena, but destined to vanish from the scene when their purpose has been served; and the existence of which as external realities we neither assume nor deny" [1866,855-859].

Brodie's appeal to an "abacus" recalls De Morgan's comparison of traditional logic with an abacus [6.9,(1)]. Further, his statement about symbols being instruments for the discovery of facts reminds us of Babbage's [2.9] and De Morgan's [3.6,(9)] views on the merits of signs in reasoning under a semiotic influence. We may finally draw a distant analogy between Brodie's metaphysical concerns and those pursued by Boole and Gratry in the mid 1850's [see 8.9]. A comparison between Brodie, Boole and Condillac has been recently attempted in [Brock 1967,82-86,89-90]. Brodie's statement about symbols is regarded as too strong, and he is accused of misunderstanding Condillac in this respect "by confusing taxonomy with discovery" [Brock 1967,84].

As we saw, Brodie's work had many weaknesses on both a philosophical and scientific level. But what above all was wanting of reconsideration was its logico-mathematical methodology, which was at once attacked on the basis that it was inconsistent for admitting of paradoxical results. Numerous scientists responded to Brodie's paper in 1867 challenging him for a revision of the first part [1866]. As a result the publication of the

wa'

second part [1877]<sup>wa'</sup> delayed, partly meant as an answer to these criticisms. In general, the sequel to Brodie [1866] was even less fortunate, followed by only a shadow of the debate which the first one had stirred. Omitting any reactions relevant to the actual chemical theory produced by Brodie, let us comment upon the most characteristic criticisms and suggestions concerning his mathematical methodology; our main source of reference is the part of Brodie's correspondence in 1867 as published in Brock [1967].

As in Boole's case, similarly in Brodie's case Jevons was the most fierce critic striking at the root of the mathematical formulation of the calculus of chemical operations. As expected, Jevons once more denied the operations of multiplication and division, and arriving at the contradiction

$$(97.10) \quad x + x + x = x + x + x + x$$

he claimed that equations (97.4) and

$$(97.11) \quad x(x+x) = xx + xx$$

himself

are incompatible. Jevons did not confine<sup>h</sup> to communicating to Brodie his disagreement with the fundamental law (97.4), but<sup>he</sup> challenged him to a public dispute in June 1867. Brodie, however, was unable to reply due to illness, a factor that led to an early retirement and the incompleteness of his system<sup>(12)</sup>.

The fact that the distributive law was inconsistent with (97.4) was also noticed by Donkin and De Morgan, whose correspondence with Brodie reveals on one hand the latter's limited knowledge of the principles of the differential and finite difference calculi, and on the other hand some interesting issues concerning the use and abuse of symbolical methods. In his first letter in May 1867, De Morgan thanked Brodie for sending a copy of his paper, adding: "I have long been looking out for an attempt at a calcular distinction between aggregation and combination [see text above (5)]". He called Brodie "an alchemist who is strong and practiced in the calcular and functional notions of algebra" but surprisingly not versed in the differential calculus [Brock 1967, 99-100].

Brodie admitted his moderate knowledge of "the forms of the differential calculus" which he meant to apply in his next paper, willingly expecting De Morgan's illuminating comments [Brock 1967, 100]. De Morgan responded immediately, incorporating in his generous reply representative aspects of the calculus of opera-

tions "into which the forms of all the diff. calc. are packed", claiming that the only thing in fact missing from Brodie's calculus was the "symbol of operation" which would change from aggregation  $(x+y)$  to composition  $(xy)$ . For the law (97.4) could not remain unaltered; following more closely on Brodie's lines of reasoning than any of the other commentators, De Morgan was shrewd to point out that (97.4) is not even convertible:

for though Water = Oxygen x Hydrogen is certainly Oxygen + Hydrogen  
yet Oxygen + Hydrogen is not necessarily Water<sup>(13)</sup>.

De Morgan opened his account by communicating to Brodie how  $\phi x$  becomes  $\phi(x+1)$ , denoted by  $E\phi(x)$ , by means of the operation

$$(97.12) \quad \phi(x+1) = \phi(\phi^{-1}(\phi x) + 1),$$

furnishing him next with the symbolic form of Taylor's theorem. He next pointed out that an aggregate  $x+y$  can be converted into  $xy$  by means of the operation

$$(97.13) \quad e^{\phi(\log x)} x;$$

For example  $2x+4y+6z$  combines into  $x^2y^4z^6$  by means of the operation

$$(97.14) \quad e^{2\log x + 4\log y + 6\log z}.$$

A propos, he remarked "I dare say it has never even been laid down that  $E$  is  $\phi(\phi^{-1}(\quad) + 1)$ "<sup>(14)</sup>.

While De Morgan tried to enrich Brodie's knowledge in order to persuade him to suitably alter the law (97.4), Donkin tried to explain Brodie's foundations, refuting De Morgan's employment of (97.13). Addressing De Morgan, Donkin remarked that what actually Brodie employed was not (97.4) but equation

$$(97.15) \quad x + y = xy + 1,$$

or more generally,

$$(97.16) \quad mx + ny = x^m y^n + m + n - 1,$$

which "never leads to paradoxical results like  $0=1=2=...$  and allows of all ordinary algebraical processes". According to Donkin, these two equations do not contradict the ordinary laws any more than *does*

$$(97.17) \quad \left(\frac{d}{dx} + \frac{1}{x}\right)^n = \left(\frac{d}{dx}\right)^n + \frac{n}{x} \left(\frac{d}{dx}\right)^{n-1}.$$

He added that it is equally absurd to replace  $x, y$  in (97.15)-(97.16) and  $d/dx$  in (97.17) by any numeral [Brock

Donkin wd5 to repeat emphatically this restriction to Brodie in order to prevent him from committing blunders like comparing " $xy=x+y$ " with " $2^2=2+2$ ", and thus to defend the latter's system against Jevons's unsound charges. It is interesting to follow Donkin's arguments in some details:

I really think you ought to persuade Jevons [...] to defer his publication [...]. The unmathematical world (including chemists) will think he has refuted your system. Any mathematician will see at a glance that whatever objection there may or may not be to  $xy=x+y$ , at all events Mr Jevons has not found it out [...]. His objection to  $xy$  shows that either he totally misunderstands what you mean by it, or that he does not know what a calculus of operations really is, or more probably both<sup>(15)</sup>.

Donkin was the only one to distinguish the numerals represented by  $n, m$  etc in (97.16) from the operational symbols  $x, y$  which should not be replaced by numbers. For "it cannot possibly be maintained that all symbols in a calculus of operations ought to be replaceable by numbers". And this delicate point was ignored by both Brodie and Jevons as none of them was really acquainted with the calculus of operations. Drawing on (97.17) — a case of Boole's theorem (93.3) which was regarded by him as one of "the most singular results" of the calculus of operations [Smith 1982a,15; see also 4.7;5.5;5.10;9.3] — Donkin not only stressed the huge gap existing between pure logicians (like Hamilton, Mansel and Jevons) and mathematically trained ones (like Boole and De Morgan) to which were due all the disputes traced so far between the two groups, but also delved deeply like Boole into the subtleties involved in mathematical symbolical reasoning which could easily evoke an abuse of operator methods as Bronwin's case indicated in 1848 [see 5.2,(15)-(18)].

It comes, however, as a slight surprise to see people like Herschel, once a pioneer in the calculus of operations, to be reluctant in accepting Brodie's only phenomenically paradoxical symbolic process. He held that:

I must confess I think it [(97.4)] a fatal objection to any system of notation when it runs so contrary to the conventions of a very widely diffused and [thoroughly] strongly established system [algebra] received on another subject and whose elements it

employs<sup>(16)</sup>.

While Herschel was among the first English analysts in the mid 1810's to initiate abstraction and generalization in the realms of analysis, in the late 1860's he declared that "I cannot be at all sure that I have seized the spirit of the very abstract way in which you present the system". Brodie replied stressing that  $\alpha$  and  $\xi$  are operations and (apparently after Donkin's notes) "cannot be replaced by numbers", trying to persuade Herschel that "I do in the strictest sense conform to the principles of algebra", but nothing particularly interesting was effected by their limited exchange of letters [see (16) above].

Unfortunately for Brodie, his main mathematical supporters, Donkin and De Morgan, died in 1869 and 1871 respectively, depriving him of the opportunity for further help. Traces, however, of their influence exerted in their correspondence in 1867, are spotted in the second part of his "Calculus of chemical operations" [1877], which <sup>was</sup> delayed for reasons already mentioned and which was taken seriously only by Naquet, who raised some questions and translated both memoirs in French in 1879 [Farrar 1964, 178]. Let us note the principal novelties that featured in Brodie [1877] after the decisive influence of his two correspondents a decade earlier.

Drawing apparently on Donkin's suggestion [(97.15)] and Boole's index law, Brodie set off to normalize chemical equations so as they admit of multiplication and division, aiming to answer his critics on one hand, and to proceed to the theory of chemical events where factorization of chemical equations would be lawful on the other hand. Letting  $z$  be the symbol of the weight "contained in an empty unit of space", it follows that  $z$  satisfies the condition

$$(97.18) \quad z^p = z.$$

He then added: "Now among the symbols of number we have one symbol, and one symbol alone, which satisfies the condition satisfied by the symbol  $z$ , namely, the symbol 1. If, therefore, we put 1 as the symbol of the "weight" contained in an empty unit of space, and work with this symbol (as a factor) in the algebra of chemistry, according to precisely the same rules as in general algebra [...] we shall never be led into error" [1877, 39].

Thus the role of "1" in chemical equations is analogous to that of "0" in arithmetic; or,  $\alpha = \alpha + 1$  is "strictly analogous" to  $1 = 1 + 0$ . Brodie showed consequently that no algebraical processes are inadmissible in chemical equations in which the sum of the numerical coefficients in the equation is equal to zero. This is certainly not the case with the so called "abnormal" equations (97.4) or (97.9). To render them "normal" it sufficed to make the identity in the equation apply not only to weight, but also to space; or, by applying his additional theoretical analysis of 1, (97.4) was normalized into (97.15) and (97.9) into (97.19)  $2\alpha\xi + 1 = 2\alpha + \xi^2$  [Brodie 1877,70-73; Brock 1967,43-44].

Following next a different reasoning Brodie arrived at the new fundamental equation (97.15) asserting: "Now this equation, regarded as a numerical equation, is satisfied by the values  $x=1$ ,  $y=1$ ; and since this property is perfectly general, and every symbol of a chemical operation satisfies this condition, every chemical equation must necessarily be true when the prime factors by which it is expressed are severally or collectively put equal to 1" [1877,73]. If this principle be applied to an "abnormal" equation we are led to the "apparent" paradox (97.5) or (97.20)  $1(1) = 2(1) = \dots = n(1) = 0$ , whereas in a "normal" equation "the anomaly disappears" [1877,73-74].

Not all of Brodie's arguments are clear, but he is certainly much more conscious of the principles of symbolical reasoning and careful to avoid inconsistencies. It might be interesting to cite his allusion to De Morgan as appended to a footnote in connection with (97.20):

The apparent paradox involved in this assertion may be removed by assigning a special symbol,  $e^p$ , to the unit of space,  $p$  being a positive integer. But on investigating the properties of this symbol we should soon find that  $e = e^p$ , whatever be the value of  $p$  (the value 0 included), and that, as we might always replace algebraically the symbol  $e^p$  by the symbol (1), we are really dealing with the symbol 1 under another name. Such paradoxes, however, have no significance when the meaning of the expressions employed is properly understood. Thus DE MORGAN, in his "Double Algebra" (ed.1849,p.114), speaking of the term addition as there employed by him, says "Nor is there, in one sense, the slightest objection to

saying that 12 and 12 make 10", an assertion quite as paradoxical (to say the least) as any here made"<sup>(17)</sup>.

Brodie finally applied Taylor's theorem in order to develop chemical equations in a rather eccentric way of no apparent utility<sup>(18)</sup>. On the whole Brodie's sustained effort to found a non-atomic chemistry was a failure. However it would be of interest to note that his spirit is not quite dead [Farrar 1964,178,fn 36]. Moreover, he offered us a remarkable example of a scientist keen in employing the philosophy of Condillac in combination with Gregory's and Boole's algebra of commutative symbols in a bold De Morganian spirit which admitted of inaccuracies and was open to criticism. For contrary to Boole, Brodie looked forward to suggestions for alterations that would render his system consistent and free from fallacies, a task partly fulfilled. Independently of Brodie, links between chemistry and algebra were explored by Sylvester in the 1870's, but it is beyond the scope of our work to enter into this study<sup>(19)</sup>.

## 9.8 Epilogue.

The unity of the forms of thought in all the applications of reason, however remotely separated, will one day be matter of notoriety and common vnder; and Boole's name will be remembered in connection with one of the most important steps towards the attainment of that knowledge<sup>(1)</sup>.

De Morgan.

What in fact, is Logic but that part of universal reasoning; Grammar but that part of universal speech; Harmony and Counterpoint but that part of universal music [...]?

Spottiswoode [1878,14].

From Babbage to Sylvester and from Kirwan to Brodie, most of the British scientists discussed in this thesis dwelled upon similarities and analogies seeking generality of methods and unity of the forms of thought. Condillac's dictum that algebra is a well-made language par excellence [1.8,(10):9.3,(1)] happened to characterize their common concern for a well-established symbolic language which served not only as an efficient tool in calculations but also formed an invaluable source of invention



manifesting, according to Boole, "the intimate and vital connexion of language with thought" [8.8,(2);9.4,(8)]. Relying heavily upon Lagrangian algebras, they effected ultimately a much deeper study of the influence of signs in reasoning than that merely hinted at by the Idéologues at the turn of the century [1.8,(16)-(18)]. Rather indifferent to Cauchy's innovations in rigorously founding analysis upon the notion of limit, they focused ardently on explaining the principles of operator methods (used but refounded by Cauchy [1.7,(13)]), delving further into their nature and thus attaining remarkable results in applications [1.1,(5); 3.1,(2)].

It is well-known that the "deux jumelles", abstract algebra and algebraic logic, were born in <sup>the</sup> mid 19<sup>th</sup> century [Dubreil 1968]. The inquiries into symbolic algebra focused mainly on the role of negative numbers and Peacock's PEF [3.4,(19)]. The history of the calculus of operations and its multidimensional role has been only partially investigated [Koppelman 1971; Laita 1977; Merrill 1990]. A more thorough, unifying approach which would cover distinctly the reasons that led to this simultaneous genesis, the very procedures applied to that effects and lastly a critical evaluation of the symbolical methods employed, together with a study of the degree and quality of their impact to our days was missing. This thesis partly fills these gaps, our concluding remarks noting topics worthy of further attention.

Operator and functional methods were developed in England in the 1810's, and consequently from the mid 1830's up to the 1850's, mainly as a result of an urge:

1. To assimilate and diffuse Lagrangian algebras,
2. To comprehend Laplace's Mécanique Céleste and integrate the EFE and the LE in finite form,
3. To update the curriculum at Cambridge University in the late 1810's and late 1830's,
4. To explain the principles of these symbolical methods, systematize their theory and pose restrictions to an abuse of their application,
5. To construct new general and direct methods for the solution of a wide class of ordinary and partial differential, finite-difference and functional equations, and finally,
6. To find fault with one another's work, extend and elaborate partial results, simplify it, or improve on it by encompassing it

under a more effective and comprehensive scheme<sup>(2)</sup>.

Symbolical methods offered a remarkable ability for the systematization and extension of forms and procedures, as well as for the suggestion of new ones. This was due to their property of abstraction as they permitted separation of form from matter, of the invariable from the variable element, of the laws that govern the symbols of algebra from the nature of the symbols. The so called form-matter issue (cited hereafter FM) did not have its roots in Peacock's PEF; it originated almost simultaneously -and perhaps independently- in Carnot's [1797] principle of indeterminateness of algebraic symbols and in Arbogast's [1800] method of separation of symbols of operation from those of quantity, both works heavily influenced by Lagrange's algebraic calculus [see 1.8]. Babbage [1827] and De Morgan [1836] took over Carnot's line [2.9;3.4-3.6], whereas Herschel [1814], Murphy [1837], Gregory [1841] and Boole [1844], to mention our main pioneers, deployed Arbogast's method [2.3,2.7-2.8;3.3;4.4-4.5]. Of interest to note is the case of Sylvester [1867], where we see the blend of the two forms of this issue in a way strongly foreshadowing 20<sup>th</sup>-century studies in the metalanguage of symbolic logic [see 5.10,(12)].

Peacock was a member of the Analytical Society and contributed substantially to the educational reforms at Cambridge in the late 1810's [3.2,(2)]. However, he was not involved in Babbage's and Herschel's work [2.8,(8)], and as De Morgan pointed out, he omitted from his Algebra [1830] any reference to the calculus of operations or the FM issue [3.9,text below (6) and (16)]. Peacock's most influential novelty was the notion of extension, introduced via the PEF, which allowed the passage from arithmetic to the more comprehensive and abstract structure of algebra. Mathematicians were thus equipped with a new tool for generalization other than ordinary mathematical induction<sup>(3)</sup>. Under his influence Gregory combined Arbogast's and Servois's work, establishing operator methods and preparing the grounds for Boole's non-commutative algebras [4.4;9.3]. On the other hand, De Morgan applied Peacock's, Carnot's and, to some extent, Arbogast's principles in order to found the calculus of functions and develop symbolical algebra [3.5-3.9].

We believe that these two distinct, though fairly close, lines of abstract reasoning have not been stressed before as two different methodological tools (in fact the role of Carnot's principle is missing from relevant historical studies). To trace their origin, to stress the educational, foundational and epistemological concerns of their main cultivators, taking further under consideration the wider state of mathematics and logic during the first decades of the 19<sup>th</sup> century, means to clarify the genesis of the "deux jumelles" at mid century. For it was these two lines of reasoning, as based on Arbogast's and Carnot's principles respectively (enriched on the way with Servois's laws and Peacock's PEF) that served as background to Boole's algebraic logic and De Morgan's logic of relations.

The FM issue, or the ability to isolate forms and to extend procedures, did originate in mathematics; but, as both De Morgan and Boole independently realised, it is not an issue peculiar to mathematics. It forms part of universal reasoning regardless of symbols whose function simply serve to facilitate and manifest its power in applications. Upon these grounds Boole appealed to the laws (93.6) in his logic, solved elective equations in analogy to differential equations, applied Maclaurin's theorem for the expansion of elective functions, admitting moreover of uninterpretable terms in intermediate steps of his symbolic procedures. On similar lines, De Morgan did not hesitate to draw on his calculus of functions when handling abstract copula, converse relation and composition of relations. In combination with the FM distinction, Peacock's PEF actually reinforced our pioneers' confidence in employing symbolic algebra as a science of suggestion [see 9.3].

To a great extent this thesis studied the reasons that led to the highlights of symbolical methods at mid-19<sup>th</sup>-century England and the links between the development of these methods and differential equations, algebra and logic. Several relevant questions arise concerning the peculiar features of this development, e.g. its non-linearity, the fact that it involved many heterogeneous figures and the obstacles encountered on its way. And finally the impact of Boole's and De Morgan's symbolical reasoning on science and logic in late 19<sup>th</sup> century comprises an interesting topic for further study, reflecting best the evaluation of the Lagrangian algebras movement in the first half of

the century.

These questions have been partially dealt so far; in what follows we suggest new researches to fill in any gaps and to clarify any ambiguities. To start with, the attempt <sup>at</sup> a Renaissance of English mathematics by the Analytical Society, and most particularly by Babbage and Herschel, found a <sup>great</sup> impediment in the poor state of professionalisation in Cambridge in the first decades of the century [Enros 1979,208;see 2.8,(10);9.2]. This obstacle did not cease to exist in the following decades; on the contrary, it was further enlarged by Whewell's conservative program [3.2] and financial problems that put restrictions in publications [9.2 text and (3)]. Due to these problems the development of English mathematics was far from linear and rapid up to the mid 1830's. Due to the reasons 2-4 listed above, interest in functional and operator algebras revived, and the pioneering work of Gaskin, R.L.Ellis, De Morgan, Murphy and Gregory [Table 3] had as a result Boole [1844] which marked a new era in the history of symbolical methods [Table 4].

Until the 1840's the centre of research was Cambridge University but soon the situation was altered and new figures emerged, many of whom worked for a living in other areas than mathematics. In 9.3 we recorded the reasons <sup>that</sup> these diverse intellectuals were attracted to D-operator methods elaborating Boole's general method in analysis under a more or less competitive spirit [see (2) above]. Can we say that the group of mutually influenced analysts of the 1850's, Boole, Bronwin, Hargreave, C.Craves, Donkin and Carmichael -to mention the most outstanding contributors in D-operator methods- formed a real school? Taking under consideration their common ardent pursuit of symbolical methods we might say yes, in a loose sense. However, if we consider the marginal character of their results, the lack of their functioning as a group, their lack of collaboration in the sense of Babbage and Herschel, and, if we further compare them with the new algebraists Cayley, Sylvester and Spottiswoode, then the answer tends to be no.

The influence of this "group" and the wider impact of symbolical methods was rather limited from 1860 onwards; the main reasons for the gradual decline of Boole's general method and its by-products were the following:

- 1) The search for general methods was proved utopic by 1851 and

attention focused on particular methods.

2) Boole's peculiar techniques were regarded as artificial and complicated and Gregory's more direct and simple procedures revived.

3) Despite Murphy's, Gregory's and Boole's [and Carmichael's] attempts to establish symbolical methods, there was still a mistrust in them due to Whewell's conservative attitude.

4) For non-specified reasons, most late-19<sup>th</sup>-century mathematicians seem to be ignorant of Carmichael's and Boole's work on non-commutative operators [the latter textbooks, even when referred to, were far from thoroughly read].

5) Mathematicians and textbook writers on physical astronomy pointed out the little value of symbolical solutions of differential equations related to physics.

6) The most eminent pure mathematicians to be involved with differential equations (like Cayley and Glaisher) employed mainly the method of solution by series, focusing on particular, rather, than general solutions.

7) It was the new algebra of forms (group theory, determinants, invariants etc) which monopolized the interest of the new generation of algebraists from mid-century onwards, and finally.

8) Many important contributors in symbolical methods died soon after the most important publication of their works: Carmichael (1861), Boole (1864), Hargreave (1866), Donkin (1869) and De Morgan (1871)<sup>(4)</sup>.

Boole's influence, though, was not altogether to vanish as symbolical methods ceased to be at the forefront of research, reaching at the turn of the century a ~~standard~~ utility confined to linear differential equations with constant coefficients [9.1.(2);9.4.(17)]. The last representative traces of his work on non-commutative operators are found in Russell's encyclopedia article [1873] and Glaisher's invaluable article [1881], both unknown to historians so far. Boole's textbook [1859] formed a model for Forsyth [1885] on differential equations, and his name survived in a few 20<sup>th</sup>-century textbooks as a pioneer in symbolical methods [9.1.(2);9.9.(17)]. But his method for the solution of equations with variable coefficients was not to be employed in the realms of differential equations from the 1880's onwards. The case of finite difference equations, however, was less dis-

sappointing, as Boole's general method was to survive under modifications until <sup>the</sup> mid 20<sup>th</sup> century [see 9.4, (18)].

As far as the utility of operator methods in physics is concerned, we would like to note the following. Only Pratt and Todhunter mentioned en passant Boole's, Hargreave's and Donkin's finite solution of the LE [9.4, (15), (16)], making it clear that symbolic solutions were not of any practical utility in physical astronomy, omitting altogether any mention of their voluminous researches on the EFE which had been a motivation in the study of symbolical methods in the 1840's [4.6; Tables 3-4]. It was the series method to be applied to the Legendre, Bessel and other equations, prominent for their role in physics from the 1870's onwards [9.4]; in particular, the LE was to be treated by an improvement and elaboration of Poisson's techniques (after Laplace [1.3]), forming in our century one of the most obscure branches of analysis [Whittaker 1902, 1952: 3.2]. Related to the more recent history of symbolical methods as linked with physics, is Heaviside's work on operator and operational methods in the 1890's. Produced largely under Boole's influence, Heaviside's work was rejected due to lack of rigour. His main contribution was that he initiated a new field for the application of symbolical methods, that of electrical engineering. Up till the 1950's the legend in our century was that operator methods were invented by Heaviside; and the theorem of expansion (17.51), known to Cauchy, Gregory, Lobatto and Boole, was wrongly attributed to him<sup>(5)</sup>.

Concerning logic, Boole's and De Morgan's main successors were Peirce and Schröder. They developed De Morgan's logic of relations within the framework of a modified Boolean system, further pioneering the introduction of the predicate calculus and the use of quantifiers. Schröder influenced Skolem and Lowenheim in our century, but a more thorough survey on Boole's and De Morgan's impact on the evolution of algebraic (and to some extent mathematical) logic opens up a fruitful area for research. An interesting question, not yet raised and relevant to the period under study, is how late 19<sup>th</sup>-century English mathematicians viewed Boole's and De Morgan's mathematically influenced logics. Did they pay any attention to this branch of reason? Did they incorporate it under the wider realms of the study of forms via their epistemological tool of morphology [5.9-5.10; 9.4]?

We saw in 9.5 that among the first commentators on Boole's calculus of logic included mathematicians with an interest in the calculi of operations and functions, R.L.Ellis being the most representative case. There followed Jevons who was attracted by Boole's calculus as a device of reasoning, but who stripped Boole's logic not only of its most peculiar mathematical elements but also of its wider metaphysical background. In a way Jevons's attitude foreshadowed 20<sup>th</sup>-century historians' tendency to isolate and emphasize Boole's apparent dual algebra of 0 and 1 and connect his work with the rise of computer science<sup>(6)</sup>. From the more philosophically and literary orientated new algebraists such as Sylvester and Spottiswoode, one might expect a deep appreciation of Boole's investigation of the forms of thought. Nevertheless, this was not the case. Sylvester did make some interesting, though marginal, comments on the links between common language and its grammar, logic and the language of operators which were of no further notice [9.4,(5)]. Spottiswoode, emphasizing the limited mechanical utility of the prevailing logical systems wrote in his Report [1878]:

And, coming to recent times, although we may admire the ingenuity displayed in the logical machines of Earl Stanhop and of Stanley Jevons, in the "Formal Logic" of De Morgan, and in the "Calculus" of Boole; although as mathematicians we may feel satisfaction that these feats (the possibility of which was clear a priori) have been actually accomplished; yet we must bear in mind that their application is really confined to cases where the subject-matter is perfectly uniform in character, and that beyond this range they are liable to encumber rather than to assist thought<sup>(7)</sup>.

Maybe late-19<sup>th</sup>-century English mathematicians lacked the necessary insight to fully appreciate De Morgan's and Boole's attempts to extend and reformulate the forms of common logic into unity, surprisingly regarding their success as given "a priori". Moreover, there is not a single word on De Morgan's logic of relations, and in general not a single hint on the potential utility of their logical contributions within pure mathematics. We suggest that a more thorough consultation of the reports and textbooks of pure mathematicians and physicists of the last decades of the 19<sup>th</sup> century may prove useful in furnishing a more

comprehensive view on the intellectual benefits of the rise of operator methods and the reasons of their decline. In particular, the role of the notion of operation in symbolic geometry, mechanics, the calculus of variations and quaternions, as well as in Cayley's group theory, needs further investigation. Another pertinent topic of research concerns the work produced under the influence of Arbogast's calculus of derivations and Brinkley's  $\Delta^n O^m$  numbers on Cayley, Sylvester and a dozen minor figures who contributed papers in the Journal of Pure and Applied Mathematics in the 1860's<sup>(8)</sup>. Finally, it would be interesting to carry a comparative study between English symbolic algebra and logic and the formal structures introduced in Germany by M. Ohm, Grassmann and others.

We conclude with a brief commentary on an interesting but neglected figure Glaisher. In his "Address on mathematical and physical science" [1890] he revealed a romantic support of "the abstract sciences" when "treated by means of a powerful symbolic language" [1890, 725]. Recalling De Morgan's historical spirit and tendency for optimistic prophetic remarks, Glaisher conveys in the passage below an appeal to Boole's search for general methods as an alternative to the clumsy method of series:

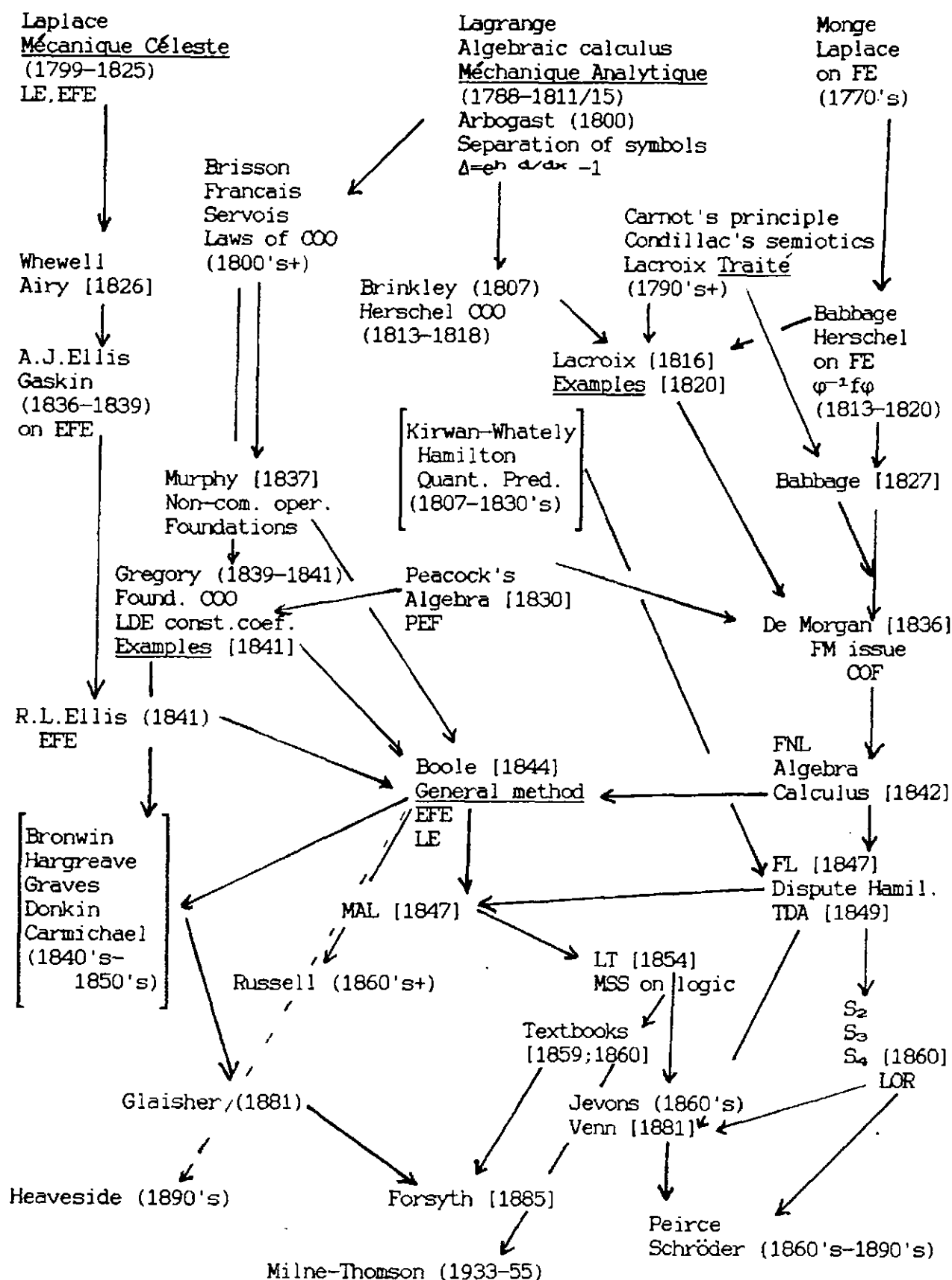
... I believe that every mathematician must cherish in his heart the conviction that at any moment some special analysis, devised in connection with a branch of pure mathematics, may bear wonderful fruit in one of the applied sciences, give short and complete solutions of problems which could hitherto be treated only by prolix and cumbrous methods. For example, it is difficult to believe that the present unwieldly and imperfect treatment of the Lunar theory is the most satisfactory that can be devised. We cannot but hope that some happy discovery in pure mathematics may replace the clumsy and direct analytical methods exactly suited to the problem in question<sup>(9)</sup>.

As an overall summary of our investigation we append a table which records the main line of the development of Lagrangian algebras by British mathematicians during the period 1800-1860. The reader can additionally refer to Tables 1-5 [9.2-9.3] for more details and for any abbreviations used in Table 6 which follows on the next page.



Table 6

The rise of symbolical methods in England and their effect on the genesis and development of algebraic logic in the 19<sup>th</sup> century.



## Endnotes

### Chapter 1.

#### Section 1.1

- (1) This quotation forms part of Valéry's marginal notes, written in 1929-1930, of his essay entitled "Introduction to the method of Leonardo da Vinci" [Valéry 1977, 62]. See also [7.1, (2)].
- (2) As Laplace's early work lacks any references it is very difficult to judge the degree of influence effected on him by Lagrange and others. Particularly on Lagrange's influence on Laplace see [Grattan-Guinness 1990, art. 5.4.4].
- (3) Greenberg's thesis [1979] on the link between the integral calculus and the earth's shape in <sup>the</sup>1740's in France is an excellent illustration of the mutual fertilization of these two domains. See also comments in the introduction of [Todhunter 1873a].
- (4) Under this title the book appeared in the first edition in 1788. In the second edition, in 1811-15 the title was spelt Mécanique Analytique.
- (5) On the contrast between Lagrange's and Cauchy's calculus see [Grabiner 1978, 1981a, b]. On Cauchy see also [Grattan-Guinness 1990, chap. 11; Kline 1972, 948-980]. On its slow diffusion in England see [Grattan-Guinness 1986; Guicciardini 1983; Smith 1984b].

#### Section 1.2

- (1) Both Lagrange and Laplace were influenced from the discoveries of earlier geometers such as I. Newton (1642-1727), A. Clairaut (1713-1765), MacLaurin (1698-1746), D'Alembert (1717-1783), Jacques Bernoulli (1655-1705), Jean Bernoulli (1667-1748), L. Euler (1707-1783) and A.M. Legendre (1752-1833). In his Mécanique Lagrange acknowledges most of his predecessors and contemporaries both in connection with the principles of dynamics and with variational methods. On the origins of his work see [Fraser 1985; Grattan-Guinness 1990, art. 5.2.1- 5.2.4, 5.3.1].
- (2) Quotations in text are from [Jellet 1850, xix]. Jellett's introduction provides a brief outline of the history of the calculus of variations. See also [Fraser 1985; Grattan-Guinness 1990, art. 4.5.1; Kline 1972, 513-582; Mach 1902, 422-436].
- (3) See references in (2) above. Particularly see [Jellet 1850, 1; Mach 1902, 436].
- (4) On a brief outline of the early history of the calculus of operations see [Koppelman 1971, 157-158].

(5) For details on the derivation of equations (12.4)–(12.6) see [Joos 1934, chaps 4, 6]. Joos's approach is fairly close to that of Lagrange. On the Euler–Lagrange communication see [Fraser 1985]. For comments on Lagrange's equations (12.5)–(12.6) and on Euler's approach to dynamics see [Grattan–Guinness 1990, art. 5.2.4–5.2.5].

(6) On these two principles see [Fraser 1985, 172–6; Grattan–Guinness 1990, art. 5.2.3, 5.3.1].

(7) Mach regards Jellett [1850] as the best treatise written on the subject on lines very close to those followed by Lagrange [Mach 1902, 437]. On Jellett's influence on Carmichael see [Koppelman 1971, 213] and 5.7.

### Section 1.3

(1) In the first volumes Laplace omits any reference to his predecessors and contemporaries mentioned in [1.2, (1)]. In the last volume the case is different. Among others, he acknowledges the work of Lagrange, Legendre and Fourier. As Laplace's work is more lucid compared to that of Lagrange's, and as his style was followed by British analysts, we will cite it directly by giving the date of the volume, the number of the Book and finally the number of the respective article. When explanations or proofs are missing, English textbooks will be cited. The reader can also consult Bowditch's translation of the first 4 volumes which appeared in Boston in 1829–1839. On this translation see [Grattan–Guinness 1990, art. 5.4.3; Kline 1972, 495–6].

(2) At the turn of the century "Laplacian physics" was followed by mathematical physics to which Laplace himself contributed from the late 1810's onwards. See [Grattan–Guinness 1981, 97–99; Herivel 1966, 120–129].

(3) See Poisson's penetrating remarks on the difference between Lagrange's and Laplace's style in [Grattan–Guinness 1990, art. 5.4.4]. Brief biographies and interesting comments on these two analysts are also included in [Kline 1972, 493–496].

(4) Equation (13.2) first appeared in a memoir of Laplace written before his Mécanique. The function  $V$ , named "Potential" by Green in 1828, was attributed by Legendre to Laplace. See [Todhunter 1873b, 25–26, 56, 46, 176; Kline 1972, 683]. For details on the formulation of equations (13.2) and (13.4), known earlier to Euler, see [Pratt 1836, 154–159]. Pratt's textbook gives a slightly simplified version of Laplace's approach enriched with certain theorems provided by Poisson.

(5) The name "Laplace coefficients" was given to  $Q^{(1)}$  by Whewell in his [1830, 146]. The distinction between coefficients and functions was provided by Pratt in the second chapter of his [1860]. Usually by "Legendre polynomials", denoted by  $P_n(x)$ , we mean the coefficients of  $a^n$  in the develop-

ment of  $(1-2ax+a^2)^{-1/2}$ ,  $n>0$ . Hence  $P_n(x)$  satisfy the respective equation (13.6) in one variable, namely

$$(1) \quad \frac{d}{dx} ((1-x^2) \frac{dP_n}{dx}) + n(n+1)P_n = 0$$

For details on Legendre's priority over Laplace see [Todhunter 1873b, 23, 57, 138-143, 189-190]. In the text we will use the term "Laplace coefficients".

(6) Lagrange regarded Laplace's demonstration of theorem (13.8) as erroneous and provided a rigorous proof. Laplace referred to Lagrange's demonstration in his (1825, Book 11, art. 3) claiming though that his own proof had appeared as erroneous only because he had wanted to present it in a simple manner. For further comments on (13.8), including a proof of this theorem, see [Todhunter 1873b, 57-59].

(7) For details on Laplace's procedure as briefly presented in text see [Todhunter 1873b, 190-1]. On another process followed by him earlier in a memoir see [ibid, page 59]. On Poisson's proof of the general property quoted in text—given in the 8<sup>th</sup> chapter of his (1835) —see [Pratt 1836, 164-9]. The relation between  $U^{(1)}$  and  $Y^{(1)}$  was easily deduced by Laplace in art 12 to be

$$U^{(1)} = \frac{4\pi}{2i+1} a^{1+3} Y^{(1)} + \omega_0$$

(8) Let  $Y^{(i)}$ ,  $Z^{(j)}$  be Laplace coefficients of different indices. According to the orthogonality property we have

$$\int_{-1}^1 \int_0^{2\pi} Y^{(i)} Z^{(j)} d\mu d\omega = 0 \quad \text{and when } i=j \text{ we have}$$

$$\int_{-1}^1 \int_0^{2\pi} Y^{(i)} Z^{(i)} d\mu d\omega = \frac{4\pi}{2i+1} Z_1^{(i)},$$

where  $Z_1^{(i)}$  is the same as  $Z^{(i)}$  when  $\theta$  and  $\omega$  are changed into  $\theta_1$  and  $\omega_1$  [Pratt 1836, 169-170; Todhunter 1873b, 61, 125].

(9) By means of the transformation

$$\rho = e^{\int \mu dr} \quad \text{and} \quad t = \mu + \frac{Rr^2}{\int Rr^2 dr}, \quad \text{Clairaut reduced (13.13)}$$

to the form

$$\frac{dt}{dr} + t^2 = \frac{r^2}{\int Rr^2 dr} \frac{dR}{dr} + \frac{6}{r^2} \quad \text{which is a particular case of (13.14).}$$

Clairaut acknowledged Riccati as the first to introduce such equations, as

(13.14), in the process of reducing second order differential equations to first order ones [Clairaut 1743, 276; Greenberg 1979, 568-570]. Clairaut was the first, not only to formulate the earth figure equation, but also to perceive its link with the Riccati equation. On the Riccati equation see [1.4].

(10) During his procedures Laplace concluded that  $Y^{(0)}$  can be arbitrary and for simplicity he often considered it as equal to 0. Among other properties regarding  $Z^{(1)}$  he proved that

$$\int \frac{d\Omega}{\rho} = V + ar^2[Z^{(0)} + Z^{(2)} + rZ^{(3)} + r^2Z^{(4)} + \dots] \quad \text{and}$$

$$Y^{(1)} = \frac{3(2i+1)}{8(i-1)n} a^{1-2} Z^{(1)},$$

but of most importance was his proof of the independence of  $Z^{(1)}$  from  $a$  [art. 23-24].

(11) For a detailed discussion of Legendre's and Laplace's procedures on this matter see Todhunter's paper [1879] devoted to a study of (13.16) (where  $Z^{(1)}$  is taken to be 0). In this paper Todhunter proves in detail the reduction of (13.16) to (13.17) and comments upon the later reproductions of Laplace's procedure in British textbooks of physical astronomy.

(12) We have avoided to provide all the details of Laplace's long and complicated procedures. A simplified short version of Laplace's intermediate steps from (13.15) up to (13.31) is given in [Pratt 1836, 541-558]. However, no proof of (13.31) was provided there either.

(13) Laplace assumed the solution of (13.34) to be of the form

$$q = \frac{A}{\sqrt{nk}} \sum \left[ \frac{F^{(s)}}{z^{2s}} \sin(z + \theta) + \frac{F_1^{(s)}}{z^{2s+1}} \cos(z + \theta) \right].$$

He then derived by substitution of  $q$  in (13.34) recursive relation<sub>A</sub> between  $F^{(s)}$ ,  $F_1^{(s)}$  and  $F^{(s-1)}$  [1825, Book 11, art 9].

(14) See (2) above and [Grattan-Guinness 1990, chapters 6-7; Todhunter 1873b].

(15) On theorem (13.35) -whose proof by Poisson in 1813 was not rigorous- see [Kline 1972, 530, 682; Pratt 1836, 156; Todhunter 1873b, 357]. On Poisson's proof on the expansion of a function of  $\mu$  and  $\omega$  see (7) above.

## Section 1.4

(1) The most extensive account of Clairaut's and Fontaine's contributions in various domains of the integral calculus and mechanics is <sup>V</sup>Greenberg's excellent thesis [1979].

(2) For the most important results in 18<sup>th</sup> century work on differential

equations see chapters 21, 22 in [Kline 1972]. Another interesting source of information is [Vessiot 1910].

(3) See [Kline 1972, 500]. The switch to partial differential equations is also stressed by Grattan-Guinness in his recent extensive work [1990] on French mathematics and physics in late 18<sup>th</sup> and early 19<sup>th</sup> century; see particularly art. 3.3.1.

(4) The importance of the link between these two equations was raised by Glaisher [1872, 137]. The most extensive account available on the transformations of the Riccati equation and on some definite integrals which satisfy them is [Glaisher 1881]. On the points raised in the brief sketch given in text see particularly his [1881, 759-763, 779-782, 807 and 823-828].

(5) On Lagrange's innovations see [Engelsman 1980; Grattan-Guinness 1990, art. 3.3.5-3.3.7; Kline 1972, 478, 534]. On the history of the development of singular solutions see also [Boole 1877, 174-177].

(6) On the contributions of these analysts see references on Lagrange in (5) above. In addition see [Kline 1972, 532-540; Vessiot 1910, 113].

(7) On the equation (14.13) and the debate over the application of trigonometric series for the representation of  $\psi$  and  $f$  see [Dhombres 1986, 127-133; Engelsman 1984; Grattan-Guinness 1990, art. 3.3.2-3.3.3; Kline 1972, 454-59, 502-505, 513].

(8) On Poisson's equation  $\varphi(x+y) + \varphi(x-y) = 2\varphi(x)\varphi(y)$  see [Dhombres 1986, 150]. On its solution by differentiation see [Boole 1860, 227-28].

(9) The equation solved by Lagrange is the following:

$$\alpha\varphi[t + a(h + kt)] + \beta\varphi[t + b(h + kt)] + \dots = T,$$

$\varphi$  the unknown function [1766, 201-205].

(10) It is exactly for this reason that we gave Boole's application of Laplace's method at this point. In fact, we will not deal with this method extensively in later chapters.

(11) Monge's first two memoirs are included in [Koppelman 1971, 171, fn 33] only as bibliographical reference. The only account of his process, as in (14.20), is given by [Dubbey 1978, 88]. In [Dhombres 1986] the only account given of Laplace is concerned with his application of D'Alambert's method for the solution of (14.18) [see text above].

## Section 1.5

(1) This tendency is particularly striking in early and mid-19<sup>th</sup> century England. It forms the basis of the calculus of operations, and is noticed with Brinkley and Herschel in the 1800's and 1810's up to De Morgan, Gregory and Boole in the 1830's and 1840's [see chapters 2-5].

(2) Laplace was indeed very proud of his invention. See the introduction to

his [1811] summarised in 1.7. Though it is true that in certain aspects their methods overlap, an actual comparison is far from easy to be drawn. A further study of Laplace's generating functions—only touched upon in recent works—may bring to light interesting similarities and contrasts. Nevertheless, its basic contrast with Arbogast's method is that it is devoid of explicit symbolical principles.

(3) I follow Lagrange's original notation with minor abbreviations such as writing  $n!$  for  $1.2....n$ .

(4) A sketch of Lagrange's proof of (15.10) is included in [Goldstine 1977, 167; Koppelman 1971, 158–9]. However, both accounts miss essential aspects of Lagrange's reasoning and a reliance upon them can be very misleading. For a brief commentary upon Lagrange's and Laplace's results in the calculus of operations see [Grattan-Guinness 1990, art. 3.4.1–3.4.3; 3.5.2].

(5) On the special case  $\lambda=-1$  see also [Goldstine 1977, 170]. Lagrange developed further his algebraic calculus in later works, such as [1797] and [1806]. See [Fraser 1987; Grattan-Guinness 1990, art. 4.3.1–4.3.3].

(6) I follow Laplace's notation with minor changes. I put  $u_1$  instead of  $u'$  and in formulae (15.25)–(15.26) below I use  $d$  instead of  $\partial$ .

(7) This assumption was expressed by Herschel in his [1816, 25]; see 2.3.

(8) Laplace's treatment for  $n=-1$  was reproduced in [Lacroix 1819, 106–109; see also 2.2–2.3].

(9) By [1812] I will cite below the first edition of this book. By [OC,....] I will cite its third edition included in Laplace's Oeuvres Completes [for details see the bibliography].

(10) This symbol  $\underset{\sim}{G}$  was used by many English analysts, who applied Laplace's method, such as Herschel, Hymers, Pearson and Boole. The latter avoided an extensive use of Laplace's generating functions viewing it as an inverse method whose application implied certain limitations and thus he referred to it mainly for historical reasons [Boole 1860, 14–16]. The limitations involved in the consideration of inverse operations would be acknowledged by Sarrus in 1822 [see 1.6]. For example, the step from (15.36) to (15.37) below in text is based implicitly on the distributivity of the operation  $\underset{\sim}{G}$ . But this property can not apply to Laplace's own procedure, since the concept of distributivity was not known at that time and his process was not symbolic either.

(11) We will provide reference information in the course of chapters 2–5. For the reason mentioned in (2) above we will not deal extensively with this method in the thesis.

(12) For recent research on Arbogast's earlier work, including interesting comments on his [1800], see [Grabiner 1981a, 316, 323; Grattan-Guinness 1990,

art. 4.3.6; Koppelman 1971, 161-162].

## Section 1.6

(1) Brief comments on Brisson are included in [Cooper 1952, 8; Ince 1927, 114; Pincherle 1912, 10; Vessiot 1910, 108]. A partial study of his [1808] is carried out by [Grattan-Guinness 1990, art. 4.3.7; Koppelman 1971, 162-163]. For references on Petrova's extended work on Brisson see her [1987, 22-23].

(2) By  $\nabla$ , Brisson denoted expressions such as

$\frac{d^2}{dx^2} + \frac{d}{dx} \frac{d}{dy} + \frac{d^2}{dy^2}$ . In his [1776a, 74] Laplace used this symbol in order to de-

note the product  $H_1.H_2....H_k = \nabla H_k$  of the terms of a series. For Sarrus [1822] it stood for an arbitrary function or operation. Cauchy and Carmichael used it also in 1843 and 1851 respectively for the symbolic representation of partial differential equations. On Carmichael see 5.7.

(3) On Français's work see [Grattan-Guinness 1990, art. 4.3.5 ; Koppelman 1971, 163-4, 173-4].

(4) On Servois see [Cooper 1952, 8; Grattan-Guinness 1990, art. 4.3.7 ; Koppelman 1971, 174-5; Pincherle 1912, 5]. In none of these references do we see any comments on Servois's definition (16.17).

(5) The reader can draw a comparison between Sarrus [1822] below in text and Murphy [1837] in 3.3 .

(6) Interesting comments on Gergonne's journal are included in [Grattan-Guinness 1990, art. 4.2.6]. On Gergonne see also 1.8 .

(7) In the context of differential equations Liouville introduced the concept of complementary functions in 1832. See [Koppelman 1971, 169].

(8) Murphy's formula has as follows

$(\theta - \theta_1)^{-1} = \theta^{-1} + (\theta - \theta_1)^{-1} \theta^{-1} + \dots + (\theta - \theta_1)^{n-1} \theta^{-1} + (\theta - \theta_1)^n (\theta - \theta_1)^{-1} (\theta - \theta_1)^{-1}$  [see 3.3].

(9) Other analysts outside France, such as Lorgna and Tortolini in Italy and Gruson and Kramp in Germany, contributed in the development of the calculus of operations at that time. Their work had no impact, however, on English analysts. See [Lacroix 1819, 731; Koppelman 1971, 160-1; 172-3; Pincherle 1899, 14; 1912, 4]. In none of these references do we find any comments on either Sarrus or Schmidten. We would like to refer, though to a relevant footnote in [Grattan-Guinness 1990, art. 11.5.6].

## Section 1.7

(1) It took, in fact, over a century for what we now call the "Laplace transform" to feature prominently as a method in analysis. See [Deakin 1981, 352-360; Grattan-Guinness 1990, art. 9.3.4]. On the solution of (17.1) on



- lines similar to Laplace's see [Boole 1877, 461-463].
- (2) On Laplace's functional solution (17.12) see also [Grattan-Guinness 1972, 446-7; 1990, art. 9.3.2].
- (3) On (17.13) see also [Deakin 1981, 358; Grattan-Guinness 1990, art. 9.3.4].
- (4) Despite his earlier acknowledgements to Lagrange [1.5], Laplace does not mention him in this memoir [1811] in the course of his historical review.
- (5) Fourier had submitted a paper in 1807 which aroused controversy. A revised version of it in 1811 won the award proposed for research on the diffusion of heat. For details on Fourier's early work see [Grattan-Guinness 1972; 1990, chapter 9].
- (6) On Fourier's solution of the heat diffusion equation and on his representation of an arbitrary function in trigonometric series see [Grattan-Guinness 1972, 447-450; 1990, art. 9.2.5-9.2.7; 9.3.2; Kline 1972, 675-681].
- (7) At page 518 of article 401 Fourier omitted  $\phi(t)$  in the right-hand side of (17.30).
- (8) See [Boole 1877, 478-80]. See also R.L.Ellis [1845] and Gregory [1865, 57; 184, 504].
- (9) On Poisson's operator method for the solution of (17.32) see art. 10.4.2 in [Grattan-Guinness 1990]. On his earlier work on the integration of the heat-diffusion equation (17.16), (17.26) see [ibid, art. 9.3.2].
- (10) For historical record I would like to refer here to another paper which dealt with the earth figure equation in a transformed form by [Lebesgue 1846]. Lebesgue was based on [Liouville 1841]. He worked on lines fairly close to those followed by English and Irish analysts of that time, but his approach was independent from them. Lebesgue was mentioned only by Glaisher [see references in 1.4, (4) and in 4.8].
- (11) On Brisson's unpublished work see [Cauchy 1827, 199; Grattan-Guinness 1990, art. 11.5.6; Koppelman 1971, 166-7].
- (12) On the importance of Cauchy's transform (17.43) see also [Deakin 1981, 364-6; Grattan-Guinness 1990, art. 10.3.6, 11.5.6].
- (13) On Cauchy's residual calculus see comments in [Cooper 1952, 9-10; Grattan-Guinness 1990, art. 11.5.8; Koppelman 1971, 172].
- (14) See [Cooper 1952, 8-10; Deakin 1981, 369-371; Fourier 1822, 561; Koppelman 1971, 167-170, 182-3; Pincherle 1899, 15-16; 1912, 11-12].

## Section 1.8

- (1) The educational programs of Laplace and Lagrange come in contrast with that of Monge. A comparative study of these programs is included in [Glas 1986, 253-261]. On the education at French institutions in the 1800's see

[Grattan-Guinness 1990, chap. 2]. On the role of Lagrange and Laplace see [ibid, art. 2.5.1-2.5.2].

(2) In what follows below in text, fragments of information on the work of these people are taken from the Encyclopedia of philosophy, but no specific references will be given. A most useful article consulted is the one on the history of "Semantics" in the 4<sup>th</sup> volume. At certain instances the Dictionary of Scientific Biography is also used.

(3) An interesting discussion of the "holly horror of the infinite" and the foundations of the calculus is included in Gratry's Logique [1855] studied in 8.9 as [1944]. See 8.9 and [1944, 424-26]. For an account of the different methods in the calculus prevailing in late 18<sup>th</sup> century and the debates over its foundations see [Grabiner 1981b, 23-37].

(4) On Lagrange's work in the 1790's and on the prize contest of 1786 see [Grabiner 1981b, 37-46; Grattan-Guinness 1990, art. 3.2.1-3.2.6].

(5) An extensive account of Carnot's paper of 1786 and the two editions of it as a book in 1797 and 1813 is presented in [Youschkevitch 1971]. On the prize contest see [ibid, 155-9].

(6) Most of the information on Condillac provided in this paragraph was communicated to me in a letter by professor Dhombres. See also his [1983, 197, 212]. On the distribution of Logique to students of the Ecole Normale see [Glas 1986, 250; Grattan-Guinness 1988b, 73].

(7) On Condillac's social background, his influence from Locke and his early work see [Albury 1980, 7-29; Dan Badareu 1968, 337-345; Knight 1968].

(8) See [Dan Badareu 1968, 350-1]. I have recently come across a translation of Logique in Greek published in Vienna in 1801. It is of interest to notice an appendix on syllogistic logic added by the translator at the end of the book after a friend's suggestion. In what follows in text I will cite from Albury's translation of Logique dated as [1980].

(9) References on the linguistic tradition of the Port-Royal logic will be given in 6.2.

(10) For further discussion on Condillac's method of analysis as in his Logique see [Albury 1980, 16-23; Baker 1975, 110-12; Dan Badareu 1968, 340-4. 350-1].

(11) See (6) above. In Dhombres [1983] a comparison is drawn between Condillac's La langue de calculs and the work of Clairaut and Euler in algebra.

(12) On Condillac's inadequate mathematical background and on the failure of his method in higher algebra see (6) and (11) above. In what follows in text, La langue will be cited as [1948], the date of the edition of Condillac's collected works.

- (13) Further on the issues of analogy, clarity of signs, abstraction and natural generalization (induction) in Condillac's work see [Dan Badareu 1968, 348-355; Dhombres 1983 -particularly page 217].
- (14) This is particularly evident in the introductory chapter of La langue [1948, 421-469].
- (15) On Condillac's influence on chemistry -particularly on Lavoisier- see [Albury 1980, 26; Dan Badareu 1968, 356; Dagognet 1969, 41-8, 217-9]; in logic -particularly on Lambert- see [Dan Badareu 1968, 354; Dumitriu 1977, 332-3]; in anatomy, physiology and zoology see [Albury 1980, 28; Babbage 1827, 374]; in crystallography [Lacroix 1828, 23] and on political sciences [Baker 1975, 116; Dahan 1986, 104].
- (16) The only English scientist to follow closely the Ideologues and particularly Tracy's universal grammar, was J. Bentham (1748-1832), the uncle of G. Bentham [see (2) above].
- (17) On the Ideologues see (2) above and [Albury 1980, 25-8; Baker 1975, 112, 116; Dan Badareu 1968, 338, 355-8; Dahan 1986, 103-4].
- (18) See Degerando [1800, Tom II, 196]. Further on his discussion on the brevity of algebraic language see [ibid, 214].
- (19) The project of a universal language dates in fact earlier than Leibniz. See [Bochenski 1961; Nidditch 1962; Rider 1980, chap. 9; Styazhkin 1969, 56-80]. On Condorcet's comparison with Leibniz see [Baker 1975, 127; Granger 1954, 198-9; Rider 1980, 194-5].
- (20) A link between probability and logic is perceived in De Morgan and Boole independent of any semiotic influence. This subject will not be pursued in this thesis. Pertinent references will be provided where necessary.
- (21) On Lacroix's Traite see [Grattan-Guinness 1990, art. 3.2.7; Hodgkin 1981, 65-8].
- (22) On the reception of Lacroix's textbooks on the calculus by the English see [2.1, (2); 2.3, (9)].
- (23) On the origins and impact of Lacroix's Essai see [Grattan-Guinness 1990, art. 3.2.5; Hodgkin 1981, 64].
- (24) On the proximity between Condillac and Clairaut see reference in (11) above.
- (25) On Annales see [Dahan 1986, 98-9; 113-4; 1.6, (6) ].
- (26) Euler's mathematical work is regarded as outstanding for its clarity, rigor and brevity in Gergonne [1817]. For more on Gergonne's papers on the philosophy of mathematics and on the Euler diagrams see [Dahan 1986, 106-110; Grattan-Guinness 1988, 73; 1990, art. 3.2.5; Styazhkin 1969, 138-140].
- (27) On the status of algebra as a language in the period shortly before Cauchy's reform, see some further comments in [Dahan 1986, 125-6; Glas 1986,

266-268]. According to Dhombres [1983, 212] there is a probable, though ambiguous, possibility that Condillac was an inspire of the work of Cauchy, Abel and Bolzano on the etymology of mathematics. Some interesting remarks on the consequent inspiration of Abel's and Cauchy's enquiries in the paradoxes of analysis for the development of the philosophy of science by Gratry in 1855 will be included in 8.9.

## Chapter 2

### Section 2.1

- (1) The title of this journal is Memoirs of the Analytical Society, for the year 1813, hereafter cited as Memoirs [1813]. On the origins and activities of the shortlived society see [Enros 1979, chap. 4; 1983]. Further information will be given in 2.3.
- (2) On the introduction of "Analytics" in Cambridge University in the mid 1810's see [Becher 1980b, 8-14; Enros 1979, chap. 6; 1981; Guicciardini 1989, art. 9.5; Koppelman 1971, 178-183].

### Section 2.2

- (1) Guicciardini's thesis [1987] -published as a book [1989]- is an excellent study of 18<sup>th</sup> century fluxional calculus in England. The reforms in these little-known military academies are extensively studied in his [1989, chap.8].
- (2) On Woodhouse [1803] see [Becher 1980b, 8-11; Dubbey 1963; Guicciardini 1989, art. 9.2; Koppelman 1971, 176-9].
- (3) According to Pycior [1984, 431-5] Woodhouse's formalist approach, as in his [1803], influenced considerably Babbage's and Peacock's algebraic style. However, Becher claims that [1803] "was an elementary textbook which was neither lucid nor a satisfactory introduction to higher analysis or current research" [1980a, 398]. In any case, it was via Woodhouse [1803] that Babbage came across differential notation in the early 1810's [Koppelman 1971, 178].
- (4) Laplace's procedure is presented for example in his [1818, 210-11]. References to his Mécanique are traced in [1818, 93, 95, 98, 140, 208, 261, 271, 400, 406]. Woodhouse [1821] however, presents no references to Laplace's procedures.
- (5) On the Repository and on Wallace's contributions to it and to encyclopedias see [Guicciardini 1989, 103-4, 115-6; Panteki 1987, 125, 128 fn 26]. Particularly on Wallace's letter to Peacock, in which he complained for being ignored by the latter, see [Panteki 1987].
- (6) This quotation is from [Spence 1820, 269], Herschel's edition of

Spence's essays.

(7) Formula (22.7) was proposed for solution by Ivory in the Repository in 1806. Further on the introduction of the differential notation by Wallace and Ivory in that journal see [Panteki 1987, 121, 128, fn 18].

(8) On the reform at Dublin University see [Grattan-Guinness 1988a; Guicciardini 1989, 131-135].

(9) Brinkley's and Spence's [see (6) above] ambivalence between fluxional and differential notation refutes somewhat the mid-20<sup>th</sup>-century belief that British analysts were divided in two categories, either for or against Newton's notation. The introduction of analytics in early 19<sup>th</sup> century Britain was not simply a transition from "Newtonian dot notation and synthetic methods to the Continental differential notation and analytic methods" [Enros 1981, 135]. Dubbey [1963, 37-42] expresses the standard view of that time by stressing the merits of the differential notation and overlooking other factors. With [Grattan-Guinness 1979, 82-3] a new approach appeared on that subject.

(10) "This demonstration", wrote Herschel, "is in substance the same with that given by Dr. Brinkley" [1816, 488]. In fact the only differences lay in the substitution of 1 for h in (22.11) and the ability to use the method of separation of symbols -which was not possible in Brinkley's fluxional notation. The latter's procedure in [1807, 117-8] has as follows: Taylor's theorem was written in the form

$$(i) \quad {}_n u = u + \frac{\dot{u}}{x} nh + \frac{\ddot{u}}{x^2} \frac{n^2}{2} h^2 + \dots = u + e^{\frac{\dot{u}}{x} nh} - 1$$

$$\text{where } \frac{\dot{u}}{x} = \frac{du(x)}{dx}, \quad \frac{n^2}{2} = \frac{n^2}{2!} \text{ etc and } {}_n u = u(x+nh).$$

Putting  $m=n-1, \dots, 1$  in (i) Brinkley obtained the values of  ${}_m u$  and substituted them in the formula

$$(ii) \quad \Delta^n u = {}_n u - \frac{n}{1} {}_{n-1} u + \frac{n(n-1)}{1.2} {}_{n-2} u - \dots$$

Then, by simple considerations, Lagrange's theorem was obtained in the form

$$(iii) \quad \Delta^n u = \left[ e^{\frac{\dot{u}}{x} h} - 1 \right]^n$$

(11) The quotation is from the letter [H.S., 20: 5, 8 February 1813]. On the citation from the Herschel-Babbage correspondence see [2.3, (7)].

(12) Formula (22.18) here stands for  $n < 0$ . Let  $n > 0$ . Then Herschel proved prior to (22.18) in his [1820, 70, 82] that

$$\frac{1}{1 \dots (x+n)} \left[ \frac{\log(1+\Delta)}{\Delta} \right]^n 0^{n+x} = \frac{1}{1 \dots x} \Delta^{-x} 0^x. \text{ Thus the } \Delta^x 0^x \text{ numbers are defined for}$$

$x \leq 0$ . For further improvements on Brinkley's work see 2.3.

(13) Plenty of applications of the  $\Delta^x 0^x$  numbers are to be found in Herschel [1820] --see 2.3-- and De Morgan [1842c]. In fact, these numbers were used up to the 1860's --see Boole [1860, 19-25]. For later research in the calculus of operations --as based on these numbers-- see the Quarterly Journal of Pure and Applied Mathematics for the years 1860-1866.

### Section 2.3

(1) On the poor reception of the Memoirs see [Enros 1983, 37]. On Babbage's disappointment on the lack of a review of this journal see his letter to Herschel [H.S. 2: 25, 1 August 1814]. On the Babbage-Herschel correspondence see (7) below.

(2) On the avoidance to refer to their papers in the Memoirs in later publications see particularly Babbage's letter [H.S. 2: 46, 22(?) Nov. 1815].

(3) Few instances only from the single copy of this journal are recorded in [Enros 1979, chap 4; 1983, 34-39; Koppelman 1971, 181-184].

(4) This inscription, in De Morgan's handwriting, is from the copy of Memoirs with code number L<sub>3</sub> [Anal. Soc.] kept in the London University Library. Another copy of the Memoirs is kept in the British Museum Library. De Morgan referred to the Memoirs in his [1836] (see particularly 2.4).

(5) Babbage was employed about the preface according to Herschel's letter [H.S. 20: 9, 27 June 1813]. See details in (10) below.

(6) On the most important issues stressed in this "Preface" see [1813, v, xi, xvi]. For further comments see references in (3) above.

(7) The Herschel-Babbage correspondence is kept in the Library of the Royal Society in London. Reference to these letters will be given as [H.S. A:B Date], where A stands for the number of the volume and B for the number of the letter. Volume 2 contains the original letters of both men, whereas volume 20 includes only copies of Herschel's letters in legible form. Herschel was

very systematic in keeping records of his correspondence which he valued highly. The most revealing letter on this issue is his [H.S. 20:41, 3 April 1817] extracts of which are quoted in [2.4, (13)].

(8) Herschel was particularly enthusiastic about the role of Memoirs and his [1813b] on finite differences. In his [H.S. 20:13, [Oct. 13 1813]] he wrote: "I never yet in the course of my Math.<sup>1</sup> reading, saw anything so beautifully executed as the complicated formulae in my paper on finite diff.ces. Upon my word this work will do the Soc.<sup>y</sup> great credit-- I shewed Peacock's copy of your

Mem. to Ivory, Wallace and Leybourn at the Military Coll. Sandhurst. They all declared that they never saw its equal in typography". Further on Babbage's and Herschel's memoirs written for this journal see 2.4.

(9) On Herschel's suggestion to revive the Society by asking Ivory and Wallace to present papers see his letter [H.S, 20:38, 30 Jan. 1817]. Apparently the revival never took place. On the importance of the Lacroix translation see Herschel's letter [H.S, 20:35, 14 July 1816] extracts of which will be quoted in 2.7. Herschel was keen in obtaining the third volume of Lacroix's Traite [see 1.8] for his personal studies [H.S, 20:10, 25 July 1813].

(10) Herschel wrote in his [H.S, 20:7, 25 Febr. 1813]: "Perhaps it would on the whole carry a better appearance in the first number or two, to conceal our names. We may thus (a weighty consideration) be enabled to give a greater number of Memoirs than we otherwise could". But in his [H.S, 20:9, 27 June 1813] he insisted that something should be written in the "Preface" about the Analytical Society and its aims: "Is it candid, nay is it politics to keep the public quite in the dark as to this point? Place yourself in the situation of one of our readers (....) would you not naturally say who are these people? [...] what are their resources, their views, their expectations? If we pass over all notice of this, consider how open we are to ridicule in the character of a few unknown Quixotic individuals who take upon themselves to enlighten the world... ". And Herschel went on to tell Babbage what he should write about the objectives of the Analytical Society.

(11) For example, by means of the second method, it was proved that  $u_n = \Delta^{n+1} u_0$

and further that  $u_{n+1} = u_n(1 + \frac{n+1}{1}) - \frac{(n+1)n}{1.2} u_{n-1} + \frac{(n+1)n(n-1)}{1.2.3} u_{n-2} - \dots$

Since by definition  $u_0 = e$ , (23.5) follows readily from the above [1813a, 61].

(12) In details:  $e^x = 1 + \frac{e^x}{1} + \frac{e^{2x}}{1.2} + \dots =$

$$= 1 + \frac{1}{1} [1 + \frac{x}{1} + \frac{x^2}{1.2} + \dots] + \frac{1}{1.2} [1 + \frac{2x}{1} + \frac{2^2 x^2}{1.2} + \dots] + \frac{1}{1.2.3} [1 + \frac{3x}{1} + \frac{3^2 x^2}{1.2} + \dots] + \dots =$$

$$= [1 + \frac{1}{1} + \frac{1}{1.2} + \dots] + \frac{x}{1} [1 + \frac{2^1}{1.2} + \frac{3^1}{1.2.3} + \dots] + \frac{x^2}{1.2} [1 + \frac{2^2}{1.2} + \frac{3^2}{1.2.3} + \dots] + \dots,$$

hence (23.6).

(13) He wrote: "The sum of the series  $u_n = 1 + \frac{2^n}{1} + \frac{3^n}{1.2} + \dots$  may be found also by considering that the generating function of  $u_n/(1.2\dots n)$  is  $e^t$  for we have

$$e^t = 1 + \frac{e^t}{1} + \frac{e^{2t}}{1.2} + \dots = [1 + \frac{1}{1} + \frac{1}{1.2} + \dots] + t[1 + \frac{1}{1} + \frac{1}{1.2} + \dots] + \frac{t^2}{1.2}[1 + \frac{2^2}{1} + \frac{3^2}{1.2} + \dots] + \dots$$

thus  $u_n = 1.2.3 \dots n$  x the coefficient of  $t^n$  in the development of  $e^t$ , hence (23.8). The error lies in that the coefficient of  $t^2/(1.2)$  equals  $u_1$  and not  $u_2$ , as Herschel implied in his [1813a, 62]. But in his [H.S, 20:36, 10 Oct. 1816] he wrote "Did you ever try to expand  $e^t$ , the coefficient of  $t^x$ , is evi-

dently enough  $\frac{1}{1.2 \dots x} \{ \frac{1^x}{1} + \frac{2^x}{1.2} + \frac{3^x}{1.2.3} + \dots \}$ ", which is the right result. In

other words, the coefficient of  $t^n/n!$  is  $u_{n-1}$ , hence (23.6).

(14) In his [H.S, 20:8, 4 May 1813] he wrote to Babbage: "At length I have set to work in good earnest on the *Mécanique Céleste* with which I am more charmed every day, tho' of course my progress is very slow as I wish to be as systematic as possible in my reading". In his next letter [H.S, 20:9, 27 June 1813] he wrote to Babbage that he drew on volume 4 (page 254) of that work for his paper on finite differences [1813b] and advised Babbage to go through it. One more letter followed [H.S, 20:10, 25 July 1813] in which he posed several questions to Babbage on French mathematical papers, <sup>in</sup> particular as to where to find the solution of the equation

$$(i) \quad u_{x+1}u_x - a(u_{x+1}+u_x) + b = 0.$$

(15) Babbage's answer—a full length direct response to Herschel's [20:10, 25 July 1813], including reference for equation (i), quoted in (14) above—has code number [H.S, 2:15, 30 June 1813]. The date is in Babbage's hand-writing but according to (14) above, the proper date must be "30 July 1813".

(16) "Why not put "Divers points d'Analyse"? " wrote Babbage, adding "What the Devil did you choose such a title". The undated letter has code number [2:32], and is a direct answer to Herschel's [2:31 or 20:20, 25 Oct. 1814]. Its most probable date is in November 1814. In fact Babbage's next letter is [H.S, 2:34, 12 Dec. 1814].

(17) Herschel's obscure notation was criticized by Babbage as inconvenient, and a "committee of notation" was proposed by him in his [H.S, 2:80, 12 May 1817]. In fact, Herschel drew considerably on Arbogast's notation and rules. He became acquainted with the latter's [1800] in 1813. See his letter [H.S, 20:10, 25 July 1813]. On the calculus of derivations see also 2.8.

(18) See Laplace's definition (15.29). In fact, Herschel regarded  $x$  as ranging from  $-\infty$  to  $+\infty$  instead from 0 to  $+\infty$  as Laplace did.

(19) We omit this theorem here as it is rather complicated and of no further use in Herschel's work. However, we refer to it in 3.3 as its form bears a close proximity to that of Murphy's theorem (33.3). Among other results



deduced was Laplace's interpolation formula (15.37). However, it is of interest to note that Herschel's procedures and symbolism in [1814] differed considerably from those followed by Laplace. In Lacroix [1816, 472-477] Herschel exhibited the main results of the calculus of generating functions on lines close to those of Laplace [1.5]. The theorems demonstrated were rather elementary in comparison with his [1814] and served above all to illustrate the analogy between the process of substitution and the elevation of a binomial to its powers [ibid, 477].

(20) Lacroix only mentioned in an addition at the end of the third volume of his treatise on the calculus that Brinkley and Herschel had provided formulae for the development of (23.17) [1819, 732].

(21) On the presentation of Herschel's theorem and its applications see [Boole 1860, 14-25; De Morgan 1842c, 307-311; Hymers 1858, 10-26; Pearson 1850, 8-30]. In these references elements and few applications of the theory of generating functions are included. The role of Lagrange's and Herschel's theorems will be illustrated in our study of the development of symbolical methods in England in chapters 3-5.

## Section 2.4

(1) Babbage and Herschel very often omitted parentheses, such as in the case of the continued product in text above. In what follows in text, the only change in their notation will be the addition of parentheses, e.g, we will write  $\psi(x)$  instead of  $\psi x$ . However, for convenience, this change will be only temporary [2.4-2.5] and the existence of parentheses will be obvious from the context in later sections [2.6-2.7].

(2) As will be evident throughout the chapter, Herschel used principally Laplace's method for the treatment of functional equations. He became acquainted with Monge's work in the summer of 1813. Having had access to volumes 1,3,4 and 5 of the Mélanges de Turin he wrote to Babbage: "As for the Monge's paper in Vol 5 it contained nothing worth looking at (except the constructions) on the determination of functions and I confess myself much disappointed in finding nothing of any use to me there. The chief information I have obtained is from one of his in the Sav. Etran: 1773 [Most probably Monge 1776 cited in 1.4 where Monge reduced functional equations to finite difference ones; H.S, 20:11, 16 Aug. 1813]. In his [H.S, 20:28, [6 Nov. 1815]] he wrote to Babbage: "You say [Babbage 1815, 393] it is to Monge that we are indebted for the most general view of the subject, and I think with great propriety, for Laplace only overcame the difficulty which bounded Monge's progress, but not his view. For although he could not surmount the obstacle, he must be allowed to have seen very distinctly and generally, what lay beyond

it". Thus, both analysts acknowledged Monge as the first French analyst to offer a general view of functional equations, a point missed altogether in [Dhombres 1986; see 1.4].

(3) Let, for example,  $n=2$  and replace  $x$  by  $A_2^{-1}(x)$ . Then  $\varphi A_1 A_2(x) = B(x) \longrightarrow \varphi A_1 A_2(A_2^{-1}(x)) = B(A_2^{-1}(x)) \longrightarrow \varphi A_1(x) = B(A_2^{-1}(x))$ . Substituting next  $A_1^{-1}(x)$  for  $x$  it follows that  $\varphi(x) = B A_2^{-1} A_1^{-1}(x)$ . By induction we have (24.17). On Herschel's notation for the inverse function see also his letter [H.S. 20:42, 16 April 1817].

(4) In his [H.S. 20:9, 27 June 1813] he wrote to Babbage: "My paper on Differences improves daily under my hands in sweeping generality and horrible complication". See also [2.3, (8)].

(5) The procedure sketched above was included in details in his letter [H.S. 20:10, 25 July 1813]. He called his method "unexceptionable" and such that affords the integration of (24.15) to be "performed in all its generality, granting the perfection of the ordinary analysis, and that of finite, partial differences."

(6) Herschel went on in that letter: "Conceive what a bore! Thank god, however, the Mem. Anal. Soc. has had but little sale. Do you not think I ought to buy up all the remaining copies?". Thus, the poor circulation of the Memoirs was to some extent an advantage, particularly for Herschel. Nevertheless, as we shall see in the end of this section [(13)], he was to complain for not being acknowledged in Babbage's work for his [1813b]. In fact —see (26.14)— his general method was still valid for certain cases, as he was happy to discover in 1817 [H.S. 20:47].

(7) De Morgan dealt extensively with Herschel's method —as in [1813b]— in his [1836, art. 252–259]. Amending what was overlooked by Herschel, he illustrated the new approach by solving the equation " $\varphi x + \varphi 2x = \varphi 3x$ ". However, De Morgan's account is not much more lucid than that by Herschel, and it will not be discussed any further. On the citation from De Morgan's treatise [1836] see 3.5.

(8) Herschel was extremely proud and confident of his "unexceptional" method at that time. He wrote at the end of that letter: "I shall work hard at the subject, for I feel confident that I can solve even functional equations involving  $F, F^2, \dots, F^n$  and the resulting equation of differences will be partial, and of the  $n^{\text{th}}$  order, involving as many variables as the original equation has terms of the form  $f F^n \varphi(x)$  in which  $\varphi(x)$  is different,  $-1$ . And I shall endeavour to apply the same theory to many different functions of many variables". Judging from our study in 2.3, 2.4 and the above quotation, we see that it was Herschel who first showed a remarkable tendency for abstraction and generalization. See also (4) and (5) above, as well as, his work in 2.8.

(9) Herschel first got involved with functions relative to more than one variable in his [H.S, 20:11, 16 Aug. 1813]. In this letter he wrote: "To take a simple instance, suppose

$$\varphi(x,y) + A_{x,y}.\varphi(x^m,y^n) + B_{x,y}.\varphi(x+a,y^2+y) + C_{x,y} = 0$$

[... ] I have some idea of a calculus of partial functions which I mean to bestow some thought upon.". He added that, contrary to his belief in [20:10] -see (8)-, "I was mistaken however in supposing I could integrate functional equations of superior order to the first".

(10) Herschel's observation gave rise to the consideration of fractional differentiation briefly referred to in [1.7, (14)]. Babbage further considered fractional indices in his [1816, 249]. See further [Koppelman 1971, 182-3]. On his application of the method of separation see also 2.7.

(11) On Babbage's paper on mixed differences see [Enros 1979, 168; 1983, 33]. The reasoning mentioned in text was included in Babbage's letter [H.S, 2:41, 25 Sept. 1815] and was further illustrated in his [1816, 253-6]. See also (13) below and (25.49)-(25.50).

(12) Babbage's treatise exists in the Buxton MSS.13 in the History of Science Museum, Oxford [see Dubbey 1978, 51-2; Enros 1979, 187]. On Babbage's letters to Herschel on this topic see [H.S, 2:75, 76].

(13) But earlier than that, Herschel complained to Babbage for not being acknowledged in the latter's [1815] for his own work as in his [1813b]. In his [H.S, 20:28, [Nov. 6 1815]] he wrote: "2<sup>nd</sup>." Eq<sup>ns</sup> of the 2<sup>nd</sup> and higher orders have never been even mentioned [Babbage 1815, 391]". Now just look into the following publications Mem. Analyt. Soc. 1813 page 111, 112, 113, ( a work published by a Society of anonymous authors at Cambridge) where these Eq<sup>ns</sup> are expressly treated of, your notation  $\varphi^{m,n} \text{ etc}(x,y,\dots \text{etc})$  explained and the reasoning about the arbitrary constants and their numbers given [... ]. Also into a paper publ<sup>d</sup> by J.F.W.H. in Phil. Trans. 1814. ii. where the complete sol<sup>n</sup>. of  $\varphi^2(x) = x$  (w<sup>h</sup>. I am proud to acknowledge was suggested by your method of solving the problem about the normal) and particular sol<sup>ns</sup> of  $\dots \varphi^n(x)=f(x)\dots$  are given, and the fallacy of the reasoning of the aforesaid anonymous writer about the Number of arbitrary constants noticed. Now in this state of things you could hardly say that function. Eq<sup>ns</sup> of Superior orders had never been mentioned". With his [H.S, 20:41, 3 April 1817] Herschel sent all Babbage's letters back begging him not to lose them "as I prize them greatly. Should you have mislaid any of mine on mathematical subjects and want to refer to them I can furnish you with copies of them as I keep a great MS. of my mathematical correspondence. [... ]. I am not sorry to hear that you are busy about settling the early history of the theory of functional Equations. It is good to prevent the possibility of future misconceptions". This

letter ended with a postscript saying: "(I wish you would date your letters)".

## Section 2.5

(1) Both techniques were illustrated with equations from Babbage's Examples [1820] in Gergonne [1822, 73-83]. Babbage's name in late 19<sup>th</sup> and early 20<sup>th</sup> centuries is mostly linked with the transform (25.1). See for example [Laurent 1890, 243; Pincherle 1912, 51-2]. In recent researches Babbage's work on functional equations is omitted in Dhombres [1986] and Koppelman [1971]. A study of Babbage's mathematical work is carried out by Dubbey [1978]; this work should be read with care due to misprints, omissions or other errors transferred unnoticed from Babbage's papers. On (25.1) see also (3) below.

(2) The letter has code number [H.S., 20:6]. It is dated as "1814" in the Royal Society catalogue but its most probable date is "Febr. 1813". This letter does not belong to the volume 2 of all the surviving manuscript letters by Herschel and Babbage [see also 2.3, (7)].

(3) Maule's letter dates 12 May 1814 [see Enros 1979, 170]. W.H Maule (1788-1858) was a close friend of Babbage, acknowledged for his help in the latter's [1864, 435]. See also [Enros 1983, 33-34]. On Babbage's reference to Maule's solution (25.4) see [2.6, (5)].

(4) This letter has code number [H.S., 2:27]. It bears ~~some~~ <sup>the</sup> ~~date~~ <sup>head</sup> as to the month it was written, but according to some further research its definite date must be around 22 September 1814 [see also Enros 1979, 170 fn 5].

(5) For Herschel's approval of Babbage's statement see [2.4, (2)].

(6) The generality of the solution of a functional equation was primarily discussed in [Herschel 1814, 460-1]. He further delved into this matter in his letters to Babbage, whereas the latter studied it in his [1815, 408-9; 1816, 229-30; 1817, 210]. For further comments on this matter see [De Morgan 1836, art. 259; Dubbey 1978, 59-60].

(7) Babbage introduced his method for the problem (25.5), the concept of a symmetrical function as well as the above example (for  $a = 1$ ) in his [H.S., 2:27, 22 Sept. 1814 - see (4) above].

(8) It is surprising that scarcely any example corresponding to the theory sketched in text for the problems (25.5) up to (25.33) was included in his Examples [1820]. As the constructed examples in his [1815] present very little interest, we have included in our study as few as possible. The reader can further consult Dubbey [1978, 55-58].

(9) The issue of analogy from algebra or from the integral calculus is apparent in Babbage's early paper. See also the reduction of (25.25) to the form (25.26) in text below. This issue will be further discussed in 2.6-2.7.

(10) Babbage was proud to stress that (25.24) was not a constructed example

but one proposed by Herschel. In fact, equation  $\varphi(e^y) - \varphi(e^{-y}) = A(y)$  was solved on parallel lines giving  $\varphi(y) = f(y) + \varphi((\log y)^{2n})$ . Herschel had suggested to Babbage this equation for solution in his [H.S, 20:17, 4 Aug. 1814]. However, he was not satisfied with Babbage's procedure and wrote "the thing is really hardly worth the trouble" [H.S, 20:28, 6 Nov. 1815]. Herschel corrected a printing mistake —still existing— in [1815, 400] where a " $\psi$ " is missing in (25.24).

(11) Equation (25.32) is given in Babbage's notation which is slightly changed in the rest of this section only so far as addition of parenthesis is concerned. See also [2.4, (1)]. As it is fairly obvious from the context I omit to repeat in text that any functions denoted by  $F, \alpha, \beta, \dots, v$  or other Greek letters stand for known functions.

(12) For a detailed discussion of his procedure see [Dubbey 1978, 57–60].

(13) See [Boole 1860, 223–4; Gergonne 1821; De Morgan 1836, art. 134–9, 192–7]. While De Morgan undertook a scholastic study of the general case (25.36), Boole confined to the case (25.38) and to the example (25.41).

(14) For  $\varphi(x)$  can not be possibly determined from (25.45). And if we assume that it can be arbitrary, then it follows that  $1 = (a - \alpha(x)) \cdot (a - \alpha^2(x))$  which is not necessarily true.

(15) As for example  $\varphi(1/x) = d\varphi x/dx$  or  $\varphi(a-x) = d\varphi x/dx$ . All these examples are reproduced in detail in [Dubbey 1978, 70–72]. Example (25.46) is included also in [De Morgan 1836, art. 216].

(16) The general problem of the normal has as follows: It is required the nature of a curve so that the square of any normal exceeds the square of the ordinate drawn from its foot by a certain quantity  $a$ . This "celebrated" problem was proposed by Euler in the Petersburg memoirs [Babbage 1815, 252; Ellis 1843b, 137]. According to Ellis a solution of it by Poisson was presented in volume 3 of Lacroix's *Traité*. Babbage, in fact, confined to the case of  $a = 0$  [1815, 392; 1816, 253–6]. There is a misprint in [1816, 253] at the right-hand side of (25.49) where " $\psi$ " is missing in " $d\varphi x$ ".

(17) The differential equation to which (25.49) was reduced is  $y^2 = xydy/dx + \varphi(ydy/dx)$ , where  $y = \varphi(x)$ .

According to De Morgan, "The connection between differential and functional equations might be advantageously examined by means of such equations, but we have already gone beyond the limits which we had prescribed to ourselves in setting out" [1836, art. 328]. Ellis wrote <sup>a</sup>few brief papers on functional equations, interested particularly in the analogy which De Morgan mentioned above but most probably independently of him. In his [1843b] Ellis focused on equation (25.49), commenting upon Babbage's procedure at page 137 by saying "This procedure appears to have been suggested by an incorrect analogy with

the way in which arbitrary functions are introduced into partial differential equations". But for reasons mentioned in text and in (6) above, we will not delve any further in this matter.

(18) The author of the review was probably P. Barlow (1776–1862). For details see [Enros 1979, 172–174].

## Section 2.6

(1) As Babbage's manuscript letters are far from legible, at certain instances I am based primarily on Herschel's letters. The most important issues of their correspondence are pointed out in the course of this chapter, but a further study of their letters can form the basis of a more detailed work in the future. I would like to add that at many instances Babbage would often discover his fallacies before the arrival of Herschel's answer. Thus it is hard to justly estimate the degree of Herschel's influence in this sense.

(2) See Babbage's letter [H.S, 2:15, 30 July 1813]. On the date of this letter see [2.3, (15)].

(3) It has to be stressed that both analysts ignored Servois's definition of commutative and distributive functions. The study of commutative functions was first tackled by De Morgan in his [1836] –see 3.5–3.8 .

(4) See [(24.23)–(24.29); 2.4, (13) ]. Equation (26.4) was called after Babbage by Gergonne [1822, 80]. See also [Pincherle 1912, 52].

(5) Babbage mentioned that the latter two methods were based upon a "friend's" [Maule] suggestion [1815, 412]. See also [2.5, (3)] and (9) below.

(6) According to Enros [1979, 176] Bromhead was the first English analyst to state around 1815 that an inverse of a function admits of many values [on (26.7) see also 1.6; (16.17), (16.25)]. There is a misprint in [1816, 191] at line 14 from the top where it is written " $\varphi\varphi^{-1}x$ " instead of " $\varphi^{-1}\varphi x$ ". Babbage studied a bit further the definition of  $\varphi^{-1}(x)$  in his [1822] –see 2.9.

(7) Apart from his [1813], Babbage applied Laplace's method only in his letter referred to in (2) above. But he frequently referred to Laplace's method, as for example in his [1815, 413, 417] or in his [H.S, 2:27, 22 Sept. 1814]. However, it is strange that he avoided to give any hint whatsoever on the technique involved in this method in his published essays, particularly as he had omitted to give any reference as to where to find this method.

(8) Notice, that "f" in (25.14) stands now for the function  $f(x) = x$  according to the conditions that followed the formulae (25.10)–(25.14) in 2.5. Typical of Babbage is the fact that such clarifications are missing in his paper.

(9) Notice that in the first application of (26.5) for the solution of (26.4) the function  $f$  –initially regarded as arbitrary– is finally taken to be a particular of (26.4), while in this case it is  $f^2$ , which is a particular

solution of (26.4) in (26.10). The inconsistency of the arbitrariness of  $f$ , which is among the weak points of his theory, in fact puzzled him a lot. See [1815, 417-9]. He went on to apply this method also to the "compound" equation  $\varphi\alpha(y, \psi y) = y$ , a case of (26.17) and (26.21). All the demonstrations discussed so far, including equation (26.12), were incorporated almost identically in his [H.S., 2:27, 22 Sept. 1814]. Among the examples in this letter is the case of  $fx = -x$ , as a particular of (26.4). Hence,  $\psi y = \varphi^{-1}(-\varphi y)$  is a general solution of (26.4). This solution is in fact Maule's (25.4) where  $n=2$ . Notice a printing error in [1815, 412] where instead of  $(-1)^{2/n}$  he wrote  $(-1)^{1/n}$ .

(10) This point was justly raised by Dubbey [1978, 61]. However, Babbage's procedure was followed by De Morgan in [1836, art. 174] for the solution of (26.12). It is strange that neither Herschel nor De Morgan were to comment upon Babbage's slip, and this fact gives us an opportunity to see what seemed rigorous or not to early-19<sup>th</sup> century English analysts. It should be noticed that at page 417 Babbage wrote in (26.16) " $\varphi^1 f^2 x$ " instead of " $\varphi^{-1} f^2 x$ ".

(11) Equation (26.17) was the main object of discussion in many letters between Babbage and Herschel. In his [H.S., 20:47, 1 Aug. 1817] Herschel pointed out a few printing mistakes—such as that at page 420 in (26.17) where instead of  $\varphi\alpha(x, \varphi x)$  Babbage wrote  $\varphi\alpha(x, \psi\beta x)$ —and regarding (16.17) [where  $\psi$  is replaced by  $\varphi$ ] he wrote: "I do not however altogether like that plan: it leads to excessively difficult equations: here is one which (at least in the case of  $a + bx + \alpha\varphi x + \varphi(\alpha + \beta x + \gamma\varphi x) = 0$ ) gives an easier solution. Put  $\alpha(x, \varphi x) = \varphi\alpha x$  then  $\varphi\alpha x = \alpha^{1 \cdot -1}(x, \varphi x)$  and  $\varphi x(x, \varphi x) = \varphi\varphi x = \alpha^{1 \cdot -1}(\varphi x, \varphi^2 x) \dots$

$0 = F(x, \alpha^{1 \cdot -1}(x, \varphi x), \alpha^{1 \cdot -1}(\varphi x, \varphi^2 x))$ . Now in the case in question the equation takes the form  $\varphi^2 x + A\varphi x + Bx + C = 0$  which is easily resolved."

(12) For a further quotation from Babbage's last letter see [2.4 text and (13)]. Since Babbage's erroneous procedure(s), as based upon the substitution  $\psi(x) = u$ , did not affect the rest of his work—as published in his two papers—we will not delve any further into it. The reader can consult the letters referred to above for further information. See also (1) and (11) above. Fallacies were an everyday routine for both of them. When Herschel told Babbage in his [H.S., 20:28, 6 Nov. 1815] that he had been at the point of sending him a false theory in a letter he finally burnt, the latter wrote: "Pray send me your false theory of functions or at least the ground of it. The errors of genius are sometimes more constructive than its brightest trophies. I have five theories for the solution of all functional equations each more plausible than the former and all equally false" [H.S., 2:46, 13 (22?) Nov. 1815].

(13) Another case of a similar nature was pointed out in his [1817]—See (27.40).

(14) On the case (26.54) and the issue of fractional differentiation see also [Dubbey 1978, 74-75; 1.7, (14)]. One of the examples with which he illustrated the case (26.55) was for  $\alpha x = a - x$  and  $\beta x = b - y$  which he sent to Herschel in his [H.S, 2:45, 9 Nov. 1815]. Reducing (26.55) to a partial differential equation which had for integral  $\psi(x,y) = \psi(x+y) + \psi_1(x-y)$ , he went on to find  $\psi$  and  $\psi_1$  so as (26.55) is satisfied [1816, 250-1; 1820, 32-34].

(15) On De Morgan's treatment of Babbage's most representative cases, such (26.7), (26.17), (26.23) and (26.44) see respectively [1836, art. 156, 173, 308, 320]. De Morgan's article will be studied in chapter 3 but we will not comment upon Babbage's work any further there.

## Section 2.7

(1) My explanations are inserted in square brackets unless otherwise indicated.

(2) We remind the reader that, as we showed in [1.5-1.6; 1.8], the observation of the analogy between indices of differentiation and exponentiation had led in fact to the method of separation of symbols advocated by Arbogast and further extended by Herschel. It is for this reason that we include a brief commentary upon the views of Babbage and Herschel on this latter issue.

(3) Bromhead communicated this information to Babbage. See his [H.S, 2:65, 20 July 1816] and [Dubbey 1978, 62-63]. In the latter reference we see quotations from Ivory's letter dated 21 February 1816, where he commented upon the solution of the equations (27.1) for  $n = 2$  and (27.15).

(4) However, in some of the letters he wrote around 1815-1816 we see him writing " $f^2 = 1$ " after " $f^2x = x$ ". But such are the only instances where Babbage separated  $f$  from the variable  $x$ .

(5) Babbage's reasoning is not very clearly presented in his [1817]. The same equation is treated in his [1820, 21]. A detailed discussion of (27.17) is carried out in [Dubbey 1978, 77-79]. Dubbey points out Babbage's erroneous procedure in the case where  $\alpha x = -x$  and  $fx = f_1x = 1$ . Though the final solution is correct, there is an "extraordinary cancellation of errors" [ibid, 79].

(6) For comments on the evaluation of the double-limit of the right-hand side of (27.24) see [Dubbey 1978, 84-86].

(7) A full account of Babbage's solution of (27.25) is included in the reference given above in (6).

(8) This quotation is from Herschel's letter [H.S, 20:43, May ? 15 1817]. The word "very" in square brackets was written but crossed. Many letters have "?" as far as the correct date is concerned. I have provided detailed information in cases where the accurate date is crucial for the reader. However, in cases such as the one in text, of importance is the code number which will al-



ways lead the reader to the exact letter under discussion.

(9) On the equations quoted by Herschel see [Babbage 1817, 202]. We will not delve into a detailed discussion of the generality of Babbage's solutions. See comments and useful references in [2.5, (6)].

(10) Another analogy from partial differential equations is implicitly apparent in the case of the solution of (26.31). See also [Enros 1979, 178].

(11) Babbage's presentation of example (27.34) is very concise and poor in explanations. The value of  $\varphi x$  given by (27.39) can be easily proved to satisfy (27.36) by mere substitution. I have however provided my own hypothesis as to how this particular value might have been deduced by Babbage. On the determination of the final solution of (27.34) or (27.17) see (5) above.

(12) Via two examples, Graves showed that Babbage's paradox is a "subtle instance" of the following general proposition "which is not a priori improbable". In his [1836, 446-7] he wrote: "Though we may prove it to be impossible to find one fixed form  $\psi$ , such that the equation  $\psi x = F\psi x$  (...) shall hold good simultaneously in different cases where particular values of  $x$  are assumed (...), we are not therefore to despair of finding direct forms of  $\psi$ , absolute or alternative, which for certain values of  $x$ , within appropriate limits, shall severally satisfy the equation  $\psi x = F\psi x$ . Such a partial form of  $\psi x$  and the corresponding form of  $F\psi x$  taken with it may be likened to two curves which coincide for a certain continuous space and divaricate in the rest of their course". Graves went on to mention the necessity of the introduction of "infinitesimals" or "limits" in the course of his paper ending by saying, "it is, I believe, a novelty in algebra, to present an instance of a given individual function of a positive or negative quantity, which varies accordingly as the functional subject is regarded as the limit of this or that kind of imaginary quantity".

## Section 2.8

(1) On Herschel's unfinished book on algebra and on the Encyclopedia articles he composed around 1817 see [Enros 1979, 188-190]. According to [Becher 1980a, 390] it was Peacock who asked Herschel to compose a textbook on algebra in 1816.

(2) This quotation together with the "theorem" (28.2) are given from Herschel's letter [H.S, 20:38, [Jan. 30 1817]].

(3) Herschel did not provide any explanations about the proof of (28.2) in his letter. I remind the reader that  $\psi x = \bar{\chi}(x, \alpha(x))$  is the solution of equation  $\psi x = \psi \alpha x$  when  $\alpha^2(x) = x$  [see (25.14)]. For further details on (28.2)-(28.6) see [De Morgan 1836, art. 219]. Equation (28.6) appears in Herschel's letter [H.S, 20:40, 31 March 1817].

- (4) See Herschel's letter [H.S. 20:40, 31 March 1817]. In this letter he asked Babbage to send him the exact reference of a paper on factorials by Nicolson [I have found no information on the latter].
- (5) In 1818 he had sent to Babbage his Notes for approval, saying: "In the Note you will find some very general and I think very pretty theorems, and as you are made honorable mention of several times in the course of it, I think you ought to see it previous to publication" [H.S. 20:53, 10 March 1818]. In the Notes to Spence's edition Herschel introduced the term "periodic" function for one which follows the law  $f^n(x) = x$  for some  $n > 0$ . By this term Herschel distinguished these functions "for their great utility and extremely remarkable properties" [1820, 153]. This term consequently appeared in the first page of Babbage's Examples [1820].
- (6) Neither Babbage nor Herschel provided any demonstrations of Bromhead's theorem (28.11) In the latter's [1824, 571-572] -referred to in text below- we are provided with a detailed proof of Taylor's expansion theorem for a function  $f(x)$  in terms of the symbol  $D = d/dx$ . As a corollary we have a sketch of the proof of (28.11) which reads as follows: "There is nothing in the nature of analysis which necessarily limits expansion to differential form. The series may be conceived to proceed by other functions of  $x$  and  $h$ ; it even may not consist of a succession of sums, but may ascend by products, or according to any other relation". Thus Bromhead regarded  $f(xe^h)$  to be developed in the form  $f(x) \cdot f_1(x)^h \cdot f_2(x)^{h^2} \dots$ . He called  $f_1(x)$  the factorial function of  $f(x)$  denoting it by  $Pf(x)$  and then suggested that by expanding  $f(xe^{1+h}) = f(xe^h \cdot e^1)$  and by equating the like powers of  $i$  and  $h$  (as he had done in the case of  $f(x+(i+h))$ ) one would obtain (28.11).
- (7) Enros [1979, 194] holds that this was due to Herschel's critical remarks. He quotes a passage from Herschel's letter to Bromhead dated 4 April 1817, which is almost identical with the passage cited in text [see (6) above]. Nevertheless, Herschel seemed to approve of Bromhead's work. In the end of the letter cited in text dated 3 April 1817 he wrote: "But his [Bromhead's] general views of analysis are very good, nay, grand". In any case the reason -or at least one of the reasons- why his paper was not published is evident in the extract from his article [1824] cited in text above.
- (8) This quotation is from [Enros 1979, 191]. For further information on Barlow and his unfavourable reviews of Herschel's and Babbage's work see [ibid, 172-4, 178-9, 182-3]. It might be of interest to mention here that Herschel was disappointed with Peacock. In his [H.S. 20:47, 1 Aug. 1817] he wrote to Babbage: "Have you heard anything about Peacock? he really treats us rather scurvily".
- (9) On circulating functions as well as on (28.25) see respectively articles

66 and 277 in De Morgan [1836].

(10) Enros's Phd thesis [1979] is an excellent study of the aims of the Analytical Society and the obstacles confronted by its members in the course of their ambitious work. Particularly on their lack of interest in mathematics around 1820 see [1979, 208-264].

## Section 2.9

(1) The first step in the direction of an automatic language for reasoning was taken by R. Lull (1235-1315) in 1270 as in his book Ars Magna. The next important step was taken by G.Dalgarno (1626-1687) and J. Wilkins (1614-1672). On the latter's Essay towards a real character and a philosophical language in the 1660's see [Nidditch 1962, 14-18]. For further references on the issue of a universal language see [1.8, (19)].

(2) See Herschel's letter [H.S, 20:42, 16 April 1817] mentioned in 2.8. Commenting upon the extension of Babbage's transform  $\varphi^{-1}f\varphi$  in the form  $\varphi^{-1}D^n\varphi$  (where  $D = d/dx$ , a symbol of derivation), Herschel wrote: "Thus you see, this calculus of derivations expanded will include your calculus of functions as a particular case. Think of this". [See also (28.13); 2.8, (6), (7)].

(3) The quotation is from Herschel's letter [H.S, 20:7, 25 Febr. 1813]. In fact, at this early stage it was mainly Babbage who suggested writings by Continental mathematicians to Herschel, therefore he must have read Euler's book that Herschel referred to [see also 2.4]. Further on Condillac's influence on Euler see [1.8, (6), (11)].

(4) In the concluding pages of his [1864, 485-6], Babbage described briefly his mental and physical efforts during his active years of study. He ended by saying: "Probably a still more important element was the intimate conviction I possessed that the highest object a reasonable being could pursue was to endeavour to discover those laws of mind by which man's intellect passes from the known to the discovery of the unknown. This feeling was ever present to my own mind, and I endeavoured to trace its principle in the minds of all around me, as well as in the works of my predecessors". This passage clearly reveals the psychology of Babbage which foreshadows that of Grätry and Boole. The connection between Boole's and Babbage's reasoning was noticed by M.Boole in her [1972, 49-50]. The proximity between the philosophical views of these two men will not be pursued in this thesis. For further comments see also [Laita 1980, 55-56].

(5) Babbage's definition of  $\overline{\psi^{0,0}}(x, y)$  is a rather vague one and the absence of any examples of illustration is striking. For further study of this defini-

tion see [De Morgan 1836, art. 267-270; 3.8].

(6) In his [1816, 249], while confronted with the possibility of a function whose index is fractional, he referred in a footnote to negative indices introducing without explanations the definitions (29.5)-(25.6) and (29.9) for the first time in his work.

(7) In his earlier papers -particularly in his [1816]- Babbage had introduced more complicated notation for cases of simultaneous substitutions for functions with more than two variables. We have omitted all the instances of the novel notation he offered as they were not widely applicable. However, we can mention at this point De Morgan's mild implicit criticism on Babbage's condensed forms: "It is much more easy to invent notation for cases which it may be conceived will hereafter arise, than to enter into the general solution of any one instance" [1836, art. 265]. We imply by this, that in many cases the symbolism or the examples presented was so constructed as to apply rather to cases that were not previously conceived than to problems which already existed [see 2.5-2.6].

(8) Babbage, according to Pycior, was mainly influenced by D.Stewart's nominalist position. A book, apparently by Stewart, was cited in a footnote [1827, 327] in connection with the role of definition in geometry. But no further direct or indirect reference to Stewart is evident in Babbage's published work. On the origins of [1827], as well as on Babbage's unpublished essays on the philosophy of signs, the reader can consult [Enros 1979, 202-207; Dubbey 1978, chapt. 5]. On Stewart's influence on Babbage see [Pycior 1984, 435-436]. The work of Stewart will not be discussed in our thesis.

(9) On Laplace's procedure as in his Mécanique see [Dhombres 1986, 148-150]. Further on equation (27.25) see 1.4 [(14.18), (14.30)]. In a footnote at [1827, 357] Babbage referred to his own general solution in the form (27.30) as well as to another form not included in his [1817] or [1820].

(10) See an introduction to De Morgan [1836] in [3.5 text and (1)].

## Chapter 3

### Section 3.1

(1) De Morgan's treatise [1836] is touched upon in [Koppelman 1971, 233-4, 241] in connection with the rise of abstract algebra in mid-19<sup>th</sup>-century Britain. Dr.I.Grattan-Guinness is the first historian to perceive a foundational link between De Morgan [1836] and the latter's work on the logic of relations [1860a]. After his suggestion, Merrill [1990] hinted at this link in his study of De Morgan's logic of relations, carried out largely in a

philosophical frame. As both a study of [1836] as such, and that of the impact that [1836] had on De Morgan's logic are missing, we will deal with the former in this chapter postponing the latter for chapter 6.

(2) Both [1836] and the textbook on The differential and integral calculus [1842c] are often cited in the course of this thesis. However, the doctrine of Cauchy's limits and its slow diffusion in British textbooks is not discussed [see 1.1, (5); 3.9].

(3) While the period 1839-1860 will be discussed in the next two chapters, De Morgan's <sup>mathematical</sup> work during this time will be exceptionally studied in this chapter as on the whole it developed independently from the calculus of operations which flourished by Boole and his followers from 1844 onwards.

### Section 3.2

(1) On the Tripos exams see [Becher 1980b, 4-6, 42-3; Forsyth 1930, x-xi; Roth 1971, 225-7, 230-236]. A moderator was an examiner who posed original questions in the Tripos exams [see Ellis 1863, xix]. Information on the role of a moderator is given also by De Morgan in his [1832, 276; 3.4,(11)].

(2) On Peacock's role as a moderator in the 1810's and as a lecturer and tutor up to 1839 see [Becher 1980b,13-14; De Morgan 1832,276; Garland 1980, 29-39; Koppelman 1971,179-180,187; Pycior 1981,25-27; and (21) below].

(3) On the issue of differential notation see [2.1, (2); 2.2, (9)].

(4) By "pure" mathematics we refer to algebra and analysis in the sense of the manipulation of abstract symbols independent from geometric or physical concepts or limits. "Mixed" or "applied" mathematics involve Newtonian calculus (but not its notation) as based upon geometric and physical considerations [Becher 1980b; Garland 1980, chap.3]. Cayley is <sup>a</sup>striking example of a Cambridge mathematician who contributed <sup>to</sup> pure mathematics [see (1) above].

(5) On Whewell's "liberal education" see [Becher 1980b;Garland 1980, chap.3].

(6) For a general survey of mathematics and mathematical physics in Cambridge during the period 1815-1845 see [Grattan-Guinness 1985a].

(7) Since the name of an author or editor is often missing, we cite the books devoted to collections of Cambridge problems as "Cambridge" followed by the date of their publication. For details see the bibliography under "Cambridge".

(8) This passage, following formula (27.14), is an extract from Herschel's letter [H.S, 20: 35, 14 July 1816].

(9) However, Whewell's memoirs on the tides are regarded as exceptional.

(10) Whewell referred to the main 18<sup>th</sup> century English analysts who developed Newtonian mechanics. On Maclaurin's, Simpson's and others' work on the theory of the earth's shape at that time see [Guicciardini 1989, chap. 5].

(11) Whewell [1837] included a chapter on common algebra (as based on Wood's

textbook [see De Morgan 1832; 3.4] ) and one on inductive logic. On Whewell's philosophical and educational views in general see [Garland 1980, 32-36; Pycior 1982a, 408; 1983, 224-5; Richards 1980, 350-353; 1987, 19]. On Whewell's endorsement of Euclid's geometry see [Becher 1980b, 24].

(12) See references in (5) and (11) above, as well as [3.4, (7)].

(13) For bibliographical information on the articles contributed to this encyclopedia in the 1830's and early 1840's see [Grattan-Guinness 1985a, 67-88]. Among the authors we mention Airy, Whewell, Herschel, De Morgan and Barlow.

(14) From 1831 onwards the Tracts included an introduction to Fresnel's optics. Further on Airy's Tracts and on Whewell's reaction see [Becher 1980b, 26; Garland 1980, 45; Grattan-Guinness 1985a, 103-4].

(15) See also [2.2 text and (4)]. Particularly on equation (32.5) see (14.9), (22.1) and [Herschel 1845, 677; Whewell 1823, 30-43, 198-204]. In fact, Whewell incorporated more elements from Woodhouse's study of (32.5) than Airy did.

(16) Airy presented (13.12) under a different notation and form. As the essence is actually the same we omit Airy's version as this would demand clarifications irrelevant to our study. For the equivalent of (13.12) in Airy's Tracts see the last equation at page 99 in [1826] and equation (I) in [ibid, 107]. The same equation is traced in [Airy 1845, 187; O' Brien 1840, 53].

(17) In fact "c" in (32.7) relates to "a" in Laplace's formula (13.19) on the radius of the earth. However, formula (13.19) is not mentioned by Airy.

(18) On Laplace's reasoning from (32.7) up to (33.11) see (13.21)-(13.28). For a simplified reproduction of it see [Pratt 1836, 556-7; 1.3, (4), (12)].

(19) For a critical commentary on O' Brien's insufficient account see [Pratt 1860, "Preface"; 1871, 29; Todhunter 1879, 313, 315; 1.3, (11)].

(20) Becher's paper on "William Whewell and Cambridge mathematics" [1980b] has been of great help in this section. Moreover, his lecture on Cambridge mathematics and physics in the late 1830's -given at Chelsea College, London in 1987- was very informative. Becher stressed the role of the theory of the earth's shape in the development of fluid mechanics at Cambridge. In the course of this lecture he mentioned that W. Thomson found Airy's presentation as totally unconvincing, and also that Airy was not happy with his own treatment.

(21) On the contributions of Babbage, Herschel and Peacock on the Cambridge curriculum in the 1810's and 1820's see also [Wilkes 1990].

### Section 3.3

(1) On Murphy's life see [Smith 1984a, 1-7].

(2) An outline of Murphy [1837] is included in [Koppelman 1971, 195-6; Smith 1984a, 23-25]. The first to acknowledge Murphy for his theory of transforms and distributive operations was Pincherle [1899, 14; 1912, 1-9]. See also

[Cooper 1952, 11]. However, hardly any reproduction of his methodology is noticed, with an exception [Smith 1984a, 24].

(3) As it is beyond the scope of our study we have omitted Murphy's work on definite integrals. For a summary of his early papers see [Smith 1984a, 7-22]. On Murphy's inversion formula, as in [1833b, 362-3] see [Smith 1984a, 14; Deakin 1981, 372-3].

(4) (33.2) is equivalent to (14.3) if  $A=B=1$ . Murphy reduced (33.2) to

$$(i) \quad \frac{d^2y}{dx^2} = \frac{1}{a^2} yx^{1/\alpha - 2} \quad \text{via (ii)} \quad m + 2 = 1/a, \quad t = \alpha x, \quad a^{1/\alpha} AB = 1/a^2$$

where  $m, a, A, B$  and  $\alpha$  constants. If  $A=B=1$  then (i) is equivalent to (14.4). Form (i) was claimed to be the "form best adapted for resolution" [Murphy 1830, 440]. Indeed the Riccati equation was usually studied in that form [1.4, (4)].

(5) The constant  $\alpha$  in (33.3) stands for the  $\alpha$  in (ii) given in (4) above.  $S$  and  $\Phi(h)$  are given by

$$(iii) \quad S = \sum_n \frac{x^{n/a + 1}}{n!(a+1)\dots(a+n)} \quad \text{and} \quad (iv) \quad \Phi(h) = \sum_n \frac{h^n}{(a+1)\dots(a+n)}$$

respectively. The constant  $a$  in (iii) and (iv) stands for the same  $a$  as in the first formula in (ii) above [1837, 440-441].

(6) We omitted theorem (33.4) in 2.3. Its proof is based upon (23.15). See [Herschel 1814, 448-449]. On Herschel's notation see (23.9)-(23.14).

(7) For an outline of Murphy [1833a] see [Smith 1984a, 8-10]. Murphy was to draw frequently on this paper in later works [see (33.61) and his 1838, 99]. Among Murphy's illustrations were Lagrange's theorem for the expansion of  $f(z)$ , where  $z=a+hF(z)$  and Burmann's theorem of expansion [1833a, 139-141]. On the latter theorem see [Grattan-Guinness 1990, art.3.4.3; De Morgan 1842c, 303] for its original appearance in 1798 and a detailed proof respectively. Apparently independently of Murphy, De Morgan incorporated a systematic collection of all such known theorems of expansion, including Herschel's (23.18), in his [1842c, 74, 168-174, 303-311]. Most of these theorems were repeatedly re-demonstrated by symbolical methods in the 1840's and 1850's [5.2, 5.4].

(8) For  $m = 0$  equation (33.5) obtains the form

$$(i) \quad t(1-t) \frac{d^2u}{dt^2} + (1-2t) \frac{du}{dt} + n(n+1)u = 0.$$

Putting  $\mu = 1-2t$  equation (i) is reduced to

$$(ii) \quad \frac{d}{d\mu} \left[ (1-\mu^2) \frac{du}{d\mu} \right] + n(n+1)u = 0,$$

Murphy's form for the "Legendre equation" [1833c, 14; 1.3, (5), equation (1)].

(9) Murphy's account, as in his Notes to [1833b], is given in full. For a detailed computation of the appendage of  $\Delta^{-1}$ ,  $[0]\Delta^{-1}$  or  $\Delta^{-1}(0)$ — see [1837, 189–190]. As we will not present this computation later on, I would like to mention that  $\Delta^{-1}(0)$  is of the form (33.13) where  $m_i$  are  $\pm i\pi(-1/h)$ .

(10) Pincherle [1912, 1, fn 1] attributes to Murphy this distinction which, however, goes back to Sarrus in 1822 [see 1.6, text before (16.23)].

(11) The rule (33.19) in its general form was given only verbally. It resulted from his deducing  $[x^2]\psi^3$  by writing  $\psi^3$  as  $\psi\psi\psi$ . However no parentheses are spotted [1837, 180]. Followed applications of the rule (33.19) to examples.

(12) As we shall see in text below, (33.22) was a result of theorem (33.27) and the definition of  $e^0$  (33.35). Theorem (33.20) was stressed for its importance by [Koppelman 1971, 196; Pincherle 1912, 9], but its demonstration is missing. Surprisingly, it is absent in [Smith 1984a].

(13) The transform (33.51) is of wide use within mathematics (functional calculus, definite integrals, matrices etc.). See [Pincherle 1899, 14; 1912, 8, 35] on Murphy's and Laplace's introduction of (33.51). For further references on Babbage's transform (28.11) or (25.1), see [2.5, (1)].

(14) Formula (33.57) will appear in our study in the form :

$$(i) \quad \left[ \frac{d}{dx} - (n-1) \right] \dots \left[ \frac{d}{dx} - 1 \right] \frac{d}{dx} = y^n \left[ \frac{d}{dy} \right]^n, \quad (ii) \quad x = \log y,$$

h in (33.57) taken equal to 1. Notice the peculiarity of Murphy's notation (33.19)–(33.20). On (i) see (45.20).

(15) Formula (33.58) —apparently of no useful application— is recorded here as a first instance of nonsensical symbolical expressions which resulted from an obsession for outmost generality and abstraction. Relevant instances are recorded in chapter 5.

(16) See Carmichael's book [1855] studied in 5.7.

(17) See also [4.4, (2), (3), (4)].

(18) Smith [1984a, 4]. De Morgan was the first to appreciate Murphy's work. A reference to [1837] is given in De Morgan's article on operations [1840, 445].

### Section 3.4

(1) On the rise of the problem of negative and imaginary numbers in early-19<sup>th</sup>- century England and on the early development of abstract algebra see [Nagel 1935; Pycior 1987; 1981, 27, 34].

(2) Peacock's principle is discussed in text below [see (3), (17)–(21)].

(3) On the early critical reception of symbolical algebra see [Pycior 1982a]. On Whewell's objections towards Peacock's principle and his obstruct-



ing role in the adoption of symbolical algebra see [Pycior 1982a, 408; 1983, 224-5; Richards 1980, 362-3]. It is, however, of interest to note that Whewell embraced Peacock's principle in his philosophy [Richards 1980, 352-3].

(4) See [Clock 1964; Nagel 1935; Novy 1968; 1973, 189-199; Pycior 1983].

(5) See particularly [Clock 1964, 17-107; Pycior 1983]. Koppelman [1971, 215-220] takes a slightly different position partly objecting to Clock's arguments. On a brief review of these researches see [Richards 1980, 344-5].

(6) De Morgan did not pursue a career at Cambridge, objecting to pass a theological test. On his life see [Mac Farlane 1916, 19-33; Howson 1982, 75-96].

(7) On Whewell's influence on De Morgan see [Richards 1987, 15-20].

(8) As this matter is beyond the scope of this thesis we omit a discussion of De Morgan's views. On his [1831] see also [Pycior 1983, 212-216].

(9) [1831, 62]. The quotation in text was given in English but De Morgan did not provide any reference. Probably he drew from the Seances of the Ecole Normale in 1795 [see 1.8, text above (6)].

(10) We remind the reader that Whewell had shown particular preference to Wood's algebra endorsing elements of it in his [1837; see 3.2, (11)].

(11) [De Morgan 1832, 276; see also 3.2, (1)].

(12) On De Morgan's defence of Whewell's "liberal education" and on the latter's obstructive role towards the development of abstract algebra see [3.2, (5); 3.4, (3), (7)].

(13) For a rather unusual paper on the students' reaction to De Morgan [1835b] see [Pycior 1982b]. In the preface to the book De Morgan distinguished between the "art" and "science" of algebra [1835b, 3; see also Richards 1980, 354-5].

(14) [1835b, 198]. As we shall see in chapter 6, De Morgan would claim in the same spirit that "Logic considers not thought but the form of thought", adding that the forms of thought are "more visible in algebra than in other thought" [1858, 82].

(15) For a further discussion on this quotation see [Pycior 1983, 217].

(16) This passage is discussed in [Pycior 1983, 218]. Pycior describes it as "crucial", holding that "De Morgan ranked algebra as a science, a status he later denied its symbolical component". She holds that by defining algebra "as a science of investigation rather than quantity", he accused Peacock of "algebraic conservatism" [ibid, 217]. I regard her arguments as slightly exaggerating as it is difficult to describe as "Abstract stage" the period during which De Morgan reviewed in a far from lucid way Peacock's work, a period which apparently in her paper hardly covers one year. As Richards stressed [1987, 14] De Morgan's review took a long time to be constructed, for as the latter admits [1835a, 311] it was "of very great difficulty of forming fixed opinions upon views so new and so extensive". Both writers express interesting

views on this review; however an isolated study of it can not lead to definite conclusions on De Morgan's views at that time. For example, his [1836], written shortly after the review, does not show evidence of any accusation towards Peacock's conservatism; it is representative of De Morgan's ambiguous and ambivalent style, and of his tendency towards meaningful, rather, than symbolical algebra [3.5-3.9]. On the whole his tendency for conceptual understanding is stronger than the instances which imply a formalistic one.

(17) Most probably this example was drawn from Peacock's work. On Peacock's treatment of (34.3) see [Richards 1987, 14-15].

(18) Further on Peacock's algebra, his distinction between "arithmetical" and "symbolical" algebra and his emphasis on arithmetic as "science of suggestion" of the latter algebra, see Pycior's extensive discussion in [1981, 33-38]. I call the reader's attention to a recent paper by Durand [1990] where Locke's influence on Peacock is discussed.

(19) [Peacock 1830, 104]. On earlier formulations of what we call as Peacock's PEF, by Woodhouse and Poncelet in 1801 and 1822 respectively, see [Pycior 1984, 431-2; Novy 1968, 215]. On Peacock's principle see [Nagel 1935, 179-182; Novy 1973, 190-4; Pycior 1981, 38-40; Richards 1980, 346-350; 1987, 14-15]. On Babbage's influence on Peacock's work see [Dubbey 1977, 301].

(20) [De Morgan 1835a, 308]. See also [Pycior 1983, 219-220; Richards 1980, 354-5; 1987, 14-15].

(21) [1835a, 310]. On De Morgan's critical acceptance of PEF see [Pycior 1983, 219-221; Richards 1987, 15-17].

(22) [1835a, 310-311].

### Section 3.5

(1) The volume is dated 1845, but an offprint of the treatise in the Library of the Royal Society bears the date 1836 [see also Grattan-Guinness 1985a, 100]. The 328 articles cover 75 quarto double column pages in small type.

(2) Prior to this passage, De Morgan commented briefly on the work done so far on the calculus of functions [see citation in 2.9, (10)].

(3) De Morgan's emphasis on the utility of mathematical theories is probably due to Whewell's influence [see 3.4 text and (7)].

(4) This last statement, in a slightly modified way, will be found in the "Preface" of Boole [1847a; see 7.1, (15)-(16)].

(5) De Morgan's difficulty to follow a consistent method of arrangement of the topics under study will be evident in our study in 3.6-3.8.

(6) At this instance we would agree with Herschel's pertinent remark on Euler's presentation of Calcul Integral [2.9, (3)]. A similar remark was traced in 1.8 by Gergonne<sup>about</sup> Lacroix. Both Euler and Lacroix were influenced

by Condillac's epistemology [see 1.8, (6), (11)].

(7) It is impossible to give a completely accurate list of points not discussed: the reader may consult the index of contents at [1836, 390-392]. As far as Laplace's, Babbage's, Herschel's and Spence's work is concerned, De Morgan's pertinent remarks are to be found in chapter 2, particularly in [2.3, (4); 2.4, (7); 2.5, (13), (17); 2.6, (15); 2.8, (3), (9); 2.9, (5), (10)].

(8) Apart from (2) and (5), all the other issues listed above in (35.1) were discussed in 1.8 in connection with French semiotics, as well as in 2.9 in connection with Babbage's and Herschel's work. Probably De Morgan was influenced by both French and English mathematicians. A detailed account of De Morgan's peculiar adoption of these issues will be given in 3.6.

(9) A first study of the functions listed in (35.2) below was carried out by Babbage and Herschel. We stress that De Morgan was not yet acquainted with the work of Servois and François on inverse or commutative functions.

(10) This concept, attributed partly to W.H.Maule [2.5, (3)] and partly to Babbage [1836, art.49] was probably so called after Arbogast's calculus of derivations. The name "derivative" might also have been borrowed from Bromhead's and Herschel's use of that term in 1817 [see (28.11)-(28.16), text and (6), (7)].

(11) The reader can draw a comparison between Murphy's study [3.3] and that of De Morgan's [3.6]. The latter lacks the rigour and depth of Murphy's research as well as the generality of his results.

### Section 3.6

(1) The peculiarities and consequences of De Morgan's approach are discussed in Richards [1987] in connection with algebra and analysis [see 3.4 text and (7)]. All the elements of De Morgan's reasoning pointed out in this recent study are to be perceived repeatedly in our discussion of his [1836], a work which was surprisingly omitted in Richards [1987]. The reader can consult this paper on lines parallel to the present inquiry.

(2) In art. 4 De Morgan wrote that  $a-x^2$  is the same function of  $x$  as  $a-y^2$  is of  $y$ . This evident remark was often used in his treatise. For an application see text below (36.9) and (36.21).

(3) [1836, art.8]. As we shall see in many instances, De Morgan regarded algebra as a kind of science of suggestion for the calculus of functions.

(4) [1836, art.73, fn]. This is an example of De Morgan's nonsensical remarks.

(5) This last citation, given by De Morgan in " ", echoes of empirical philosophers and particularly of Condillac [1798; see also 1.8; 3.4; 3.5, (8)].

(6) In art. 19 De Morgan draws an analogy between Lagrange's theorem

$$(i) \quad (1+\Delta)^n u_x = u_x + n\Delta u_x + \frac{n-1}{2}\Delta^2 u_x + \dots \text{ and } (ii) \quad (1+a)^n b = b + nab + \frac{n-1}{2}a^2 b + \dots$$

where  $a, b$  quantities, but  $(1+a)$  is viewed as an operation. In art.20 he claimed that "It would not be difficult so to generalize the notion of successive counting, that Algebra should become a Science of operations, even in the higher sense in which  $\Delta$ , an operation, is distinguished from  $u_x$ , a quantity". Thus, (ii) can become a particular case of (i) "in which  $u_x$  is derived from the equation  $\Delta u_x = b u_x$ . And Laplace's theory of generating functions might easily be connected with such a system". See also 3.9.

(7) Indeed, let  $\varphi_1, \varphi_2$  stand for (36.16) for  $i=1,2$ . Then, by (36.17), both sides of (36.15) are equal to:  $\varphi_1^2 x + 2\varphi_1 \varphi_2 x + \varphi_2^2 x + 3m$ . See [1836, art.22].

(8) De Morgan wrote in art.22 after the computations mentioned in (7) above: "Hence it appears that the separation of the symbols of operation and quantity can (for these functions [(36.16)]) be carried out to the same extent at least as in the Calculus of Differences, with this extension of meaning of the sign + ; that + placed between two isolated signs of operation, each taken once, requires the addition of m when the symbols of operation are reconnected with their symbols of quantity". This exaggerated statement was followed by (36.18).

(9) Prior to this statement, De Morgan wrote in a footnote [1836, art.25, 313]: "To the student of mathematical symbols we should decidedly recommend attention to the methods by which the deaf and dumb are taught to read and write". Art.25 is the main philosophical section in the treatise. Once more we see an influence from French semiotics, in this case of Diderot's letter to the deaf and dumb which is probably implied in the above quotation.

(10) This emphasis on symmetry strongly echoes Babbage's remarks in [1827; 2.9]. It also foreshadows a new mathematical trend which stressed the importance of symmetrical forms put forward by Carmichael, Spottiswoode and others few decades later [see 5.7-5.10].

(11) Apparently by mistake De Morgan wrote  $f^\circ$  instead of  $f$  in (36.20). The symbol  $\times$  stands for multiplication. His remark at this point about the solution of (36.19) is unusual for an elementary account; it far from persuades the reader why a further study of inverse functions is required.

(12) The only explanation provided was by means of the example (36.21).

(13) Typical of De Morgan, inconvertible inverses, were not excluded from his study. He admitted in [1836, art.34, 316,fn] that the demonstration of this latter theorem is "incomplete in several points: it is worth further consideration". Inconvertible inverses will not be discussed.

(14) The demonstration of (36.28) is perhaps the most rigorous in the

treatise. However, it was not illustrated within the functional calculus. De Morgan's paper [1838] was devoted to his so-called "discontinuous constants". His research, connected with some "anomalies" he had observed in "certain series", arose out of his [1836; see 1838, 185-6, 193].

(15) At [1836, art.49, fn] he wrote: "We do not like to suggest any other than a generic name. Any person may specify to himself what derivative he will call it". On the probable reason why he introduced this terminology see [3.5, (10)].

### Section 3.7

(1) [1842c, 737]. This method, applicable to non-periodic functions, is explained in (37.55) below. On periodic functions see the passage quoted below in (12).

(2) The assumption that  $\alpha^n x = c$  is an ad hoc one which facilitates the elimination of  $n$ . For further illustration see (37.32) and (37.66). As it is implied from his remark in art. 126, when De Morgan speaks of elimination of  $n$ , he implies the elimination of any  $n$  involved in the formulae under consideration, but where it stands as a functional index.

(3) [1836, art.144]. In seeking for a connection between  $\phi\alpha x$  and  $\psi\alpha x$ , De Morgan is close to Murphy's concerns in [1837].

(4) De Morgan's speculations are close to those communicated between Herschel and Babbage on the analogy between  $\psi^n x = x$  and  $z^n = 1$  [see (27.1)-(27.2)]. An outcome of this communication was included, as we saw in 2.7, in Babbage [1817], a work familiar to De Morgan [see 1836, art.72, fn]. But, contrary to these analysts, De Morgan was not satisfied by a mere observation of an analogy but wished to delve into its nature. He probably felt that it was through his critical statements and hints for further research that he contributed in the development of functional equations and relevant topics, than by establishing new results. However, despite the apparent failure of his method, he did contribute in his own way in establishing certain principles of symbolic reasoning—as studied in 3.6.

(5) [1836, art.121]. As in the passage quoted in (4) above, we have here again evidence of De Morgan's tendency to regard algebra as science of suggestion [see 3.6, (3)]. It might be implied from this quotation that he sought for a theorem in the calculus of functions analogous to Maclaurin's theorem in algebra. In this case successive functions would be analogous in role with the differential coefficients in the latter theorem. See also (7) below.

(6) On the disorder of his exposition see [3.5, (5)]. Other cases discussed in a fragmentary way concern inverse, convertible and periodic functions.

(7) Collins's method is but an application of Lagrange's [1.4, (9); (26.1)]. De Morgan was acquainted with the latter's [see 1836, art.246]—motivated by

it to his statement in (5) above.

(8) My own explanatory remarks --as implied by De Morgan-- are inserted in square brackets. Particularly on the elimination process see also (2) above.

(9) It is assumed that  $b \neq 1$ ,  $b' \neq 1$ . For an additional illustration of the ad hoc assumption (37.2) or (37.34), we will deduce the "exponential inverse"  $Ax$  of  $\alpha x$ . This amounts to the elimination of  $n$  between (37.33) and (37.34). By means of these two formulae we have that

$$(i) \quad b^n \left( x + \frac{a}{b-1} \right) = C \quad \text{hence} \quad (ii) \quad n = Ax = \frac{\log C - \log \left[ x + \frac{a}{b-1} \right]}{\log b}.$$

De Morgan omitted at this stage to mention the "exponential inverse"; however, formula (ii) was obtained in art. 53 in a different context [see (10) below].

(10) Equation (37.38) is not mentioned by De Morgan. In art. 52 he commented upon the evaluation of the indeterminate form produced when  $b'=1$ . Finally, in art. 53, he determined  $\phi$  in the case of  $b'=1$ . We now have (37.33)–(37.34) for  $\alpha x$ , and (iii)  $\beta^n y = n a' + y = C'$ . Based now upon (ii) given in (9),  $n$  is eliminated and  $y$ , or  $\phi x$ , is deduced from (iii). Hence,  $\beta x$  is once more a derivative of  $\alpha x$ .

(11) On different occasions he suggested different classifications of functions, mainly based upon the properties of their successive functions. The earliest classification is introduced in art. 45: Case 1: all the successive functions are different in form ( $f(x)=1+x^2$  or  $\log x$ ), Case 2: the successive functions assume only a finite number of recurring different forms ( $\phi x = (1-r+rx^2)^{1/2}$ ,  $r$  the cubic root of 1 [art. 36]) and Case 3: all are cognate forms [(37.46)], either with or without recurrence. On another type of classification see [art. 191]. In art. 61–63 he gave three distinct methods for the determination of  $\phi^n x$  where  $\phi x$  is the second of the forms in (37.46). Among them Horner's method as in Babbage [1820, 5; see art. 61, fn].

(12) [1842c, 737]. De Morgan's study of symmetrical functions is only a slight generalization of Babbage's study [see 2.5–2.6].

(13) Equations such as (37.52) often occur in "various branches of Mathematics" [art. 94], but no references were provided. On the history of such equations see [(14.14), (14.16); 1.4, (7), (8); Dhombres 1986, 157–164; Dubbey 1978, 89–91; Pincherle 1912, 47–51].

(14) In [art. 104, fn] De Morgan wrote: "The reader may, as in the case of the derivative [see 3.6, (15)], supply any name he likes better. It is important to observe that, in finding  $\mathcal{E}x, Ax$  may stand for the solution of  $A\alpha x = Ax \pm p$ , where  $p$  is any whole number". This latter equation, is a generalization of (i) in (37.65) and can be deduced by the same reasoning. This observation gave

rise to another mode of solution of (37.1) as based on a particular solution of an equation of the form  $\psi f x = f \alpha x$ , where  $f, \alpha$  are known [see art.109]. In art.113 he deduced from (i) in (37.65) the value of  $\alpha x = A^{-1}(Ax-1)$  from which it follows  $\alpha^n x = A^{-1}(Ax-n)$ . Thus, given  $Ax$  we can find  $\alpha x$  [see also (15) below].

(15) If  $\alpha x = bx$ , it was found that  $Ax = (\lambda c - \lambda x) / \lambda b$ . Now given  $Ax$ ,  $A^{-1}x = e^{-\lambda bx} + \lambda c$ , and  $Ax-1 = (\lambda c - \lambda x - \lambda b) / \lambda b$ . Hence  $\alpha x = A^{-1}(Ax-1) = e^{-\lambda b(Ax-1)} + \lambda c = e^{\lambda bx} = xb$  as required. Such trivial illustrations were omitted in the treatise.

(16)  $Ax$  in (37.37) is the exponential inverse of  $\alpha x$ ,  $\psi Ax$  the solution required and  $u_x$  equals  $\psi x$ .

### Section 3.8.

(1) De Morgan did not cite Babbage [1822]—or [1827]. His sole basis for the theory of functions of two variables is Babbage [1816]. Only in art.277 did he refer to Herschel [1822; see 2.8, (9)].

(2) In art.267, a bar is missing in the left-hand side of (38.5). De Morgan slightly diverges here from Babbage's definition of simultaneous substitution by a unique formula (29.4).

(3) Bars are missing from  $\psi^{\overline{0}, \overline{0}}$  in (38.8) in the text.

(4) In art.264 De Morgan introduced the notation  $\alpha \overline{x, y}$ . Thus, a function  $\alpha$  of two variables is denoted as  $\alpha(x, y)$ ,  $\alpha \overline{x, y}$  or simply as  $\alpha$ .

(5) As in the case of  $\psi^{-1}x$  and  $\psi_{-1}x$  [see (36.23)–(36.24)] he distinguished between  $\psi^{\overline{0}, \overline{0}}$  and  $\psi_{\overline{0}, \overline{0}}$  as those functions which satisfy (38.7)–(38.8) and only (38.7) respectively. We focus on the first concept  $\psi^{\overline{0}, \overline{0}}$ .

(6) Let  $\chi(x, y)$  be a zero-function. Then, from (38.7) formula (38.14) is immediately implied.

(7) In [art.281, fn] he wrote: "Such terms as modulus, derivative [exponential inverse...] have been purposely used, because, from the various senses in which they have been used, they are movable terms. They must not be considered as of permanent application". See also [3.7, (14)]. De Morgan did not prove (38.20) but argued on its evidence by providing examples.

(8) The exponent 3 is missing from  $\alpha(x, y)$  in text [art.281].

(9) Properties (38.26), (38.27), (38.29) and (38.32) correspond to (36.32), (36.33), (36.35) and (36.36) respectively for functions in one variable.

(10) In the search of non-permanent periodic functions De Morgan is more lucid in the theory he proposed than in his example (38.34). In text I have combined his theory with the example under consideration; explanatory remarks are inserted in square brackets.

(11) The process applied to the example (38.45) is the same as that applied to (37.23) in art.50.

(12) De Morgan assumed in theory that for any two zero-functions  $\alpha(x, y)$ ,

$\beta(x,y)$ , we have (i)  $\alpha(x,y)=c$ ,  $\beta(x',y')=c'$  in analogy with (37.56). Evidently zero-functions remain unchanged by simultaneous substitution. From (i) he deduced (ii)  $y'=Bx'$ ,  $y=Ax$  in analogy with (37.58). He further assumed that (iii)  $y'=\gamma y$ . It was shown that  $\beta$  is a derivative of  $\alpha$  if (iv)  $Bx=\gamma A\gamma^{-1}x$ . In the course of discussing example (38.52) he arrived at the conclusion that in general the problem of making any one zero-function a derivative of another "remains unsettled" [art.285].

(13) In art.291 a <sup>u</sup>coma was missing between the indices  $\alpha, \beta$  in (38.60). Notation was restored in art.292. As in the cases pointed out in (3) and (8) above, these omissions were apparently due to typographical errors.

(14) The procedure in this case is similar to that introduced in art.50 [see (37.25)-(37.31)] and further developed in art.102 [see (37.56)-(37.65)]. From (38.61) it follows that

$$(i) \quad V(x,y,n) = V(\alpha, \beta, n-1) \text{ and } W(x,y,n) = W(\alpha, \beta, n-1)$$

[see (37.26), (37.29), (37.56), (37.59)]. Then, from (i) and (38.63) it follows that (38.64) holds true.

(15) See also example (37.66). He added in art.292 that we can find "a more general solution" of (38.67) in

$$(i) \quad \theta \left[ \theta_1(x-y), \theta_2 \cos 2\pi \left( \theta_2(x-y) - \frac{\lambda y}{2} \right) \right] \text{ where } \theta, \theta_1, \theta_2 \text{ arbitrary functions.}$$

(16) See (15) above and related arguments in 3.6.

(17) In art.272 he claimed that "the most obvious form of all zero-functions" is (i)  $x+\theta(x-y)\psi(x,y)$  where  $\theta(0) = 0$  and " $\psi(x,x)$  is not infinite". If  $\psi(x,x) = \text{constant}$  then we consider the corresponding zero-functions as "infinite". Such functions "present analogies in some respect with the constant of the differential calculus, and also with the absolute zero of Algebra". Displaying examples to justify his argument, he wrote that such observations convey "certain ideas of classification among functions of two or more variables, and by means of which the everyday algebraical properties of simple functions may be made the guides to generalization" [art.274]. On classification see text below (38.71) and (38.72).

(18) In the second case we determine  $n$  from

$$(i) \quad \alpha(x,y)_{\alpha, \alpha}^{\eta, \eta} = F(n, x, y) = \mu \cos 2\pi n$$

in the form  $n=A(x,y)$  and hence the solution of (38.71) is given exactly as in (37.65) in the form (ii)  $\varphi(x,y)=\theta \cos 2\pi A(x,y)$ . This analogy between functional equations in one and in two variables was noticed in [art.297].

(19) The inverse of  $\theta(x,y)$  was denoted as  $\theta(x,y)_{\alpha, \beta}^{-1, -1}$ . In order to further justify his notation De Morgan wrote  $\theta(x,y)$  in the form  $\theta(x,y)_{\alpha, \beta}^{\theta, \theta}$  [art.298].

(20) This assumption of the zero-function  $(xy)^{1/2}$  seems arbitrary. Notice,



however, that both  $2xy-1$  and  $4xy-3$  have  $(xy)^{1/2}$  as their zero-function. Indeed, (38.73) holds true. We have

$\overline{\varphi}(z) = 2z^2-1$  and  $\overline{\psi}(z) = z(4z^2-3)$ . Now

$\overline{\varphi}\overline{\psi}\overline{\varphi} = \overline{\varphi}\overline{\psi} = 2[(\sqrt{xy})^2(4xy-3)^2]-1 = 2.16x^3y^3-3.16x^2y^2+18xy-1$  and

$\overline{\psi}\overline{\varphi}\overline{\psi} = \overline{\psi}\overline{\varphi} = (2xy-1)(4(4x^2y^2-4xy+1)-3) =$   
 $= 2.16x^3y^3-16x^2y^2-2.16x^2y^2+16xy+2xy-1.$

De Morgan typically omits such computations.

### Section 3.9

(1) In the late 1830's, elements from [1836] appeared first in the second edition of his [1835b], [1837, 203] and then in his [1838]. On these two works see [3.4, (13), and 3.6, (14)] respectively.

(2) See [1842c, 168-174; 303-315; 337-340, 746-750; 751-758; 703-706] on Arbogast's calculus, Herschel's theorem and related topics, generating functions, Gregory's symbolical methods and the Riccati and earth-figure equations respectively. De Morgan was not to develop substantially any aspects of the calculus of operations, but <sup>he</sup> certainly contributed in its diffusion. Besides his [1842c] see his [1840] on "Operations" cited below and his manuscripts on the calculus of operations cited in (6) and (13) below. On the topics referred to above see also [3.3, (7); 4.4]. I would like to add that he produced some work on Laplace's generating functions and on Arbogast's derivations [see his manuscripts with code number MS775/35-36, 131-133 in the Library of the University of London and De Morgan 1846].

(3) In the Watson Library of University College, London, there exist three letters by Higman to De Morgan with code number [MS Add 97/5 1847]. These letters date respectively 5.5.1847, 27.11.1847 and 4.3.1848. In the first letter he reminded De Morgan of having been his favourite pupil and went on to discuss aspects of the calculus of variations remarking "The French writers seem here all to copy one from another". In the second letter he wrote that there is no subject I am fonder of than the History of science", mentioning Montucla as his "favourite writer".

(4) This quotation, including further comments on Wallace's "Fluxions", is from Higman's letter dated 4.3. 1848 [see (3) above].

(5) [1836, art.52]. This comment concerned the study of problem (37.4) when applied to the functions  $a+bx$  and  $a'+b'x$  when  $b'=1$  [see 3.7, (10)].

(6) This quotation is from his manuscripts, <sup>with</sup> <sup>number</sup> code MS 775/208, written in October 1854 probably for teaching purposes. Instances from his Penny Cyclopaedia article [1840] are included. These manuscripts belong to the De Morgan Collection in the Library of the University of London. The concept of "joining" foreshadows the concept of logical relation [6.5-6.9]. Emphasis on

*in general*  
relations is noted in his [1836; 3.6, (9); 3.7; 1865; (15) below].

(7) These studies deal strictly with the purely algebraic character of his inquiries. On the laws which define +, and exponentiation see [De Morgan 1842b, 287-9; Clock 1964, 81-3; Nový 1973, 197; Richards 1980, 354-5]. For further references on De Morgan's algebraic work see [0.2; 3.4, (4), (5)]. The laws laid down by De Morgan nearly define the modern notion of a field.

(8) In [1836, art. 21] he introduced the symbol  $\nabla$  in  $u_{x+1} = u_x \nabla u_x$ . If  $u_x = \lambda u_x$ , where  $\lambda x = \log x$ , we have that  $\nabla u_x = e^{\Delta u_x}$ . In [1842a, 181] he let, for example,  $\nabla^0 \phi x$  and  $\nabla^1 \phi x = a_0 \phi x + a_1 \phi(x+h) + a_2 \phi(x+2h) + \dots$  [see text above]. We omit his further comments as they are far from elucidating the purpose of this last example.

(9) [1849a, 142]. In the beginning of this passage we have an instance of Carnot's issue of the various degrees of indeterminateness as used by De Morgan in his [1836, art. 14, 18; see also 3.6, (3), text below (36.12)]. This issue gradually developed in that of the form-matter distinction within logical context. See further [(16) below and 6.7].

(10) On the subject of divergent series see [Richards 1987, 26-28].

(11) [1849c, 89]. Condillac's apparent influence on De Morgan is hinted at only in [Nový 1973, 198] in connection with this passage. For other instances recorded in our study see [3.4; 3.6, (5); 3.5, (6)].

(12) See [Nový 1973, 198-9; Clock 1964, 95-106; Pycior 1983, 221-2; Richards 1980, 355-6].

(13) This passage is drawn from the manuscripts cited in (6) above. Further on the calculus of operations see those with code number 775/48-51, 60, 209-210.

(14) See [De Morgan 1865, 168, 170-172]. For a comparison with Boole see [(84.14); 8.9].

(15) In the last paragraph of this passage we have an instance of De Morgan's peculiar emphasis on the notion of relation.

(16) This quotation encapsulates De Morgan's mature reconsideration of Peacock's Algebra reviewed in 1835 [see 3.4 text and (19)-(21)]. Now he held that it is the "form-matter" distinction which was missing from Peacock [1830]. Besides Carnot's influence, the study of the method of separation of symbols and the comparison between the calculus of operations and functions had led De Morgan to emphasize the operational character of algebraic symbols. He had earlier remarked that Peacock had omitted such considerations in his algebra [see 1840, 443; text below (6)].

## Chapter 4

### Section 4.1

(1) On an acknowledgment of Boole's contributions in connection with symbolical solution of differential equations see [Cooper 1952, 10-11; Koppelman 1971, 197-200; Laita 1977; Petrova 1987; Pincherle 1912, 5; Rota 1975, 62].

(2) As is well known, operator methods are often mentioned as alternative to classical ones in current textbooks on differential equations. However, the instances provided concern principally the solution of linear differential equations with constant coefficients; the formulae now in use were invented, in fact, prior to Boole [1844] by Murphy and Gregory. See [Rota 1975, 62; Rainville 1974, chapters 6, 9]. Thus Boole's peculiar techniques are out of fashion, though Boole is widely acknowledged by historians as the mathematician who promoted operator methods in general. Moreover, his [1844] is hardly touched upon in recent papers on the history of the calculus of operators as cited in (1) above.

(3) The distinction between what we may call the "theoretical" and "practical" aspect of the early development of the calculus of operations in the 1830's and 1840's has not been pointed out in any historical survey on this subject so far. We recall that Airy did not encourage the method of variation of constants and, after Whewell, he favoured approximate techniques [see 3.2, (15)]. It is surprising that Koppelman missed altogether the role of the earth-figure equation in her extensive account on the history of the calculus of operations [1971].

### Section 4.2

(1) There is, however, one exception: Herschel's unique application of separation of symbols in his [1814; see (23.11)]. Apparently the first to discover Herschel's application was Gregory [1839c, 122]. There is a mistake in Gregory's reference: read [1814] instead of 1816 for Herschel's relevant paper. On the whole Herschel [1814] must have been hardly read.

(2) Fractional differentiation is not pursued in our thesis [1.7, (14)]. However, a casual look at Greatheed [1839] shows his strong background in the recent researches in analysis and hints at the possibility that earlier than 1839 he had been influenced by Fourier's operator method for partial differential equations [see 1.7; Greatheed 1839, 11].

(3) [1837, 239].

(4) The solution of (42.2) is partly reproduced in [Koppelman 1971, 188-9]. We have thus omitted it, focusing on other aspects of Greatheed's paper which

will be of importance later on in our study.

(5) There is a slight possibility that Gaskin was aware of Greatheed's work. However, as their methods differ substantially as well as the orientation of their attempts, we might hold that Gaskin worked independently from Greatheed.

(6) [Routh 1889, ii-iii].

(7) From now on, by "standard" form we will refer to the EFE in the form

$$(i) \quad \frac{d^2y}{dx^2} + n^2y = \frac{6y}{x^2}$$

cited as (13.32), (32.11) or (42.20). Its solution will be cited in the form (42.24) [equivalent to (13.31) or (32.12)]. By "general" form we will refer to the EFE (i) where instead of 6 we have  $m(m+1)$ , that is to (13.33) consequently cited as (14.8) and (42.13).

(8) I have copied Gaskin's exam question from [Cambridge 1849, 62; see 3.2, (7)]. The reference for the first appearance of that problem in January 1839 is provided in Glaisher [1881, 810]. The only alteration I have made is that I put "p" instead of "a" in Gaskin's formulae [0] and [3] so as not to confound this constant with the different constant a as introduced in the solutions [1] and [2]. Notice that if  $p=6$  in [0] then  $m^2+m-6=0$ , thus [0] gives  $m=-1, 2$ . According to Glaisher's assumption we take  $m=2$ , and thus [3] is reduced accordingly to [6].

(9) The symbolical solution [1] or (42.28) was also included in [Hymers 1839, 84] without any explanation as the solution of the "general" EFE (42.13) [see further details in text and (7) above].

(10) Information about Glaisher's life and work is drawn from his biography written by Forsyth in [1930]. Glaisher's favourite topics of research were differential equations, combinations of observations and elliptic functions. Like Gaskin, he examined for the mathematical Tripos with fair frequency.

(11) I came across Glaisher [1881] via Forsyth [1914, 200] in an attempt to see the effect of operator methods -as flourished in mid-19<sup>th</sup>-century England- at the turn of the century. It was a happy surprise to see that Glaisher's interest in collecting the different forms of solution given for the EFE partly coincided with my own inquiries. A study of his paper facilitated me in presenting a more complete image of my inquiries, particularly through his proofs of equivalence between different symbolic forms. Glaisher [1881] was published in the Philosophical Transactions of the Royal Society.

### Section 4.3

(1) On Ellis's reference to Gaskin and Laplace see his [1841a, 169]. Notice a printing error in his reference from Hymers [1839]; read page 83 instead of 53.

(2) [Ellis 1841a, 170]. Ellis based his arguments by taking odd and even values of  $m$  in (43.7). Formula (43.8) is only implied in his paper but is explicitly referred to in other cases. Minor changes in his notation are made in text, such as putting  $n$  in (43.2) instead of  $q$ , so as to keep to a consistent notation as far as possible. Such changes do not affect the least our own account and will not be mentioned in every single case.

(3) De Morgan remarked that (43.22) can be reduced to the same equation when  $x$  in  $(d/dx.1/x)^m$  is changed into  $n.x$ . This reduced form can be put in the form (43.23) if we make  $n$  the variable of differentiation and regard  $x$  as constant [1842c, 703]. Ellis tackled the general equation (43.14). Applying the solution for the specific case (43.15), he obtained Gaskin's form (43.23) [1841 b, 193-5] to whom he referred. Notice the similarity between the symbolic solutions (43.22) and (42.34).

(4) Ellis and De Morgan were briefly mentioned in [Glaisher 1881, 808, 811] in connection with their contributions in the EFE and the Riccati equation respectively. Results of Ellis's researches were incorporated in the form of problems in Forsyth [1814, 201, 204].

(5) [Ellis 1841b, 201]. On Fourier's integral theorem see (17.22).

(6) Walton and Goodwin were Cambridge wranglers in 1836 and 1840 respectively. Together with O'Brien they contributed papers on analytical geometry, where they applied instances of symbolical methods. Moreover, Walton should be mentioned for his papers on the calculus of operations as published in volumes 3 and 9 of the Quarterly Journal of Pure and Applied Mathematics in 1860 and 1868 respectively.

(7) Five letters of the Lubbock-Goodwin correspondence are kept in the Library of the Royal Society of London with code number B 473-477. The first is dated May 1831, the second December 1848, the next two April and February 1849 while the last April 1850. Unfortunately no relevant information is provided apart from what is mentioned in text.

#### Section 4.4

(1) Walton's edition of Gregory [1865] includes a biography of Gregory written by Ellis [see 1865, xi-xxiv].

(2) A careful study of Gregory's work gives evidence of his total ignorance of Murphy [1837] in the period when he wrote his early papers published in the Cambridge Mathematical Journal in 1839 which include the core of his theory. Even in his [1841] there are only two instances where he drew on Murphy's work, and these instances were not of a major importance in his own results [see comments on Murphy's theorem (33.61) and on his formula (33.60) in 1841, 242 and 29 respectively]. Therefore Koppelman misleads the reader by writing that

Gregory "gave as his major sources both Servois and Robert Murphy" [1971, 194]. On the contrary, while this statement is true about Servois, it is not true about Murphy. What Gregory meant by the citation given in text is that having read Murphy's paper he discovered the close proximity of their work on operators as well as an additional study which is worth referring to [see also (3) below].

(3) In other words, Gregory passed over Murphy's detailed study of Taylor's theorem, as based upon the establishment of  $e^x$  as an operator, as we claimed in text before (2).

(4) This is a crucial omission, in Gregory's foundational study of the calculus of operations. Surprisingly he did not refer specifically to Murphy [1837] in his Examples [1841] in connection with these properties [see also (2) above].

(5) The method of splitting  $d/dx$  in two operators as in (44.12) was initially introduced by Arbogast in 1800 [see (15.47)–(15.49)]. So, it is quite probable that Gregory was influenced by Arbogast [1800], a work consulted by Brinkley [2.2], Herschel [2.3] and De Morgan in 1836 [3.5].

(6) See [1841, 237–239; 1846, 287–290]. Gregory referred to Boole [1841c] at page 239 in connection with a theorem which put (44.21) in a more convenient form [see (44.47) and (45.3)].

(7) This method was further applied in Gregory's Examples [1846, 290–296] and remained standard well up to Boole [1859]. For example see Boole [1877, 394]. Example (44.27) was reproduced in [Petrova 1987, 13].

(8) Surprisingly, Boole omitted to mention Gregory. It has to be stressed that the symbolical procedure (44.41)–(44.47) did not render Laplace's method indispensable. On the contrary the inverse binomial operations in (44.47) depended upon the solution of simple functional equations by reduction of the latter to finite difference ones. As this matter does not concern our thesis we have omitted further details [see 2.4].

(9) [Russell 1857b, 180].

## Section 4.5

(1) On Boole's life and work see 7.1. On the instances mentioned above see particularly [7.1, text and (5)–(8); Mac Hale 1985, 44–9].

(2) Boole's procedure so far is discussed also in [Koppelman 1971, 197; Laita 1977, 168–9].

(3) On a detailed proof of the theorem for the expansion of  $U/V$ , where  $V, U$  polynomials of  $x$ , in partial fractions see [Hymers 1831, 32–5]. On the proximity between Boole's expansion theorem and that provided by Cauchy (for distinct and equal roots) and Gregory see [Petrova 1987, 8–11]. Lobatto had

independently invented this theorem in 1837 [Ince 1927, 138]. Boole referred to this discovery in [1860, 108, fn; 1877, 391, fn].

(4) [Boole 1841c, 119; see also (2) above]. Drawing on Gregory and Murphy, Boole expanded his theory for equations with constant coefficients, including several examples, in his [1877, 381-398; for further details see (5) below].

(5) Boole omitted to give any specific references from the work of Gregory, Murphy and De Morgan. Moreover, his omission of Murphy on the properties of inverse operations is striking both in this paper and in his textbook on differential equations [1859] hereafter cited as [1877; 8.8 text and (1)]. For the reader's convenience we will provide in our study corresponding references from his [1877]. I would like to point out the following:

i) The order of exposition in [1877] is reversed, that is it starts with the particular cases and proceeds gradually to more general ones [see also 8.8].

ii) Only part of [1844] is reproduced in [1877].

iii) Most of the examples repeated in [1877] are surprisingly given in a less detailed and lucid way than in [1844].

(6) See [Koppelman 1971, 198-9; Laita 1977, 171-3; Mac Hale 1985, 64-5]. See also comments in [4.1, (2)].

(7) Boole's paper is already divided in sections and subsections. Our own division is similar to his but probably more convenient for the reader.

(8) Theorem (45.12) is mentioned nowadays only in [Koppelman 1971, 198]. As the instances to be used from it in our study are already known independently from (45.12) [such as (45.19) and (45.20)] we have omitted its complicated proof. Also, instances only from (45.12) related to the operator  $\Delta$  were applied by Boole in his textbook on finite difference equations [1860] cited hereafter as [1880; see chap. 13, 236-263].

(9) We might perhaps draw a parallel between Boole's FTD (45.26) and Lagrange's theorem (15.3). The latter shows the relation between  $d/dx$  and  $\Delta$  while the former connects differential with finite difference equations [see stage 8 below]. See also [1877, 413-4].

(10) Since the rule is obscurely demonstrated and only briefly stated in [1877, 437-41], we have omitted it. However, we should mention that in the course of its demonstration Boole provided the following formula:

$$(i) \quad f_n(D)e^{n\theta A}P = f_n(D)e^{n\theta A}x^p + f'_n(D)e^{n\theta A}x^{p+1} + f''_n(D)e^{n\theta A}x^{p+2} + \dots$$

where  $f_1$  as in (45.27),  $A$ ,  $P$  functions of  $x$ ,  $D=d/d\theta$  and  $x$  the symbol of multiplication [1844, 235]. Formula (i) is evidently a generalization of Leibniz's theorem (33.48). Useful in the development of the calculus of operations, (i) was given by Hargreave in 1848 independently from Boole [see 5.3].

(11) This example was not given in [1844]. However, we provided it here instead of the more complicated cases given in [1844] since the procedure is the same. The reader may notice that Boole's method of series is an elaboration of those used by Bronwin and Ellis [see (43.2), (43.24)]. The main difference lies in that prior to obtaining the "scale" of the equation (45.31), Boole transformed the given equation (45.29) to a simpler one of the symbolical form (45.30).

(12) Instances from stage 5 were reproduced in [1877, 446-450]. See [Russell 1857b, 181] against the inclusion of so abstract material in a textbook.

(13) The solution of equation (45.52) was given by Boole in the course of stage 4 [1844, 255] as

$$(i) \quad u = c_1 \cos(n \sin^{-1}x) + c_2 \sin(n \sin^{-1}x)$$

[notice a misprint in  $\sin^{-1}x$ ]. The only explanation provided for (i) was that it is deduced by his method illustrated via the EFE and other equations of this category [for details on the latter see 4.6]. Equation (45.52) was integrated by Bronwin in his [1843a, 32-34; 1843b, 178]. For another example from the summation of series see [1844, 264-265; 1877, 445] reproduced in [Mac-Hale 1985, 64-65] without any comments, nor even a specific reference.

(14) On the gamma function  $\Gamma(n)$  and its properties see [Greatheed 1839, 16-18; Rainville 1974, 170-171]. We omit details as we regard the theory of definite integrals as only partly related to our thesis.

(15) [1844, 279, fn]. For similar remarks by Boole on Gregory see also [1877, 391, fn]. Part of stage 8 is reproduced in connection with finite difference equations in reference given in (8) above.

#### Section 4.6

(1) Boole expressed the right-hand side of (46.10) as  $P_r \varphi(D)/\psi(D)$  "in accordance with Sir John Herschel's notation for the integrals of equations of finite differences of the first order", claiming that (46.13) below in text is such an equation [1844, 248]. In fact, (46.10) resembles Babbage's continued product [see (24.1)-(24.2); 2.4, (1)]. Theorems 1 and 2 stand for Boole's Propositions 2 and 3 respectively [1844, 247].

(2) We have omitted Boole's rules on the arbitrary constants, as in the examples which follow below in text the cases are straight forward. These rules are best explained in [1877, 421].

(3) At [1844, 250], after Boole's formula (33) [(46.19)], there is a mistake: read  $\psi(D)$  for  $\varphi(D)$ , and vice versa. This error is corrected in his later exposition in [1877, 424]. On  $\varphi(D)$ ,  $\psi(D)$  see also (4) below.

(4) Boole is very concise in his presentation, and certain clarifications are occasionally to be added. First, notice an error in his writing  $D \pm ir$  - just



below the sentence discussed in (3) above— instead of  $D \pm in$ . To make (46.20) comprehensible let  $n$  in the initial equation (46.14) equal 2. Now,  $\varphi(D)$  and  $\psi(D)$  correspond to

$$(i) \quad \frac{q^2}{D(D+a_2-a_1)} \quad \text{and} \quad (ii) \quad \frac{q^2}{D(D-1)} \quad \text{respectively. By (46.10) we have that}$$

$$(iii) \quad P_2 \frac{\varphi(D)}{\psi(D)} = P_2 \frac{D-1}{D+a_2-a_1} = \frac{(D-1)(D-3)(D-5)\dots}{(D+a_2-a_1)(D+a_2-a_1-2)\dots} ;$$

hence, we want the factors in the denominator of (iii) to disappear, or, if  $i, j$  positive integers,  $a_2-a-2i = -2j-1, j > i$ , hence

$$\frac{a_2-a_1+1}{2} = -k, \quad k \text{ positive integer.}$$

(5) Boole provided the condition (46.20) —as given in text above— and then stated simply the formulae (46.21)–(46.23) without any further clarification. According to (46.10) all consecutive factors differ by  $\pm n$ .

(6) Equation (46.25) stands for the EFE cited before as (13.32), (32.11) or (42.20). The solution (46.30) was provided in exactly the same form as (13.31), (32.12) or (42.24). Evidently the equality of the form (46.30) with those cited requires a change in notation; put  $y$  for  $u$ ,  $n$  for  $q$  and  $C, a$  for  $c, c_1$  respectively [see also (42.24); 4.2, (7)]. In his textbook Boole first solved equation (46.25) and then inquired into the problem (46.14). Nevertheless, the solution given in text (46.28)–(46.30), was still provided in a more detailed way via the theorems 1–2 used above for (46.14) [1877, 422–3].

(7) At [1844, 251] read  $v$  instead of  $u$  in the formula (46.27) given by Boole prior to (46.31). His claim for  $V$ , as followed from (46.32), was based on the rules given earlier for the arbitrary constants [see also (2) above]. Our computations prior to (46.31) were missing in Boole [1844, 1877]. The reproduction of this treatment in his textbook hardly covers 4 lines.

(8) See [4.2, (7)]. Boole mentioned in [1844, 251] that under a slightly different form, equation (46.33) was discussed by Mossotti in his memoir on molecular action, additionally treated by Paoli and Plana. In [1877, 424] he added that (46.33) featured in Poisson [1835, 158]. See also 4.8.

(9) A study of equation (46.33), including a discussion of the equivalence between its solution by Boole and Ellis was first given by Glaisher in [1872]. In this paper entitled "On a differential equation allied to Riccati's" Glaisher made use of Boole's symbolical method. Besides his proofs of the equivalence of the different forms of the solution of the Riccati equation on the EFE, he showed that equation (46.33) is additionally satisfied by the definite integral

$$(i) \quad u = x^{1+1} \int_0^{\infty} \frac{\cosh \xi d\xi}{(x^2 + \xi^2)^{1+1}}, \text{ as a particular solution.}$$

Via the gamma-function  $\Gamma(i)$ , (i) was reduced to the form (46.40) and (46.46) [1872, 130].

(10) [1844, 182]. The reproduction of the material of [1844] exposed in 4.6 in his textbook [1859 or 1877] is in general the same, only given in different order [see 4.5, (5)]. For Boole's symbolical research in the realms of binomial equations the reader can also refer to the supplementary volume to his [1859] as edited by Todhunter from Boole's manuscripts in [1865]. In connection with stage 4 see particularly [Boole 1865, 175-199].

(11) Forsyth included a section entitled "symbolical solution" [1914, 197-206]. The only example illustrated in detail was the EFE in the form (46.33) in the negative case. This equation was solved by means of the transform  $y = ux^{m+1}$  [(42.25)] and by successive differentiation on lines similar to those followed by Gaskin. The solution was given in the form (46.46) [1914, 197-9]. Followed references to Gaskin, Glaisher, Ellis and that to Boole given in text. Further on Forsyth's textbook see [4.2, (11); 4.3, (4); 7.1, (25)]. In case of misapprehension, we have traced at least two more textbooks which involve symbolical methods by Airy [1866] and Earnshaw [1871]. But both writers deal with partial differential equations and though Boole's name is mentioned, still his methods are not used [see 9.9].

#### Section 4.7

(1) This paper did not see a second part. I would like to add that instances from (45.12), such as formulae (45.19)-(45.20) were used by Boole in his paper "On the transformation of definite integrals" [1843a], drawing on Gregory's Examples [1841]. See [1843a, 216-219].

(2) Boole's manuscripts of his [1845d] exist in the Royal Society collection with code number W<sub>21</sub>. Boole provided there formulae concerning the expansions of  $f(d/dx + \cos x)$  and  $(x^2 + d/dx)^n$ . His notes ended with some questions which apparently were not answered and thus not included in his published work; for example, "Can Lagrange's theorem be obtained by this method?". For pertinent remarks, like those below (47.14), made by Donkin and Graves see [5.5].

(3) Boole included instances from his [1845d] in later writings. Particularly formula (47.13) was mentioned in his [1877, 455] and theorem (47.1) in the form

$$(i) \quad (x + \varphi' \left( \frac{d}{dx} \right)^{-1} X = e^{\varphi \left( \frac{d}{dx} \right)} x^{-1} e^{-\varphi \left( \frac{d}{dx} \right)} X$$

in [1880,281-2]. For (i) Boole included a separate proof simpler than that employed initially in [1845d]. On Boole's two textbooks see [4.5,(5) and (8) respectively].

#### Section 4.8

(1) Evidently our review of these four topics is done through the point of view of operator methods. We take for granted our study up to this point, reminding the reader of the most important issues without reproducing the formulae cited. In few instances we cite for convenience textbooks published later on, as we did in previous sections. Books concerning new theories will be accordingly studied in chapter 5 and 9.

(2) As we mentioned in 3.2, J. Pearson was a Cambridge wrangler in 1848. Unfortunately, we have not traced any information on his life and work. In 1850 we have the second edition of his calculus of finite differences cited below. Pearson stressed the method of separation of symbols in a subtitle. The first edition must have been published few years earlier. On Herschel's theorem and related topics see [1850, chaps. 1-2]. Pearson's presentation is very comprehensive and well organized.

(3) Gregory enriched his account in [1841, 240-1] with formulae concerning the forward-difference operator  $E$  including Français's related example (16.10). A propos, in connection with Arbogast's calculus of derivations I would like to refer to J. West's Mathematical treatises [1838] edited by J. Leslie 22 years after the author's death. This reference is unique in chapters 3-5 in connection with Scottish mathematics. Though not so popular among the figures under study, Arbogast's work is nevertheless occasionally referred to in the course of our study of mid 19<sup>th</sup> century mathematics.

(4) On the theory related to problem (48.2) see [Hymers 1839, 14-18, 36-42].

(5) For references to Hymers's textbooks see [Becher 1980b, 21, fn].

(6) Equations such as (48.4) were studied by Poisson, Cauchy, D'Alembert and others. See also (37.52) and [3.7,(13)]. On Wallace see [2.2, (5)].

(7) Equation (48.5) is solved by Laplace's method of substituting  $u_x$  for  $\varphi(x)$  and  $u_{x+1}$  for  $\varphi(x+\varphi(x))$  [see Pearson 1850, 64-66]. Equation (48.6) is solvable by Babbage's method. Notice that  $1/(1-x)$  is periodic of the third order, thus solved as example (25.38) [see also Hymers 1858, 93-5].

(8) None of these problems demanded the solution of the LE. As a characteristic question we cite the following. Given LE in the form (13.3) it was demanded to prove that a solution of it is (13.2), in other words that the expression for  $V$  satisfies (13.2) cited also as (33.25) [Cambridge 1849, 64-for the year 1844].

## Chapter 5

### Section 5.2

(1) Notice that Bronwin had tackled the EFE in the form (43.24) and (43.33) by the series method and by reduction through the transform (43.34) respectively; the latter method foreshadowing his work in 1846-1847. Equation (52.2) differs from those cited above only in the sign of the third term. Such differences as with equation (46.33) do not affect the procedures applied.

(2) Bronwin's procedure bears obvious similarities with those followed by Gas-kin and himself for the solution of (42.13) and (43.33) respectively.

(3) From A.J.Ellis's first attempt to solve the EFE in the standard form (32.11) in 1836 up to Boole [1844] and Bronwin [1846a], the form (ii) in (52.7) was the basic equation to which the EFE was reduced in various ways in order to be integrated (see (32.5), (32.15), 4.2-4.3, (46.14)).

(4) The result (52.8) is an immediate outcome of the successive application of (52.3) to (52.2) if no factors are discarded. Hargreave showed that (52.8) can also be put in the form

$$(i) \quad y = (D^2+k^2)^{-1} \left[ \frac{a \sin kx + b \cos kx}{x} \right], \quad a \text{ and } b \text{ constants.}$$

This form (i) was to be an outcome of Hargreave's more general method in [1848; see (53.24)-(53.25)].

(5) Boole's procedure is straight-forward. From (52.10) it follows that  $\pi_m p v = p \pi_{m-1} v$  for any function  $v$ , hence if  $p v = u$ , we have  $\pi_m u = p \pi_{m-1} p^{-1} u$ ; by iteration the latter gives  $\pi_m = p^m \pi_0 p^{-m}$ , hence (52.11) is deduced from (52.9) and (52.10). Evidently  $\pi_m$  and  $p$  defined in (52.12) obey the law (52.10). Boole claimed that this method is in accordance with his theory in [1844] in connection with (45.10)-(45.11) [Boole 1847d, 6-8].

(6) Equation (52.13) has been discussed so far in the form (17.35), (46.42) and (48.7). Bronwin's reduction of partial to ordinary differential equations is similar to Greatheed's and Gregory's procedures [see (42.9) and (44.36)].

(7) Equation (52.18), a case of Legendre's equation [3.3, (8), (ii)], had occupied Bronwin's study since [1843a, 29]. His early mode of solution by series is illustrated in our study through the EFE (43.24). Notice a similarity between Boole's transform (46.37) and Bronwin's (52.20), the former being a result of a more general but also complicated method. On Bronwin's preference for particular methods see remarks in [5.4, (6), (11)].

(8) In (52.28) " $\varphi D$ " stands for " $\varphi(x) \cdot D$ "; the same holds true up to (52.32). In (52.33) " $\varphi(D)$ " stands clearly for the function  $\varphi$  of  $D$ . Under this clarification I follow strictly Bronwin's notation. However, often " $\varphi D$ " in

papers of that time stands indifferently for " $\varphi(x).D$ " or " $\varphi(D)$ ", a fact that renders difficult its correct interpretation.

(9) Theorems of the form (52.36) were prominent in the calculus of operations since Murphy's (33.56) [see 4.7]. Hargreave would form an outstanding exception in refusing to make use of such "artificial" theorems, contrary to Bronwin who made extensive use of them in his later work [5.4,(1)].

(10) Our source of information is Smith's edition of "The Boole-Thomson letters" [1984c]. The letter by Boole cited in text has code number B<sub>166</sub> and belongs to the Cambridge University Library [see Smith 1984c, 4-5, 23, 28-29]. On Thomson see [3.2 text and (20); 7.1, (10)].

(11) [Boole 1847f,293;see also 5.8,(17)].

(12) Quotation from Boole's letter dated April 1847, B<sub>166</sub> as in [Smith 1984c, 23; see (10) above].

(13) [Smith 1984c, 23; see (10) above].

(14) We omit details of procedure as irrelevant to the scope of our present study. Boole [1847c] will be mentioned in 5.8 in connection with the LE [see also (15) below].

(15) Bronwin's generalization of Boole's procedure is illustrated in [5.4,(9)]. Bronwin based his fallacious arguments through the integration of (52.18) [1848c,258;(7) above] refuted by Boole [1848a, 414-5].

(16) Boole [1848a, 418]. Further on Boole's arguments on the "laws of correct reasoning" in connection with mathematics and logic see [7.2, (7)].

(17) Bronwin was entirely omitted in [Cooper 1952; Forsyth 1914; Glaisher 1881]. The only references to him so far are found in [Koppelman 1971, 201 and Smith 1984c] in connection with bibliographical references and Boole's comments respectively [see also (10) above]. In Pincherle [1912,5,fn 22] Bronwin [1851a] is briefly mentioned. On the latter paper see [5.4].

(18) Boole mentioned the problem of interpretability of symbolic forms first in his [1847c] in connection with the LE [see 5.8,(5)]. His remarks in [1848a;(16) above] concerned the matter of rigorous reasoning through the use of such forms. As we shall see in 5.3, Hargreave was the first to call attention to the legitimacy of using uninterpretable forms in the intermediate course of a procedure and to the necessity of the interpretability of the final result in 1848. Around 1847-1848 Boole was deeply concerned with this matter in connection with logic [see 8.2,(1),(6)]; probably independently from Hargreave's remarks. In any case, Bronwin's fallacies were a stimulus for the attention of both Hargreave and Boole in this direction [see also 7.2, (7)].

### Section 5.3

(1) See [Anon. 1868,xvii-xviii].

- (2) In (53.4) " $\varphi x$ " stands for " $\varphi(x)$ ". In what follows in text after (53.4) I use Hargreave's notation.
- (3) Theorems (53.3) and (53.4) are attributed to Hargreave in [Carmichael 1855, 18-20; Koppelman 1971, 202; Pincherle 1912,10]. Boole had provided a form of (53.3) in [1844,235; 4.5,(10), equation (i)].
- (4) [1848, 31]. See also [5.2,(18); (7) below and 5.4].
- (5) Hargreave used " $\chi$ " instead of " $g$ " in (55.10); " $gD$ " stands for " $g(D)$ ". Notice the inconsistency of his notation in (53.11)-(53.12). There is a misprint at page 32 where a " $D$ " is missing from the second exponent in (53.12).
- (6) In his textbook Boole wrote that the solution (53.15) of equation (53.13) -given by him in [1847e]- flows easily by means of Hargreave's theorem of conversion [1877, 455]. Surprisingly, apart from a brief reference to his theorems (53.3)-(53.4) in recent papers [see (3) above] no more is known nowadays from Hargreave's work as published in 1848-1850.
- (7) [1848,33; on interpretability see also (4) above].
- (8) Formula (43.1) stands for Bronwin's (ii) in (52.7); [see 5.2,(3)].
- (9) Formula (53.26) was obtained by Hargreave in 1847 through a different procedure [see (52.8) and 5.2, (4)].
- (10) Computational details, such as those inserted between (53.32) and (53.33) were omitted by Hargreave. The same remark holds for his asserting (53.40)-(53.41) below given without clarification.
- (11) On the Riccati equation see [1.4,(4);3.3,(4);4.3 above (4)].
- (12) By the interchange of  $D$  and  $x$ , Hargreave reduced (53.36) to a form which is integrable if (i)  $\chi(D)u = [\psi'(D)-\varphi''(D)]u$ . The solution of the transformed equation is then given by means of (53.15) where  $X_0$  stands for  $x^{-1}P$  [1848, 41].Hargreave was acknowledged for this study by Vessiot [1910,125,fn 209].

#### Section 5.4

- (1) [1850, 261]. By "artificial" methods Hargreave apparently referred to Boole's theorem (45.12) and its corrolaries, among them (45.19), (45.20) and (47.1)-(47.2). On the two latter see further remarks in [5.2, (9)]. Moreover, all the methods studied so far, with the exception of Boole's own as based upon the FID (45.26), are "partial".
- (2) Contrary to his initial research [1848;5.3], here Hargreave confined -after Boole [see 4.5;(3) below]- to solutions in series form (54.6).
- (3) For a simple relevant example from Boole [1844] see equation (45.29). Hargreave chose, in fact, more general examples from Boole's paper than (45.29), omitted in our study as being far too complicated. Apart from these two figures, no one dealt with such a general research with the partial excep-

tion of Russell [see 5.9-5.10].

(4) [1850,284]. This justifies our claim in text above that Hargreave was very close to Boole's spirit of investigation [see also 5.2,(12)].

(5) Hargreave showed a deep interest in sound reasoning and interpretability apparently out of a combination of his experience in court and his passion for order and mathematics [see 5.3,(1) and text; 5.2,(18)]. De Morgan's lectures and the study of Boole's work might have added to that [on De Morgan's pertinent educational beliefs see 3.4, particularly (15)].

(6) [1851a, 461]. Notice Bronwin's stress on the necessity of particular methods and our claims in the beginning of this section.

(7) Formula (54.15) —given without proof— was apparently deduced on the same lines as (52.28). The obvious steps from (54.15) up to (54.16) were given by Bronwin in detail. Whenever we refer to a "polynomial function"  $f(x)$  we mean one of positive or negative powers of the variable  $x$ .

(8) Notice, as in the case of (54.20), the necessity of Boole's theorems (47.1)–(47.2). See also [5.2, (9)].

(9) Formula (54.28) was initially given by Boole [1847c, 8] as a necessary condition for the integration of equation (52.53) which is (54.29) for  $X=0$ . We had omitted Boole's procedure in 5.2 as Bronwin's own covers it.

(10) In fact, Bronwin took the theorem (53.4) as an evident result without acknowledging Hargreave for it. Equation (54.33) is a case of Hargreave's (53.36). For further details on the latter's procedure see [5.3,(12)].

(11) [1851a, 477; see also (6) above and 5.2, (7)].

(12) Hargreave drew on De Morgan [1842c; see 3.9,(2)] for his illustrations. On the two former theorems see references in [3.3,(7)] and on the latter three the list (48.1).

(13) [1853,363]. This citation partly justifies our arguments in 5.1.

## Section 5.5

(1) With the exception of a paper [1849] published in the Philosophical Magazine, all the other papers discussed are partially published in the source cited in text. Graves's work is also known from instances mentioned by Carmichael; particularly the latter made use in his treatise [1855] of Graves's Fellowship Lectures to which I had no access. See also text below and (5).

(2) On similar deductions, such as (55.3)  $\rightarrow$  (55.4), see numerous instances in 4.7, 5.2, 5.4 and 5.5. Particularly on polynomial functions see [5.4, (7)]. Notice also that (55.4) was initially given by Bronwin as (52.49).

(3) Donkin [1850, 16]. On similar instances on analogy of expansions of non-commutative operations with Taylor's theorem see [4.7, (2); (52.51)].

(4) While Carmichael reproduced Donkin's basic theorems, Boole confined to a

brief statement. In Boole [1877, 456] Donkin is acknowledged, together with Bronwin, for his expansions of functions of non-commutative symbols but without any specific reference. Koppelman further included (55.7). Surprisingly he is omitted in [Cooper 1952; Pincherle 1912].

(5) Biographical information on C.Graves is taken from the Dictionary of National Biography. Surprisingly, C.Graves is omitted in Pincherle [1912, 5, fn 22] while his brother J.T.Graves is briefly acknowledged. The latter's rich library now belongs to University College London.

(6) Graves [1853, 285].

(7) It is of interest to notice that apart from Gregory one more analyst cared to combine functional with operator methods. Equation (55.36) is in fact an instance of (26.14). Moreover we recall in (55.35) Babbage's transform (25.1). As with Gregory, Graves's solution of functional equations depends upon that of a finite difference one [see 4.4, (8)].

(8) Graves omitted parentheses; it follows evidently from the general context that  $\psi n$ ,  $\psi D$  etc. stand for  $\psi(n)$ ,  $\psi(D)$ ... in his formulae. He followed a consistent notation, not susceptible to misconception as in the case of Bronwin [5.2, (8)] and Hargreave [5.3, (2), (5)].

(9) For a sample of correlative theorems proved independently so far see Boole's (47.1)–(47.2), Bronwin's (52.51)–(52.55), Hargreave's (53.3)–(53.4), Bronwin's (54.16)–(54.20) and Donkin's (55.5)–(55.11). Now, in virtue of Graves's sound establishment of Hargreave's theorem (53.8), the second set of the above formulae can immediately be obtained from the first.

## Section 5.6

(1) Gregory's Examples were not often cited in this chapter so far. The book featured prominently, however, from the early 1850's, in the work of Curtis, Carmichael and other analysts [see further 5.7, 5.9, 5.10].

(2) The solution of (56.1) [(53.31)] is thus  $u=e^{-\alpha_1 z}$ ,  $z$  given by (56.7). Indeed Curtis's solution is less complicated than Hargreave's own (53.33). Boole noticed Curtis's interesting study incorporating equation (56.1) as an exercise to be solved in his [1877, 459].

(3) See [Joly 1917].

(4) The reader can see the similarity between Williamson's procedure via (56.8)–(56.11) and Bronwin's own in (52.18)–(52.20). On the interpretation of the solution (56.12) via theorem (45.19) see Boole's process (46.37)–(46.39) and (46.43)–(46.44). On the change of the independent variable (56.12)–(56.13) see [4.3, (3)] and [4.6, text from (46.44) up till (9)]. I have followed strictly Williamson's notation apart from putting "z" in (56.10)–(56.11) instead of his "y" so as not to confound the latter with  $dy/dx$ .



(5) In Graves's paper "g" in (56.31) stands for his "χ". Notice at [1857a, 36] a mistake in (56.36) where instead of "g" [χ] he puts "9". Most probably this is a typographical error.

(6) Hargreave integrated the LE in [1848;5.8] by a method different from that introduced in the corpus of that paper [see 5.3]. However, Graves's generalization of Hargreave's procedure could in theory apply not only to the EFE but to the LE too [see a note in 5.8 text below (58.31)].

## Section 5.7

(1) Unfortunately no biography of Carmichael exists. Of his work we are principally informed through his textbook [1855] discussed below. Besides mathematics, Carmichael was occupied also with ecclesiastical writings. He was in close contact with all the eminent Irish mathematicians of his time. [Dowell and Webb 1982, 227-8] inform us that as soon as he became a Fellow of Trinity College Dublin he showed an ardent concern with educational matters "upholding the claims of the non-tutor Fellows". In our century Carmichael is mainly known for his textbook noticed briefly in [Cooper 1952,19; Koppelman 1971,213; Pincherle 1899,14; 1912,5].

(2) The only commentary on Carmichael's work in our days is given by Koppelman [1971,205-6]. She rightly observed that Carmichael's work "was based on the mistaken belief that  $\frac{d}{dx_1}, \frac{d}{dx_2}, \dots, \frac{d}{dx_n}$  were always pairwise commutative". She confined <sup>herself</sup> to representative instances of his work, such as to Euler's theorem for homogeneous functions (57.4) and the formula (57.11). However a printing mistake is spotted; she writes  $\nabla_n = \nabla^n$ . Let  $n=2$ . Then, by Carmichael's claim  $\nabla_2 = \nabla(\nabla-1) = \nabla^2 \nabla$ . Both this theorem and (57.11) are taken by Carmichael for granted. The first -that is (57.11) for  $n=2$ - is indeed readily proved after effecting the multiplication  $\nabla(\nabla-1)$ ; it should be noted that

$$\left(x \frac{d}{dx}\right) \left(x \frac{d}{dx}\right) = x^2 \frac{d^2}{dx^2} + x \frac{d}{dx} \quad \text{and} \quad \left(x \frac{d}{dx}\right) \left(y \frac{d}{dy}\right) = xy \frac{d^2}{dxdy}.$$

(3) Carmichael proved in detail both Hargreave's theorems (53.3)-(53.4) in his treatise [1855, 18-22].

(4) Indeed, as in the case of the theorem (57.8), once more formula (57.20) depends upon the distributivity of the factor  $[F(xD)]^{-1}$ . Gregory totally omitted Murphy's study of inverse distributive operations in his work [see 4.4, (2)-(4)]. It should be noticed that the theorems (45.19)-(45.20) used by Carmichael were known to Murphy and Gregory and thus do not form peculiar features of Boole's method [3.3,4.4-4.5].

(5) We have omitted the long and complicated computations involved. This

remark holds for all the equations susceptible to this method.

(6) [1855, 58]. See also (5) above.

(7) The evaluation of (57.48) in the case of one variable was effected in Graves [1853; 5.5, (6)]. The proof of (57.50) was based upon Graves's theorem

(i)  $e^{\psi}F(u) = F(e^{\psi}u)$ ,

$F$  any polynomial function, and  $\psi$  a distributive function such that  $\psi(uv) = \psi u + u\psi v$  holds true. For details see [Carmichael 1855, 96-8; 5.5, (1)].

(8) Carmichael's critical overview of the current operator methods was repeated in his textbook [1855, 3-4]; Boole is not mentioned but implied, since he was the main advocator of "artificial" methods (after Gaskin). Carmichael's avoidance of the latter methods coincides with Hargreave's own [5.4, (1)]. In fact, Hargreave, though he avoided to use even the theorems (57.6), (57.13) -wrongly called by Carmichael "Boole's fundamental theorems"- was much closer in spirit of method to Boole than Carmichael was.

(9) The reader had an opportunity to discern Carmichael's peculiar application of symmetry in both method and notation. It should also be stressed that symmetry coexisted throughout with analogy, elements that characterized particularly Babbage's and De Morgan's work. Together with symmetry Carmichael stressed in his textbook homogeneity; quoting Wood: "... if the proposed equation be homogeneous, the final result must be so. A proper attention to this observation will frequently detect an error in the process of solution" [1855, 5; on Wood see 3.2, (11); 3.4, (10)].

(10) Dr. Crilly pointed out to me that symmetry of expression was a feature found in French analytical geometers of the 1800's too. Probably this was due to Condillac, as according to Richards [1986, 301-2] French geometers of that time were influenced by him. The element of symmetry in Gregory's work in geometry is an issue, not touched upon, which might explain Carmichael's interest in it.

(11) The definition (57.53) is fairly similar to (57.3). The symbols  $i, j, k$  stand for Hamilton's quaternions, introduced in 5.8 in connection with the LE. On (57.53) see Hamilton [1846, 291] further discussed in 5.8. The symbol  $\nabla$  was initially used by Brisson and Laplace [1.6, (2)]. It was further spotted in Gregory [1839e, 116] in connection with generating functions.

(12) Boole was the first to apply partial differential operators in his study of invariants of homogeneous polynomials [1842a, 9-20]. He was followed by Cayley in 1845, and by the 1850's Cayley, Sylvester and Salmon -the so-called "Invariant Trinity"- contributed an abundance of work on this subject (Crilly 1986, 241-246, 248; Kline 1972, 927; Richards 1986, 308, 317). Further on Cayley's and Sylvester's application of partial differential operators see 5.10.

(13) J.H. Jellett graduated from Trinity College Dublin in 1838. Admitted into

holy orders in 1846, he was elected two years later to the chair of natural philosophy. In our century a brief reference to his treatise on variations [1850] is included in [Koppelman 1971,213; Pincherle 1912,5,fn 22]. In his Notes [1850,355] Jellett proved that if  $F$  a distributive function then

(i)  $F_i\psi = iF\psi$  "for all rational values of  $i$ , and may be extended to irrational values by the method of exhaustions". Further, at [1850,358-360] he made considerable use of the method of separation of symbols in a way reminiscent of Gregory's proof of Leibniz's theorem [see (44.12)]. The only one to acknowledge Jellett's operator study was Greer in his paper on fractional differentiation [1860b]. Further on Greer see 5.9.

(14) In fact, after Salmon and Graves, Carmichael devoted his later paper "Methods in the integral calculus" [1857c] to illustrations of the merits of the "reciprocal aid" between geometry and analysis [1857c,507].

## Section 5.8

(1) When no further indication is provided this stands for the Philosophical Transactions of the Royal Society London.

(2) On Laplace's study of equation (58.1) in physical context see [1.3; particularly (13.1) up to (13.11)].

(3) This note entitled "On the equation of Laplace's functions" was published in the Report to the British Association for the Advancement of Science for 1845. It contained only the equations (58.1), (58.8), (58.9), (58.10) and (58.11) without any details of derivations [Boole 1845e,2].

(4) Similar techniques of reduction had been used also by Greatheed, Gregory and Bronwin [see (52.14); 5.2, (16)].

(5) The solution of equation (58.1) via (58.7) and (58.5) initially yielded the integral

$$(i) \quad u = (1 - \mu^2)^{-a/2} \mu^{n+a} \left[ \frac{d}{d\mu} \frac{1}{\mu} \right]^n \mu^{n-a} [(1+\mu)^{n+a} \psi(\varphi) + (1-\mu)^{n+a} \chi(\varphi)].$$

Boole wrote that "it remains to interpret this remarkable expression", in other words to get rid of the factors with exponent  $a=(d/d\varphi)\sqrt{-1}$ . The result, after a careful application of Taylor's theorem, was the expression of the integral of (58.1) via the two equations (58.8)-(58.9). The symbol  $[(d/d\mu)(1/\mu)]^n$  involved in (58.9) has already been discussed in connection with the solution of the EFE (46.33) [see Boole 1846,17-18; 4.6].

(6) In his note [1845e,2; see (3) above] Boole called (58.10) the "real form of solution" of (58.1) commenting further as follows: "The summation is quite unlimited with respect to  $r$ , that is,  $r$  may be positive or negative, integral or fractional. In all cases the coefficient of  $\cos r\varphi$  is a finite function of

$\mu$ , which is an unexpected result".

(7) [Boole 1846,22; see also 4.6,(10)].

(8) We have omitted Boole's formulation of (58.16) as a similar process was sketched in [4.5, stage 7].

(9) This is an interesting sample of the mutual interaction between the theory of the earth and differential equations which first took place around mid-18<sup>th</sup>-century France [see 1.1,(3)]. As we saw in [3.2, 4.6, 5.2, 5.8] this interaction was repeatedly evident among mid-19<sup>th</sup>-century English analysts.

(10) Formula (58.18) was given in Boole [1846,8-9]. Equation (58.17) —when  $\alpha=\beta=0$  and where the right-hand side equals  $X(x)$ — was studied by Bronwin as (54.29). For further details see [5.2, (14)-(16); 5.4,(9)].

(11) Bronwin [1850a;5.4] followed on Boole's steps and twelve years later Russell resumed Boole's abstract inquiries [see 5.9,(8)].

(12) See Boole [4.6], Donkin on (56.19), Graves on (56.29)–(56.38) (on Hargreave's lines) and Bronwin on (54.45)– on Boole's lines.

(13) Initially the material of Carmichael [1853c] was published in the Cambridge and Dublin Mathematical Journal in 1852 [see references in Carmichael 1853c,273; 1857a,216].

(14) In a similar manner Carmichael treated equation (58.50) in four variables via (58.58), and Poisson's wave equation (17.32) [1853c,278–284].

(15) [1857a,216–17]. This statement is cited from Carmichael's paper "On Laplace's equation and the calculus of quaternions" published in the Proceedings of the Irish Academy in 1855, hereafter cited as [1857a]. The same paper was consequently published in the Quarterly Journal as [1857b]. All the papers cited in the rest of this section either bear the same title as Carmichael [1857a] or are untitled [see also (17) below].

(16) All the papers cited hereafter were published between 1854 and 1856 but we refer to them by the date of publication of the volume, 1857.

(17) In 1846 Hamilton suggested the transformation

$$(i) \quad D_x^2 + D_y^2 + D_z^2 = -(iD_x + jD_y + kD_z)^2,$$

where  $i, j, k$  defined by (58.60) [see 1846,291]. In his present note [1857,62] he recalled of (i), saying that it was "obviously connected with the celebrated equation of Laplace", adding that it had escaped his notice that the LE could also be written in the form (58.50). Hamilton [1846] —covering twenty pages— as well as Boole [1847f; see 5.2,(11)], do not belong to the Catalogue of Scientific Papers. This fact was pointed out to me by A.Clark of the Royal Society Library, London, who reinforced my vague impression that "there is lots of unlisted Irish treasure" [his words] in the Proceedings of the Irish Academy.

(18) Graves [1857c,167]. See also Graves's pertinent study of operators

devoid of imaginaries in the late 1840's up to the early 1850's in 5.5.

(19) Boole [1857,376].

(20) Boole [1857,384-5]. Boole's solution of the LE in a quaternion form is omitted on the grounds of its complexity and impracticability.

## Section 5.9

(1) The quotations are drawn from [Kempe 1885, xxv-xxvi; Rix 1898, 826].

(2) On "morphology", as introduced by Goethe (1749-1832) and further developed by Cuvier [see 2.9], forming by mid-19<sup>th</sup>-century the "very soul" of natural history with Darwin (1809-1882), see [Ospovat 1981,278-280]. Further on Huxley's endorsement of Darwin's "evolutionary morphology" see [Bowler 1989,384-4]. On Darwin's own use of this term -its definition appended in the glossary of his [1859]- see [Darwin 1985,415-17,471: a reprint of his 1859]. Sylvester alluded to Goethe, Huxley, Cuvier and Darwin in his [1869a,2-4,fn 5]. Sylvester wrote [1869a,2,fn] "... metamorphosis run like a golden thread through the most diverse branches of modern intellectual culture, and forms a natural kind of connexion". On similar lines, Darwin picked out similarities between different species of animals referring to, ten years ago, to the "unity of type" and to "successive slight modifications" [1985,415-16]. This epistemological unifying approach can be compared with Boole's and Gratry's view that the true method in science consists "in the unity of all sources of knowledge" [8.9,(11)].

(3) These are the words of Spottiswoode's master B.Price of Pembroke College Oxford. Further on Price (1818-1898) see [Clifton 1905].

(4) I have employed Spottiswoode's initial notation (59.3) in (59.8), (59.10) instead of Sylvester's (59.7) as we have a more direct analogy with Carmichael's own notation. The symbol  $\nabla_{1+j+\dots+i}$  is defined in analogy with (59.3). The ordinary form of the equation (59.8) is omitted but the discussion that follows in text clarifies Spottiswoode's extension of Carmichael's work.

(5) Formula (59.15) -a case of (57.33)- follows from Carmichael's theorem (57.26). Substituting in (59.15)  $x^{1/a}$ ,  $y^{1/b}$ ... for  $x,y,\dots$  respectively we have in virtue of (59.13) formula (59.16).

(6) Carmichael acknowledged and applied Curtis's useful method in his treatise [1855,41-44]. A very brief reference was also made on Spottiswoode [1853] but any reproduction of it was omitted [1855,7].

(7) The external multiplication  $(p-n)(p+n)$  was denoted by Russell by

$$\begin{array}{r} p+n \\ p-n \\ \hline p^2+pn \\ -p(n+1)-n^2 \\ \hline p^2-p-n^2 \end{array}$$

On similar lines he denoted division [1861,70-72].

(8) Spottiswoode and Russell were briefly acknowledged for their contributions in [Pincherle 1912,5,fn 22; 9,fn 30]. Recently Koppelman [1971,203-5] included a survey on Russell [1854a; 1861] mentioning Spottiswoode [1862] and Russell's two sequels to [1861]. It is nowhere mentioned, though, that Russell generalized Boole's theorem (47.13) [Boole 1845d;see 4.7] in the Proceedings of the Royal Society for 1865. This information was afforded by coming across an unknown survey by Russell, his article "Calculus of symbols" published in the English Cyclopedia as [1873]. In this article Russell mentioned the most important results of his papers, including aspects from Spottiswoode [1862] and Boole [1845d;1847c]. Upon Boole's work [1847c;5.8,(11)] Russell further contributed in the Philosophical Magazine for 1862 [Russell 1873,2023-26].

(9) Greer [1860a,148]. The verification of (59.33) consisted of the observation that by operating on both sides of (4) with  $\psi$ , the right-hand side will reduce to  $u$  -since from (2) and (3) it follows that  $\psi.M^m = m.M^{m-1}$ . There followed applications of (59.33) for  $\psi = \nabla$  and  $Q$ ,  $M$  functions of  $x,y,z$ ;  $Q$  the wanted function. There is a printing error in text: read Greer [1860a] instead of [1860] above and below (59.33).

(10) Between 1861 and 1870 several papers appeared in the Quarterly Journal on topics closely related to the calculus of operations, such as to fractional differentiation, Brinkley's  $\Delta^n 0^m$  numbers, definite integrals, Arbogast's derivations and others. Besides Greer and Roberts, other names that feature at that time are Scott, Blissard, Horner, Cockle, Harley and Walton.

## Section 5.10

(1) Kelland [1858,85]. The speech was delivered for the award of the Keith Medal to Boole for his memoir on probabilities [Mac Hale 1985,62; 7.1,text below (18)]. A Cambridge senior wrangler in 1834, Kelland succeeded Wallace in Edinburgh University in 1838 where he was unrivalled for his teaching abilities. Remembered for his enthusiastic reproduction of Hamilton's quaternions in class, he was also keen on acoustics, violin playing and natural philosophy.

(2) Boole did work on differential equations up till his death in 1864 [8.8] but the part of his researches pertinent to his general method were left in manuscript form, edited by Todhunter as [1865; see 4.6,(10); Mac Hale 1985,222-3]. Boole's concept of a universal calculus of symbols -his general method being a by product of it- was from 1847 onwards examined in the realms of logic [7.1;8.2,8.7-8.9].

(3) We have seen many instances in which beautiful or complicated theorems

either remained without any utility or were illustrated by artificially constructed differential equations, much as Babbage had constructed functional equations on purpose [(52.45), (52.48), 5.9, 5.2, 5.5, 4.7]. Of course there were exceptions, such as useful applications on definite integrals, analytic geometry and the calculus of variations [(52.46); 5.2, (11); 5.7].

(4) O'Brien [1852, 161].

(5) O'Brien denoted by  $x.y$  a distributive function of  $x$  and  $y$ . This symbolic form was "completely defined" by the equations

$$x.y + x'.y = (x+x')y \quad \text{and} \quad x.y + x.y' = x(y+y')$$

"just as the symbolic form  $a^m$  is completely defined by the equation  $a^m a^n = a^{m+n}$ " [1852, 165; see also Smith 1982c].

(6) It is impossible to list all the instances where terms such as "symbolic geometry", "symbolic mechanics" or "symbolic equation" appeared in the sense just described. Two examples are Goodwin [4.3, (6)] and Hamilton. After his study of Herschel's theorem in 1837 [see 2.3; (48.1); (52.44)] Hamilton applied the method of separation of symbols in connection with his calculus of quaternions. As his work is not further discussed in our thesis see references in [5.7, (11); 5.8, (17)] and Koppelman [1971, 207-8]; the latter touches upon his work on definite integrals via operators and his correspondence on this matter with De Morgan.

(7) On the similarity between Ellis's statements and De Morgan's own in [1836] see [3.6 text and (3)]. The sense in which Ellis applied the notion of operation to link a diversity of branches of mathematics could be methodologically parallelized with Gratry's "comparative science" adopted by Boole on different lines [8.9]. For a proof on the not satisfactory level of Ellis's analysis see Sylvester's advanced analysis cited in (12) below. Of course it is risky and superficial to compare two mathematicians of so different level, but Ellis could have gone at least further than De Morgan [1836]. The associative law was only hinted at by Gregory in [1843a, 238; see 4.4 text below (8); 7.3, (8)].

(8) Cayley's work on invariants is best summarized in Crilly [1986; 1988]. Further on Cayley see [3.2, text and (4); 5.7, (12)].

(9) Sylvester's work and correspondence with Cayley is discussed briefly in [Crilly 1986; 1988]. A correspondence with Crilly enriched my knowledge on aspects of Sylvester's methodology hinted at in this section. On my conjecture on the link between biology, morphology and the calculus of forms see [5.9, (2), (3)].

(10) On the theory of forms [(510.3)], see Kline [1972, 925-8] and (9) above.

(11) Cayley is mentioned in [Pincherle 1912, 5, fn 22] in his historical review of the calculus of operations only for the papers [1861] on Arbogast's

derivations. Surprisingly Sylvester is omitted. On Arbogast and his impact in the 1840's up to the 1860's see [3.9, (2); 4.8, (3); 5.9, (10)] and (12) below.

(12) Sylvester [1867, 49, fn]. On relevant speculations on notation of the differential calculus and on the form-matter distinction as a tool of mathematical and philosophical investigation, see [Brinkley in 2.2, (9); Herschel in 2.7; De Morgan in 3.6; 3.9 text and (9), (13), (16); Boole in 8.8 text above (3); 8.9, (8)].

## Chapter 6

### Section 6.2

(1) On the influence of Condillac, Degerando, D. Stewart and Locke upon the methodology of Herschel, Babbage, Peacock and De Morgan, see comments and references in [1.8; 2.7; 2.9, (3), (8); 3.4, (18); 3.5, (6), (8); 3.6, (9)].

(2) Van Evra [1984, 9-10, fn 21] is the only historian to my knowledge who mentions Kirwan and his apparent influence upon Whately. He points out in a footnote those historians who omitted Whately altogether, including in text most of the attempts to save Whately from oblivion in works published between 1967-1984 [1984, 15-18, fn 26]. The importance of Whately's work is recently stressed in [Dessi 1988, xi-xiv; Grattan-Guinness 1988b, 74; Merrill 1990, art. 1.1]. An interesting but neglected contribution in this direction is [Whately 1975], a reproduction of the second edition of Whately's Elements edited by McKerrow; the introduction written by the editor provides valuable information on the merits of Whately's work including commentaries by the latter's contemporaries as well as by recent historians.

(3) Information provided in text on Kirwan is obtained from the excellent full-length biography by Donovan [1850]. This biography contains interesting information on Kirwan's classification of manuscript work by Italian and English music composers, and draws an analogy between Lavoisier and Kirwan - hinting at the neglected role of Lavoisier's wife in the development of chemistry. Unfortunately, [on Kirwan [1807] the only remark included] is that it "appeared in two volumes octavo; it was intended for the use of students of the law, and was dedicated to the Chief Justice of the Common Pleas" [1850, xcvi-xcvi; on Lavoisier and music see xcv and ci respectively].

(4) J. Horne Tooke (1788-1856), a follower of Locke, was known for his pioneering work on universal grammar [Donovan 1850, xc-xci; see also 1.8, (2) under J. Bentham]. Smart composed his essay [1851; see 6.4 text and (4)] partly "toward completing what Locke and Horne Tooke left imperfect" [1851, 10].

(5) For detailed bibliographical references and information see [1.8, (2),



(7), (19), (20)] on Condillac and Condorcet, and [Dessi 1988, xvii; Merrill 1990, art.1.1; Van Evra 1984, 3-7] on Watt, Duncan, Locke and Aldrich; the latter's Compendium 1691 was the only English work on formal logic until Whately [1826]. De Morgan was to refer occasionally to these English logicians in his work [see 1858, 93; 1860b, 148, 177, fn 1, 180, fn 2]. The doctrines of the Port-Royal logic are extensively treated in [Auroux 1982, 1-48; Kneale 1962, 315-20; 1.8, (2)]. Auroux [1982] viewed Arnauld's Logique as foreshadowing De Morgan's and Boole's logic of classes. De Morgan would draw on the Port-Royal logic, calling it a work on language "which dares to differ, or to revive, at pleasure" [1850, 52, fn; see also 1858, 92-94].

(6) On Whately's life and work see [Dessi 1988, xiv-xxv; Merrill 1990, art.1.1; Whately 1975, Introduction; Van Evra 1984, 7-14].

(7) This distinction is similar with De Morgan's own between the "art" and "science" of algebra in 1835, later giving rise to "technical" and "logical" algebra respectively [see 3.4, text and (13); 3.9, text below (7)].

(8) Whately [1826, 14]. This passage illustrates the author's belief that formal logic investigates—as a science—the abstract patterns of argument which are common to all reasoning. Among the fierce opponents to the use of algebraic symbols in logic was D. Stewart [Whately 1826, 140; Van Evra 1984, 11].

(9) Whately [1826, 88]. Whately's nominalistic position raised objections among his contemporaries [see (10) below and 6.3, (11)]. On recent interpretations of this passage see [Dessi 1988, xxvii; Merrill 1990, art.1.1; Van Evra 1984, 12, fn 24].

→ These interpretations include speculations on whether "the mere force..." stands for "...form", here omitted as irrelevant to the scope of this introductory study. It is, nevertheless, quite probable that Whately did mean "force" as implied from a discussion on the distinction of "extent" and "intent" by De Morgan [1860b, 201] interpreted by "scope" and "force" respectively with an example.

(10) Bentham [1827; see 6.3] was <sup>ly</sup> large<sup>A</sup> devoted to a mild critical review of Whately [1826]. Among the books designed for students which followed Whately's Elements we note S. Hints Introduction to logic 1827 and B. Reynolds An abstract of Whately's logic with examination papers for the use of students 1874, published in Oxford and Cambridge respectively. On similar publications and on the critical reviews and new editions of Whately's book see [Dessi 1988, xxii-xxix; Whately 1975, Introduction; Van Evra 1984, 14-15; 6.3].

(11) Van Evra [1984, 15; (10) above]. Mill is not studied in this thesis.

## Section 6.3

(1) On J. Bentham and H. Tooke see [1.8, (2), (16); 6.2, (4)]. On Condillac's in-

fluence on Lavoisier see [1.8,(15)]. Further on the links between chemistry, logic and mathematics see chapter 9.

(2) Bentham [1827, viii-ix]. On the *Ideologues* see [1.8, (17)].

(3) The idea of the quantification of the predicate dates from Leibniz. On the pertinent work of Continental logicians during the period 1673-1803 see [Lewis 1918, 33-6; Venn 1881, 8, fn 1; De Morgan 1847b, 323-33]. Bentham was acknowledged for priority over Hamilton in [De Morgan 1858, 140, fn 1; Diagne 1989, 98-105; Lewis 1918, 36; Passmore 1968, 550; Styazhkin 1969, 148-150; Venn 1881, 8-9, fn 1]. Jevon's paper "Who discovered the quantification of the predicate ? " [1873] particularly stresses Bentham's role; see also (12) below.

(4) For reference on Hamilton's review of works on logic in 1833 see [Van Evra 1984, 15, 18]. On Hamilton's omission to acknowledge Bentham on this issue see Heath's introductory remarks in [De Morgan 1966, xvi] and Passmore [1968, 550]. See also (12) below.

(5) In 20th century historical works on logic by English logicians, Solly's name is noted only in [Lewis 1918, 36] by the statement "Hamilton's rather cumbersome notation [see (12) below] is not made the basis of operations, but is essentially only an abbreviation of language. Solly's scheme of representing syllogisms [see (9) below] was superior as a calculus".

(6) The only information we have on Solly's communication with his contemporaries are two letters he sent to De Morgan from Berlin. Dated 21.10.1847 and 11.11.47, these letters belong to the De Morgan manuscript collection in the University College Library with code number MS Add 97/5. Solly was able to keep in touch with activities on logic in England via the journal Athenaeum. Thus informed about De Morgan's engagement "in throwing the laws of syllogism into a mathematical form", he contacted him [see passage in text]. De Morgan replied immediately after receiving Solly's first letter, mentioning an instance from the latter's symbolical approach in the end of his Formal logic [1847b; see (9) below].

(7) It was "A friend [who] once suggested to the author that there was a great analogy between an abstract conception and an enveloping curve" [Solly 1839, 26]. Had Boole had been around Cambridge then, he would have been probably that "friend" since Solly's statement is very close to Boole's work on singular solutions of differential equations several years later [8.8, (6)-(7)]. Solly might have been in contact with any of the Cambridge graduates: Murphy, Hymers, Gaskin, Kelland, Sylvester, Gregory and A.J.Ellis [see 3.2 before 3.3]. Ellis was at the Middle Temple at the same time as Solly and he was equally interested in logic and language [5.10,(7); 8.4,(6)]. Other probable candidates for this "friend" might be Hymers, Gregory or Kelland [see 4.2.4.4 and 5.10,(1) respectively].

(8) Solly [1839,112]. As Solly was the first English scientist to use symbolical mathematical procedures in logic, we will incorporate a more substantial and extensive account of his work in a separate paper.

(9) De Morgan [1847b, 335-336].

(10) According to Mansel, Boole, De Morgan, Solly and Drobish were all four "guilty of one fundamental error. They represent thought as a species of algebra, instead of regarding algebra as a species of thought" [Mansel 1851, 97, fn; further on Mansel see 6.4, 6.7]. On Drobisch see [Styazhkin 1969, 156-9].

(11) Hamilton, like Solly, partly misunderstood Whately's nominalistic approach; see [Dessi 1988, xxv-xxvii Van Evra 1984, 15].

(12) The most detailed account of Hamilton's innovations and notation is given by Styazhkin [1969,150-56]. See also [Kneale 1962, 352-4; Laita 1979, 55-6; Passmore 1968, 120-22]. Bentham's and Hamilton's schemes of quantification are also amply described in [Diagne 1989, 147-150; Liard 1878, chap.3].

(13) See [Laita 1979,60-1; Nidditch 1962,34-6; Passmore 1968,128, fn; Styazhkin 1969, 156]. We have omitted a reproduction of Hamilton's notation [see comments and references in (5) and (12) above]. Specifically on De Morgan's comments see [1860c,258, text and fn2; 1850, 32-34;6.6].

(14) On the early stage of the dispute in 1847 see [De Morgan 1847b, 295-323; 1966, xvi-xviii; Laita 1979, 51-60].

#### Section 6.4

(1) Pagination from De Morgan's papers on logic will be provided from Heath's edition (with Introduction) [De Morgan 1966]. For comments on the Syllabus, cited as S, see [De Morgan 1966, xx-xxi; Halsted 1884, 2; Lewis 1918, 38, fn].

(2) De Morgan's work on the logic of relations has been discussed so far in [Halsted 1884; Hawkins 1979; Lewis 1918, 37-51; Liard 1878, 71-97; Merrill 1978; Kneale 1962, 427-428; Prior 1962, 141-156; Styazhkin 1969, 161-169]. But a first sustained study of the evolution of De Morgan's logic of relations has recently been published by Merrill [1990]. Merrill delves deeply into philosophical and logical issues and -in contrast with the references above cited- hints at certain connections between De Morgan's work on algebra and his work on logic. Viewing De Morgan's work through his eyes, Merrill devoted the last chapter of his book to a formalization of the logic of relations in modern standards. As we mentioned in [3.1,(1)] a study of the impact of the treatise on functions [1836] has not been carried out yet; on the importance of the role it might have had on De Morgan's logic see [Grattan-Guinness 1988b, 74-5].

(3) The two books written by followers of Hamilton are Thomson Outline of the necessary laws of thought, 1849 and Baynes Essay on the new analytic, 1850. As

Hamilton only occasionally published aspects of his theory, these two books are the most considerable records of his work on logic. Baynes also translated the Port-Royal logic [1662; see 6.2, (5)] into English. On these two logicians see further comments in [De Morgan 1966, xvii-xix; S<sub>3</sub>, 139-140, fn2, 145; S, 153, fn1, 177-8, fn1, 188, fn1; Mansel 1851, 114-121; 6.6, (10)].

(4) On B.H. Smart (1786-1872) and the scope of his essay [1851], which lacked the name of the author, see comments in [6.2, (4); Dessi 1988, xxviii text and fn 49].

(5) On Higman's letter, dated 4 March 1848, see [3.9 text and (3), (4)]. We believe that Mansel's, Smart's and Higman's views on FL form the best introduction to De Morgan's work. FL is not to be amply discussed in our study as superseded by De Morgan's later memoirs [see text below and 6.7, (4)].

(6) On a summary of De Morgan's innovations in FL see Heath's "Introduction" in [De Morgan 1966]. On De Morgan's not very successful attempt to reduce propositional logic to an algebra of classes and on the issue of probability see [Corcoran 1986, 6, 13; Hawkins 1979, 41; Prior 1962, 141] and [Grattan-Guinness 1988b, 76; Hailperin 1976; 1988, 153-167] respectively.

(7) We will not deal with such inferences in our work. A full analysis of (64.1) is carried out by Merrill [1977]. Further this matter is tackled in his book [1990, art.3.3, 4.5]. The drafts of this book were kindly sent to me by Merrill when I did my first study of De Morgan's work on logic proving to be of great help. Articles from this book will be quoted as complementary source of reference in due course. On certain issues, such as the "doctrine of the abstract copula" [1990, art.31] I follow his terminology.

(8) [S<sub>3</sub>, 116, fn]. The subtitle of [1839] declared that the book was meant for students of geometry.

(9) FM and PEF stand respectively for the form-matter issue [(35.1); 3.6, (3); 3.9, (9)] and Peacock's principle of equivalent forms [3.4, (18)-(21)].

## Section 6.5

(1) De Morgan introduces here implicitly the so-called doctrine of the "double copula". See text below and references in (2).

(2) Legendre was similarly an exception among Continental authors to apply syllogistic in his book Éléments de géométrie around 1794. De Morgan was informed that D. Stewart "who had a strong notion of the practical impossibility of presenting Euclid in a syllogistic form, never would believe that it has been done by Herlinus and Dasypodius" [FL, 286]. Further on historical remarks on the remarkable independence of geometry and logic, De Morgan's unsuccessful demonstration of Pythagoras's theorem and the philosophical issues underlying Reid's and Hamilton's views on inferences such as (65.5), see respectively

art. 1.2; 2.1, 7.3 in Merrill [1990] and [S<sub>4</sub>,217].

(3) Further on De Morgan [1839] and the à fortiori reasoning within this pamphlet and FL see Merrill [1990, art.2.2,3.1]. On composition of relations see text below, (7), and 6.8–6.9.

(4) [FL, 50–51]. This is the first explicit instance in De Morgan's logic where the FM issue appeared [6.4,(9)].

(5) On the PEF and De Morgan's use of it see [3.4, (19), (21); 3.6, (3); 3.7, (5); 3.8, (17);6.4, (9)]].

(6) An extensive discussion on the inferences (65.10)–(65.12) is carried out by Merrill [1990, art.3.2] who carefully reconstructs De Morgan's attempt to reduce them to traditional syllogism.

(7) [See S<sub>2</sub>, 57 and (6) above]. Further on De Morgan's interpretation of bicopular inferences in functional symbolism see [6.9,(3)].

(8) [S<sub>2</sub>, 59]. See also citations in 6.4 [text below (2)], 6.6–6.9.

## Section 6.6

(1) [S<sub>1</sub>,1]. In this paper, read in November 1846, De Morgan added in the end three months later: "Since this paper was written, I found that the whole theory of the syllogism might be deduced from the consideration of propositions in a form in which definite quantity of assertion is given both to the subject and the predicate of a proposition. I had committed this view to paper, when I learned from Sir William Hamilton of Edinburgh, that he had for some time past publicly taught a theory of the syllogism differing in detail and extent from that of Aristotle" [S<sub>1</sub>,17].

(2) On the restriction (66.5) see details in [S<sub>1</sub>,8–9,18–19;FL,144–46]. On Lambert's first conception of the principle (66.5) concerning the middle term see De Morgan's manuscripts with code number 775/358 [a source referred to in 3.9,(6)] and [Halsted 1884,4–7]. Hamilton initially claimed that De Morgan arrived at the numerical form after his own system (63.3) [S<sub>2</sub>,49,fn 1;see text below]. De Morgan refuted Hamilton's charges that he had drawn on Lambert's work in [1860c,263,fn 2]. Both De Morgan and Hamilton discovered Lambert's work around 1850 as implied from an overall consultation of the sources above mentioned.

(3) First of all, De Morgan introduced numerical propositions in S<sub>1</sub> only after providing these new concepts and the system (66.6). However, as indicated in S<sub>2</sub>, the numerical syllogism preceded the notion of contrary terms as far as its conception is concerned [see (6) below]. Secondly, it is a common characteristic of De Morgan's attitude not to exhaust a specific aspect of his theory in one memoir. Clarifications are added –together with minor or major alterations– in later writings, and for this reason a commentator can rarely

restrict his account to the material exposed in the specific memoir under study. Finally, the concept of universe and the consideration of limiting cases was among the subjects of discussions in his correspondence with Boole in the early 1860's. On the latter issue see [Smith 1982a, chap.6, particularly pp102-5].

(4) According to the second issue raised in (3) above, a full study of De Morgan's papers produced between 1847 and 1860 is necessary in order to properly understand and construct the table (66.6), due to his scattered clarifications. This remark is slightly exaggerating since De Morgan gives verbally most of the information needed in  $S_1$ . Nevertheless, the syllogistic scheme with contrary terms was given by him in  $S_1$  purely symbolically. As we will only briefly allude to the early notation in text below, we will provide it here. Let  $x = \text{not-}X$ ; then the system based on contraries was initially presented in the form

	A	$X)Y$		I	$XY$
(66.6)	a	$Y)X = x)Y$		i	$xy$
	E	$X.Y$		O	$X:Y$
	e	$x.y$		o	$Y:X=x:y$

[ $S_1$ ,4-5;FL,60-62]. On the form (66.6) in text see [ $S_2$ ,35-37]. The reader can further consult the condensed but helpful study of De Morgan's syllogistic [Lewis 1918,39].

(5) Let us see the change from (66.6) to (66.7) via the proposition a. According to the reading of A " $X)Y$ ", a in (66.6) is denoted by  $\text{not-}X))\text{not-}Y =$  [by the rules put forward in  $S_2$ ]  $X(.)\text{not-}Y=X(..(Y=X((Y, \text{ also read as } Y))X$ . Interpreted verbally, the latter gives proposition a in (66.7). In [Lewis 1918,39-40] De Morgan's rules of transformation are provided and some minor objections are raised in connection with the validity of the reading of (66.7). On De Morgan's system (66.7) see [FL,60-2; $S_2$ ,35-7; $S$ ,158-9;1860c,261-2].

(6) [ $S_2$ ,35]. This revealing passage on the priority of the numerical (66.2) over the arithmetical (66.6) system casts doubts on Laita's [1979,59] conjecture that "The quantification of the predicate arises in [ $S_1$ ] as a consequence of the study of contraries". The statement is erroneous in the sense that it does not fully convey the actual origin of quantification which is numerical and which therefore did not initially arise out of the concept of contrary terms. See also our comments on (3) above [Laita drew exclusively from  $S_1$  and FL overlooking De Morgan's clarifications in  $S_2$ ].

(7) Quite often the footnotes in De Morgan's writings cast more light on the actual evolution of his ideas than the text itself. However, they lack appropriate references: thus (66.8) is omitted -though implied- and so is the pagination from [1839]. This instance shows that De Morgan started contemplat-

ing non-Aristotelian forms as early as 1839. At that time Hamilton had just started lecturing on logic, but had not published any pertinent work, and Solly [1839] was totally unknown [6.3 text and (6)].

(8) The notion "arithmetical" was for the first time linked with the forms (66.7) in 1858 [S<sub>3</sub>,103,147,154; see also 6.4,(8);(67.5)]. In [S,154] he wrote that "the connection of name with name" belongs "to the arithmetical view of logic:there is in it result of enumeration of similar instances":as for example, in "Every X is Y" or "50 Xs are not Ys". There followed the notion of "universe" and "contrary" terms [S,156] and finally the system (66.7) as deduced from (63.1) by consideration of contrary terms [S,157-8]. See also (3) and (6) above.

(9) [S<sub>2</sub>,42]. Hamilton formed an exception among great logicians in not being mathematically trained. In 1836 he had claimed that mathematics incapacitates the mind for abstraction and generalization [Laita 1979, 49] and a similar attitude was taken up by his followers, particularly Mansel [6.4,(3);6.7]. De Morgan attacked Hamilton's lack of mathematical training which as a result revealed his non-familiarity with the processes of extension and consistency. See [S<sub>3</sub>,138-9;S<sub>5</sub>,289,274,fn ;6.7] and text below.

(10) [S,166 fn 3; see also 1860c,259]. In fact the controversy with Hamilton did not cease with his death in 1856; it was taken over by his successors up to 1863. De Morgan's final and successful attempt to restate properly the eight Hamiltonian forms was presented in [S<sub>3</sub>,280-310;see also De Morgan 1966,xxiii].

(11) [S<sub>2</sub>,45]. Singular mathematical forms, as examined via this historical approach, were discussed in [3.5,(3);3.6,(1),3.9,(10)]. Hamilton's application of "some" led apparently Boole to the conception of the indefinite symbol  $v$ , often denoted by  $0/0$  [Laita 1976,153;1979,55;7.7;8.5]. On algebra as a paradigmatic science in the development of logic see 6.7.

(12) He wrote in [S<sub>2</sub>,46]:"If the cumular proposition can, generally speaking, only be proved by the help of the exemplar, it follows that the exemplar proposition must precede in order of thought: and it is justifiable to propose it as the basis of a logical system. The distinction of the two modes exists in every language in which I can form the sentence: if there be one in which both forms do not exist, the study of the minds of those who speak that language would be curious". This quotation reminds us of the arguments by Solly [6.3 text below (6)] and Boole [8.4,(2)] on the universality of the laws of thought. Further on the issue of exemplary quantification see [S<sub>3</sub>,125;S,166-9] and the correspondence with Boole [Smith 1982a,88-90].

(13) This interesting passage from [S<sub>5</sub>,324,fn 1] confirms our claims on the algebraical origin of De Morgan's notation. It further reveals his overall in-

fluence as an algebraist and a teacher on the shaping of his logic (see further instances in (6), (8), (9) and (11) above as well as in 6.7-6.9]. Calling in text above (66.1) as "algebraic" the origin of the system (66.6) and its notation, I used the term "algebra" in the sense of generalized arithmetic. However, algebra as an abstract structure was also of considerable influence as we have already seen in [6.5, (4), (5)] and further to see in 6.7 and 6.9.

(14) On the "zodiac" produced by the symbols (66.15) see [S<sub>3</sub>, 133.4, fn 1; S, 163]. A reader interested in De Morgan's peculiar groupings of these symbols in octagons and dodecagons may consult his annotated copy of FL with code number 776/1 and his notes on FL 775/355 [see also (2) above]. On De Morgan's application of the "powerful language" of the symbols (66.15) in his classification of syllogistic forms see [S<sub>3</sub>, 89-138; S, 158-201; S<sub>3</sub>, 279-84, 305-10].

(15) First of all De Morgan did not cease to refer to his early notation [see (4) above] in his writings from 1850 onwards. Secondly, he often employed the same symbol, e.g. "X).Y", by attributing to it different interpretations. Finally, he invented variations of the symbols listed in (66.15), such as

))., ))., (o), |o|, (')', (')', |o|, |.:

and so on which are not easy to decipher. For example "X)o)Y" stands for "Both X))Y and X).)Y", while its contrary [contradictory] proposition is denoted by "X(,(Y". On these symbols see [S<sub>3</sub>, 123-125; S, 166, 197; S<sub>3</sub>, 279, 326]. On comments on De Morgan's notation see [Bochenski 1970, 297; Corcoran 1986; Lewis 1918, 38-40, fn 65; Liard 1878, 97]. See also our comments in 3.8.

(16) As we mentioned in text above, De Morgan did not develop extensively the numerical quantification on the grounds that numerical syllogism rarely occur in practice. Nevertheless, he did introduce syllogism of "transposed" quantity" as useful in application. For example take the instance "For every Y there is an X which is Y" or "For every man in the house there is a person who is aged" and "some of the men are not aged"; from the two latter premises it follows that "some persons in the house are not men" [S, 172; S<sub>4</sub>, 242-45]. Thus De Morgan made explicit the form

"( $\forall$ Z) ( $\exists$ X) (XRY)" where  $\forall$ ,  $\exists$  the known modern quantifiers, R a binary relation.

(17) These citations are from [S<sub>3</sub>, 138] and [S, 150] respectively. De Morgan is rather vague in his acknowledgements as he does not explain how exactly he benefited from Hamilton. We feel that our account has clarified to the reader that the dispute on the whole was the best opportunity for De Morgan to revise and develop his early theory, besides amending Hamilton's system.

## Section 6.7

(1) On the arithmetical and algebraic influences noticed so far see respec-



tively [6.4,(6),(8);6.6,(6),(8),(13)] and [6.5,(4),(5);6.6,(11),(13)]. On an overall influence from general mathematical notions and process see [6.6,text and (9)].

(2) [Mansel 1851,96-7].  $S_2$  was overlooked in Mansel's review. On earlier instances from Mansel's accusations see [6.3,(10);6.4,(3);6.6,text above (66.15)].

(3) Boole's Mathematical analysis of logic [1847a] —to be studied in chapter 7 —makes use of sophisticated mathematical theorems, such as Taylor's theorem of expansion. On Solly's application of mathematics to logic see [6.3,(5)-(9)]. De Morgan was aware of both Solly's and Boole's work on logic by 1847; nevertheless he did not allude to their work in  $S_3$  in connection with what he called "mathematical logic" [see (14) below and Corcoran 1986,69].

(4) The manuscripts of the old "Preface" of FL have code number 775/353 [see 6.6,(2)].

(5) [ $S_2$ ,26]. De Morgan had made several similar —occasionally groundless— statements in his [1836] based on his characteristic optimistic historical approach [3.5,(3);3.6,(1) and text;3.7,(3)-(5)]. In connection with logic see [6.6,(11),(12)]. In the case of (14) below his prophecies came true.

(6) De Morgan's analogy is shown to be incorrect [see Corcoran 1986,70;6.9]. The scope of these analogies is summarized in [ $S_2$ ,28] "These assimilations may appear ludicrous, but it will be presently seen that the ideas they suggest may help to free logic from being to the higher processes of thought what algebra would have been to its present state if it had discussed no forms more complicated than  $x=y$ ,  $y=z$ ,...".

(7) [ $S_2$ ,50]. This citation is an immediate outcome of his work on symbolic algebra [1849c,89-105]. On the latter work see [3.9,text and (11)-(12)].

(8) [Mansel 1851,98]. According to Mansel the result of every thinking process "possesses in every case a matter and a form" [1851,100]. De Morgan would be less vague and more precise than Mansel in (67.2) below in showing how form can be gradually excluded from matter. There are many similarities but also substantial differences in the approaches of the two logicians on which we will not focus as it is principally De Morgan's approach which interests us here [see also (9) and (10) below].

(9) As shown recently by Merrill [1990,chap.4] Mansel's attack on De Morgan's dictum on the materiality of the copula was not totally groundless. Merrill investigates thoroughly the FM issue in their work focusing exclusively on the theory of the [abstract] copula; in other words, he omits any discussion of the numerical syllogism, the system of contraries or the issue of probability [see also 6.4,(6),(7)]

(10) [ $S_3$ ,77-78]. On this process see [1836,art.14,18;3.6 text and (3)] where

it first appeared in his writings. In addition we read in the opening of  $S_3$  "The form or law of thought [...] is detected when we watch the machine in operation without attending to the matter operated on. The form may again be separable into form of form and matter of form: and even the matter into form of matter; and so on. The modus operandi [see (11) below] first detected may be one case of a limited or unlimited number, from all of which can be extracted one common and higher principle, by separation from details which are still differences of form" [ $S_3$ ,75].

(11) [ $S_3$ ,81-82]. The term "modus operandi" is borrowed from his [1836,art.13;see 3.6], further applied in his manuscripts on the calculus of operations in 1854 [3.9,(13)]. The necessity of new symbols to show the effect of the "modus operandi" is stressed in the latter work [see also (17) below]. De Morgan's analysis of reasoning is reconstructed by Merrill [1990,art.4.3] in a manner similar to that followed by De Morgan in (67.2) for the analysis of judgment. On De Morgan's later comments on the FM issue see [S,183,fn 1].

(12) [ $S_3$ ,131,fn1].

(13) [ $S_3$ ,82]. Similar views were expressed earlier by Kirwan in 1807 and De Morgan in 1835 [see 6.2 and 3.4, (14) respectively]. On Boole's corresponding views see [7.1;8.2;8.7-8.9]. On similar lines, as in the passage from  $S_3$  above cited, De Morgan defended Mansel's accusations on Boole's logic [see 6.3,(10)] in [1860c,255-6] by saying "The anti-mathematical logician says that it makes thought a branch of algebra, instead of algebra a branch of thought. It makes nothing; it finds: and it finds the laws of thought symbolized in the forms of algebra".

(14) [ $S_3$ ,78,fn 1]. This is the first instance where the term "mathematical logic" appeared in De Morgan's writings. As we saw in 6.2-6.3, Kirwan, Whately, Bentham, Solly and Boole had all paid to a minor or major extent a "joint attention" to these two sciences. De Morgan, though, confined to mention only Leibniz and J.Bernoulli in this context in [ $S_3$ ,77,141,fn ]. He claimed that the latter had imported "the algebraic mode of thought into logic" based on the "Port-Royal Logic [6.2,(5)]". See also (3) above and 6.9.

(15) [ $S_3$ ,87].

(16) [ $S_3$ ,318,fn ]. See also [6.6,(13)-(15)].

(17) [ $S_3$ ,87-88]. See also [(10),(11),(16) above]. Further on De Morgan's views on notation see [S,163,203;1860c,258-9; $S_3$ ,316-324;6.9].

## Section 6.8

(1) [ $S_3$ ,107]. On De Morgan's notation of combined relation "AB" see [6.6,(11)]. His reasoning, initially shaped to this direction in  $S_2$  [6.5], would be repeated by appeal to Euclid's geometry in  $S_4$  [see text and (13)]

below]. Further on the influence from the calculus of functions see 6.9.

(2) [S<sub>3</sub>,119-120]. A slightly modified version of these definitions (including transitive relations) devoid of symbolic notation was to be given two years later in [S,185-186]. "Onymatic" and "non-onymatic" was a heuristic terminology invented by De Morgan in S<sub>3</sub> so as to distinguish between theoretical, general or formal (traditional) logic and applied, particularate or material logic respectively. The necessity for this distinction stemmed out of his discussion of the issue of the abstract copula in [S<sub>2</sub>,68]. In [S<sub>3</sub>,96,fn ;S,188,fn 1] he claimed that "when the logician speaks of the distinction of form and matter, he means the distinction of onymatic and non-onymatic". Further in S<sub>3</sub> [1863,304], a paper devoted solely "on various points of the onymatic system" he stated "The logicians have admitted only one idea of relation: the connexion between terms as terms: I call the system thus produced by the name of onymatic". On a reformulation-interpretation of this definition see [Hawkins 1979,35-37]. Examples of non-onymatic inferences are the numerically definite syllogism (66.1)-(66.4), the oblique inference (64.1), the bicopular syllogism (65.12) and, the transitivity of equality as in (65.5) and finally specific relational syllogism. On the formal character of general relational inferences see [6.9, text above (19)].

(3) The only exception is theorem K [(68.27)] introduced in S<sub>4</sub> and referred to in [S,186] as well as in other journals and letters [see Merrill 1990, chap.5, fn 10]. Consultation of De Morgan's manuscripts and papers reveals no information on how he came to produce S<sub>4</sub> nor why he did not develop his relational theory further [see also 6.9].

(4) De Morgan had met with numerous instances of bad logic in his lifetime, besides the "humiliating" conclusions "which the instruction of young minds during more than thirty years has forced upon me" [S<sub>4</sub>,210]. For the first time he provided a review of the little work carried out in the field of relations in [FL,227-231]. In [S<sub>4</sub>,208-210] he confined himself to Aristotle and Ockham. On the pertinent, but far from fundamental contributions, by Galen, Leibniz, Jung and Ploucquet see [Bochenski 1970,375; Kneale 1962, 344, 428; Styazhkin 1969,161].

(5) De Morgan claimed that logicians' restriction to ordinary syllogism is equivalent to a mathematician's restriction of arithmetic to counting, thus calling the former "the logician's abacus" [S<sub>4</sub>,215-7;S,179,fn 1].

(6) The properties (68.11) were initially introduced in FL and were so named in S<sub>2</sub> [6.5 text and (4),(5)].

(7) "Should any one deny this by producing two notions of which he defies me to state the relation, I tell him that he has stated it himself: he has made me think the notions in the relation of alleged impossibility of relation"

[S<sub>4</sub>,218]. Further on this point, which reveals a bent to psychologism, see [Hawkins 1979,40].

(8) [S<sub>4</sub>,221,fn 2]. The spicular notation in (68.17) is borrowed from his syllogistic notation (66.15) introduced in S<sub>2</sub>. There is a big confusion with De Morgan's inconsistent and imprecise notation. He uses "X" and "Y" to stand indifferently for singular terms (as in unit syllogisms (68.35)) and for class terms in quantified syllogisms [(68.37)]. Moreover, in the latter case the question arise as to what does "LX" stand for. Apparently "LX" denotes a relation-class composition, similar to relation-relation composition "LM" [see further Merrill 1990,art.5.5].

(9) According to [Hawkins 1979,47] "The use of the little superior and inferior accents [in (68.15)–(68.16)] by no means accords with the importance of the notions symbolized by means of them". Further on De Morgan's complicated notation which partly was the cause for his difficulty to carry on his new relation theory see references given in [6.4,(2) and 6.6,(15)].

(10) In our account we keep close to De Morgan's own demonstrations providing additional information implied by him in square brackets. The reader can further consult [Hawkins 1979,47–49] on theorems (68.29)–(68.26) where De Morgan's verbal formulations are interpreted in modern set-theoretical notation. A complete axiomatic reformulation of De Morgan's logic of relations is carried out by Merrill [1990,chap.8]. On the property (68.24) see also [6.9, text and (11)].

(11) Our demonstration of theorem (68.27) is partly based upon Merrill's semi-axiomatic presentation in [1990,art.5.2]. We have omitted the principle "If  $R||S$  then R may be replaced by S in any formula", as its use is widespread in De Morgan's logic of relations that its employment seems evident to be stressed [see also (10) above].

(12) On theorem K and our claims so far see (3),(10),(11) above and [Prior 1962,153–4,275] –particularly on its connection with the syllogistic figures Baroko and Bokardo.

(13) [S<sub>4</sub>,228].

(14) Among the difficulties which appear in the reduction of quantified to unit syllogism is De Morgan's vague and complicated notation. See also (8) above.

## Section 6.9

(1) [S,179,fn 1]. De Morgan had hinted at the significance of the admission of relation in 1850 [6.5,(8)]. By 1860 he was, however, much more confident about the power of the logic of relations over the limited scope of traditional logic [see also 6.8 text, (5) and (13)].

- (2) On Boole's refutation of the reversibility of algebraic operations see (73.16)–(73.17). On De Morgan's analogy between algebraic elimination and logical inference see [6.7,(6); 6.9,(3); (69.28)].
- (3) [S<sub>2</sub>,56]. Notice De Morgan's application of separation of symbols, a method initially employed by him in 1836 [see 3.6–3.8; 3.9,(2),(6),(8)]; see also (16) below.
- (4) [S<sub>3</sub>,107; full passage cited in 6.8,(1)].
- (5) [S<sub>2</sub>,55]. On his discussion of logical oppositions in S<sub>2</sub> and S<sub>3</sub> see details in [6.7 text and (5),(12)].
- (6) De Morgan's statement, "a direct and inverse relation always exist", might be regarded as a vague formulation of "the existence condition" supplied by Merrill [1990, art.8.10] in his formal reconstruction of the logic of relations. On a systematic account of the reading of the arithmetical system (66.7) when "is" is replaced by either of the two "transitive converse" relations "gives to" and "receives from" see [S,174–5].
- (7) The thirty-two valid syllogisms of the form (69.3) are given in [S,192–3]. Surprisingly the form (69.4) is absent in S. On "genus" and "species" regarded as "converse relations" see [S,191].
- (8) [S<sub>3</sub>,134,fn 1]. The treatise [1836] is nowhere referred to in De Morgan's logical writings. The passages from S<sub>3</sub> cited in (4) and (8) above are the only instances in which he alludes to a direct influence from his study of functions. The passages from S<sub>4</sub> cited in text and (9) below served apparently merely for illustration.
- (9) [S<sub>4</sub>,227]. On this passage see comments in [Merrill 1990, art.5.3]. See also (16) and (17) below.
- (10) Sylvester was apparently the first to raise a similar analogy to that mentioned above (69.13) when he wrote: "The operant, sign of operation, and operand form a triad somewhat analogous to the subject, copula and predicate of the logicians" [1866,472,fn]. On Sylvester's peculiar terminology within the calculus of operations see further [1866,461,fn; 462,fn; 1867,48–49,fn and 5.10, (12)]. On the analogy (iii) in (69.14) see [S<sub>4</sub>,222,fn 1; Hawkins 1979,44].
- (11) Herschel had hinted at the property (68.15) [(24.18)] in 1813, while Murphy was the first to establish it rigorously in 1837 [(33.37)] independently of De Morgan's allusion to it in 1836 [(69.27)]. See further text and (14) below.
- (12) On the partial truth of De Morgan's statement (69.26) see [6.8, text below (68.31); Merrill 1990, art.5.3, art.8.6].
- (13) The transform " $\varphi\varphi^{-1}$ " was amply used by Babbage and Herschel after Maule's suggestion in 1814 [2.5,(3); 2.6,(9)]. De Morgan followed on their

lines in 1836 [see 3.5,(10); 3.6,(15)]. On the form " $FF^{-1}$ " see particularly [1836, art.156,art.241; 3.7].

(14) [1836, art.244].

(15) [ $S_4$ ,236,fn 1]. The editor, Heath, of De Morgan [1966] has added this passage in square brackets. Added by De Morgan in 1864, this passage hints at an influence from [1836; see pertinent instances in 3.6, text below (36.31)]. All the citations given in (4),(8) and (15) from De Morgan's logical writings have escaped the notice of his commentators, especially Merrill [1990]. The latter is the only one so far to touch upon the mathematical origin of the logic of relations [see 6.4,(2)]. Nevertheless, a comparison between [1836] and  $S_2, S_3, S_4$  has not been carried to my knowledge so far.

(16) Given the detailed study of inverse functions in [1836] and his wider knowledge of inverse operations after Murphy [1837], it is quite astonishing that De Morgan did not pursue a deeper inquiry in inverse [converse] relations [on his knowledge of the calculus of operations and his own research in that field see: 3.3,(18); 3.9,(2),(6),(8); 3.6,(6),(8); (39.5)]. A similar observation will apply to the case of Boole [1854] in connection with logical division [see 8.5;8.7,(4)-(6);8.8]. Venn [1881,xxi] is very close to De Morgan [1849c, 93,fn -see text above and 3.9 below (11)] when he wrote "Given that 'All X is Y', what is known about Y in relation to X?". Venn then states the same question in the generalized form "Given the relation of one class to another, find the relation of the second to the first" appending in a footnote its mathematical expression: "Given  $x=f(y)$  we express the inverse in the form  $y=f^{-1}(x)$ ",

concluding that "there are such restrictions and simplifications upon all logical functions as to render a general solution perfectly feasible" [1881, xxi,fn 1]. Venn's comments further confirm the functional origin of De Morgan's logic of relations but do not explain the latter's limited study of converse relations [see also (17) below].

(17) Venn devoted [1881,400-403] to what he called as "Logic of Relatives" referring to De Morgan and Lambert. He denoted  $LLA$  by  $L^2A$ , distinguished between  $L_1L_2A$  and  $L_2L_1A$  and touched upon inverse relations observing that " $L^{-1}LA$  may  $=A$  simply, or it may have  $A$  as one only out of a possibly infinite number of solutions" [compare with our comments from  $S_4$  in text below (69.26)]. Without any explanations, references or proofs, Venn incorporated the property (69.16) and introduced the identity relation by letting  $L$  represent "contradiction of" and hence deducing that  $L^2=L^4=\dots=1$ . Due to the vagueness of the "conception of a Relation" and the insolubility of "the inverse processes", Venn concluded that "the attempt to construct a Logic of Relatives seems to me altogether hopeless" [1881,403].

- (18) On our tentative distinction between mathematical examples referred to merely for illustration and similar examples, which must have served for suggestion see [6.7, (1), (5), (6), (10)–(12); (8) above]. On the process of elimination in the calculus of functions see [2.5, (13) and 1836, art. 192; 3.7, (2), (8)].
- (19) [S<sub>4</sub>, 241]. Lewis [1918, 51] and Merrill [1990, art. 7.4] are among the few commentators on this passage. On our arguments against their interpretation of "here" as S<sub>4</sub> see text below.
- (20) [S<sub>3</sub>, 105]. On representative samples of De Morgan's open question and historical approach see [3.5, (3); 3.6, (1); 3.7, (3)–(5); 3.9, (10); 6.2, (5); 6.6, (11); 6.7, (5), (12); 6.9, (15)].
- (21) [1865, 181]. On other instances where De Morgan alludes to logical notions and procedures in this paper see [1865, 146, 168, 169, 171, 177, 179, 180, 186; 3.9, text and (14)–(16); (22) below].
- (22) [1865, 180]. See also (21) above and last page of 3.9.

## Chapter 7

### Section 7.1

- (1) See [Harley 1866a, 5–7; MacHale 1985, 5–16; Neil 1865, 82–84, 88]. Both Neil and Harley knew and admired Boole. Neil's is the first biography written of Boole's and is little known. Its most striking element is the fact that he relates Boole's work to De Morgan's in logic, drawing on De Morgan's comments on Boole. Harley attempts a more extensive account on Boole's life, paying attention to his relationship with Gregory. At the end Harley includes also his own interpretation of Boole's work in logic. Harley wrote later interesting remarks on Boole's works in his reports to the British Association for the Advancement of Science. See his [1866b, 1870, 1881]. MacHale presents to us in his book a full scale biography giving a brilliant portrait of Boole elaborated with many interesting facts of his life, unknown so far. However, the book lacks an accurate conception of both Boole's mathematical background and his methodology in logic. See reviews by Laita [1985] and Grattan-Guinness [1985b]. MacHale's book is vividly marked by his intention to link Boole's work with the computer's science. Under this interpretation much of Boole's historical background loses its significance [see (22) below]. Other interesting biographies are given by [Jourdain 1910, 332–352; Kneale 1948 and Styazhkin 1969, 170–177]. A most recent biography, drawing mainly from MacHale [1985], is given by Diagne [1989]. For comments on this book see [8.6, (6)].
- (2) Poems by Boole are discussed in [Laita 1980, 50–52] in connection with

the extralogical sources of his work. A full account on Boole's poetic talent is given in [MacHale 1985, chapters 1, 12]. Another contemporary mathematician, W.R.Hamilton, shared love for both poetry and mathematics. MacHale presents an excellent comparison between the two men who shared a lot in common but had scarcely any friendly contact [1985, chapter 13]. We should also mention here J.Sylvester [see 5.10 text and (9)]. The phenomenon of mathematics and poetry being intertwined in the same person is not rare in history. However, I would like to refer to a relatively recent figure, P.Valéry (1871-1945). The analogies in Valéry's "Introduction to the method of Leonardo da Vinci" [1894] with Boole's philosophy of logic are striking. See [Valery 1977, 33-93, 136-165; Hackett 1955, 80]. The concept of unity and symbolism in general is tackled on similar grounds. A study of the interrelation between mathematics and literature would be of great interest for the history of thought. Not to mention also E.A. Poe (1809-1849) who himself admitted the purely mathematical use of language and reasoning in his writings [Poe 1986, 482]. With Boole the influence was both ways. In fact, parts of purely mathematical writings contain, as we shall see, a charm and beauty that only a poet possesses.

(3) We will hereafter cite [1847a] and [1854] as MAL and LT respectively. On Latham and Becher see [LT, 401 fn; MAL, 5 fn; 7.1,(3); 7.2,(2) and 8.2,(2)].

(4) Mary Boole recalls that her husband told her that while walking in the fields he saw as in a flash the foundation of all his future discoveries [M.Boole 1972, 61]. On Boole's influence from the Hebrew religion see MacHale [1985, 17] and Dummer in [M.Boole 1931, viii]. Dummer wrote the Preface of Cobham's edition of M.Boole's Collected works. The role of religion in Boole's logic is discussed in [Laita 1980; Grattan-Guinness 1982, 34,39-41; MacHale 1985, chapter 14].

(5) According to [Styazhkin 1969, 170-1] through this comparison Boole was led to a serious reflection on "abstracting from physical facts and the data of ordinary conversational speech and making a transformation to some system of effectively constructed symbols; such a system would have to be operative according to certain inherent laws and would have to be self-sufficient".

(6) Representative passages from Newton's address are cited and discussed in [Laita 1980, 52-53; 1982, 2; MacHale 1985, 34-8; Neil 1865, 85-8].

(7) "This theorem [(45.5)] was first published in the Cambridge Mathematical Journal (1st series, vol.ii, p114), in a memoir written by the late D.F.Gregory, then editor of the journal, from notes furnished by the author of this work, whose name the memoir bears. The illustrations were supplied by Mr. Gregory. In mentioning these circumstances the author recalls to memory a brief but valued friendship" [1859, 381 fn; 1877, 391 fn]. Some letters ex-



changed between the two men are cited in [Harley 1866a, 8-17].

(8) A great portion of the correspondence between the two men --covering over 90 letters-- is edited by [Smith 1982a]. Comparison between their logics is provided in Corcoran's review of Smith's book [Corcoran 1986].

(9) Gregory was unable to go through Boole's manuscripts of MAL due to illness [MacHale 1985, 59].

(10) W.Thomson (1824-1907) is widely known as Lord Kelvin. His correspondence with Boole is cited and discussed by Smith [1984c]. Among the topics discussed in their letters feature mainly aspects of differential equations, including the Laplace equation and infinite integrals. Boole's paper "On the equation of Laplace's functions" was published in 1846 in the Cambridge and Dublin Mathematical Journal. Thomson assumed the editorship of this journal and Boole was used as referee for papers submitted in it. On Boole's letter to Thomson about Graves's offer see [Smith 1984c,14]. See also [MacHale 1985, 67-8].

(11) Boole needed testimonial letters for a post at Cork. Among those scientists that sent such letters were De Morgan, P.Kelland, C.Graves, A.Cayley, R.L.Ellis and W.Thomson [MacHale 1985, 76-77].

(12) Laita has extensively dealt with the influence of both Hamilton and De Morgan in the genesis of Boole's logic in his thesis [1976,145-159] and in the paper [1979].

(13) On the appearance of the now rare book see also [Harley 1866a, 19; Kneale 1948, 152; MacHale 1985, 71]. Neil went on to characterize Boole's work as "distinctly mathematical", in contrast to De Morgan's system which he described as one "constructed on the ideas of logic already received" but at the same time as one distinguished by the use of a symbolic language which, though not mathematical, "could never have been invented except by a mathematician" [1865, 91].

(14) According to Neil, Hamilton also called Boole "a very acute philosophical mathematician" and spoke of MAL as "a very able" work [Neil 1865, 91].

(15) Prior to this, Boole cited Mill: "Whenever the nature of the subject permits the reasoning process to be without danger carried on mechanically, the language should be constructed on as mechanical principles as possible, while in the contrary case it should be so constructed, that there shall be the greatest possible obstacle to a mere mechanical use of it" [MAL,2].

(16) Few commentators on Boole have paid the attention that the passage above in text deserves. Neil quoted it in his biography in order to assure the readers of both MAL and LT, who had found these works "impractical and impossible", that Boole had "no desire to introduce the x-y-z-ity into speech or writing". He then went on to say: "It is as an educational agent he proposes his scheme, as a gymnastic training that we press its study" [Neil 1865, 170].

This passage is also quoted in [Hackett 1955, 83] in connection with Boole's deep interest in education. It is a surprise not to find it in [MacHale 1985].

(17) On the professorship at Cork see interesting details in [MacHale 1985, Chapter 5].

(18) To prevent misunderstanding, what I mean by that relates to our discussion above in connection with MAL. For Boole wrote in the "Preface": "If the utility of the application of Mathematical forms to the science of Logic were solely a question of Notation, I should be content to rest the defence of this attempt upon a principle which has been stated by an able living writer" and Boole went on to cite Mill [see (15) above]. Symbolic notation is helpful for proving the existence of a universal calculus of symbols which embodies both mathematical and logical sciences and not for showing that logic is applied mathematics. On Boole's study on the nature of symbolical notation see [8.7, 8.8].

(19) Among them R.L.Ellis, Harley, C.Graves, De Morgan and Neil. Evidence for that is shown in their respective papers and comments on Boole's work [see 7.4, (6); 8.4 and M.Boole 1972, 61-62].

(20) Harley and Lewis regarded LT as presenting the science of logic developed more fully and systematically than in MAL; see their [1881, 559; 1918,52] respectively. In [Kneale 1962, 406] we read "This work [LT]... is often regarded as Boole's masterpiece. The chief novelty of the book is its application of his ideas to the calculus of probabilities. This is well done; but for the rest, the book is not very original". In LT certain key points that feature emphatically in MAL are presented in a less emphatic way. We will discuss this matter later on in chapter 8 but for reference see [Bryant 1888, 192-3; Hailperin 1984, 40].

(21) In fact Boole's algebra is not exactly what we nowadays call "Boolean algebra". See [Hailperin 1981].

(22) MacHale overstresses Boole's connection with computers in his book [1985, xiii, 6, 72, 136, 198, 210, 220-221]. It is indeed to the dismay of a reader to come across passages as the following "One can just imagine the great delight with which Boole would have greeted present - day models of the human nervous system, portraying it as an electronic network whose components obey his "laws of thought" " [1985, 198]. Our belief in this thesis is that what Boole meant to do and did is only implicitly linked with mechanization of rational thought.

(23) Besides the paper cited in text, Boole's interest in education - mainly in connection with an essay he wrote which was not published - is discussed in [Hackett 1955 and MacHale 1985, 23-25; 162-3]. The essay - code number E<sub>6</sub> - belongs in Boole's manuscript collection 782 in the library of the Royal

Society. A passage from it - reminiscent of the one quoted from MAL - runs as follows: "It is the discipline of patient study that invigorates the mind... I conceive that they who aim to supersede by the perfection of mechanical teaching all strenuous efforts on the part of the pupil do really though unconsciously labour to make superficial scholars and shallow men" [Hackett 1955, 83].

(24) The "Preface" of this book, code number W<sub>3</sub>, is transcribed and belongs to the Royal Society collection. A variety of manuscripts of Boole's relevant to his philosophy of logic are cited in [Hesse 1952; see also 8.7].

(25) "Milne-Thomson, in a major text-book on finite differences published in 1933, has sections on Boole's symbolic method, Boole's iterative method and Boole's canonical forms, as well as a number of theorems of which Boole's are special cases" [MacHale 1985, 222]. A.R.Forsyth, in his "Preface" to A treatise on differential equations, first published in 1885, pays special attention to Gregory's Examples [1841], De Morgan's [1842c] and Boole's symbolical methods as in his [1859] and in the supplementary volume edited by Todhunter from Boole's manuscripts in [Boole 1865]. See particularly the chapter on symbolical methods in [Forsyth 1914 (4th edit), chap 3; 197-206 (on the earth figure equation)]. Most of the problems solved or proposed for solution in Forsyth's textbook, which is still in print today, are drawn from exam problems in Cambridge. Forsyth was a pupil of A.Cayley. See [Roth 1971, 230-33]. On Boole [1859] see also 8.8.

(26) Gratry wrote his Logique [1855], cited as [1944] independently from Boole's respective work. When Boole came across Gratry's work he approved it, for, the last chapter of his LT greatly coincided with the spirit of Gratry's book, the concept of unity given primary importance [see 8.8-8.9].

## Section 7.2

(1) In Boole's vocabulary to "elect" means to select mentally [MAL, 4-5; LT, 42-43]. The word is mostly used in Boole's early work. What he called as "elective equation" often appears later under the name of "logical equation" [LT, 16]. Most probably this change was due to the fact that in MAL variables x,y,z... stand for elective symbols whereas in LT for classes. See also text and (4) below.

(2) The view that a symbolic treatment of language can help to the formation of a philosophical language - which goes at least as back as Leibniz - is well expressed, wrote Boole, in Blanco White's Letters. White (1775-1841) was a liberal theologian born in Spain. Letters most probably are his Letters from Spain 1822. He was a close friend of Whately. In the same footnote Boole refers to Becker's Grammar and Latham's First outlines of logic applied

to language 1847. [MAL, 5,fn; see also 8.2, (2)]. It is quite probable though that Boole was also influenced by Whately who held that "Logic is entirely conversant about language" [6.2]. In fact, in some manuscript notes, which we will cite with code number [N<sub>1</sub>-N<sub>27</sub>] with probable date around 1848, Boole wrote the following: "Dr Whately has expressed the opinion that man is not distinguished from the lower animals by the faculty of reason but by the employment of signs as the instruments of reason. If this view is a correct one it is a question of great speculative importance to determine what are the essential laws of language as an instrument of reason" [N<sub>7</sub>]. The above quotation is from a loose notebook which has no classification mark and we cite it only by its pagination. It belongs to the Boole - manuscript collection of the Royal Society London with general classification mark 782. About my conjecture for the probable date of these notes see 8.1.

(3) On Condillac and Whately see 1.8 and 6.2 respectively. Boole's quotation is from the notes mentioned above in (2), page N<sub>7</sub>.

(4) This distinction was paid particular attention to by S. Bryant. She insisted that all symbols in Boole's logic should be read as symbols of operation. She claimed that Boole himself said this "with reference to his class-logic symbols, but not so clearly in his Laws of Thought as in the little-read and delightful treatise, The Mathematical Analysis of Logic" [Bryant 1902, 107; 1888, 192-3]. Even in COL [1848b] Boole identifies  $x$  with the class  $X$ . He also says that every elective symbol represents a class and that a proposition is "a relation between classes" [COL,126,128]. It is in fact, taking under consideration various manuscripts written between MAL and LT, only in MAL that Boole distinguished between the elective symbol  $x$  and the class  $X$  so strictly.

(5) This was first pointed out by Bryant. She wrote "The symbolism of the calculus of Operations is, indeed, the natural mean term, in the development of mathematical invention, between common mathematics and that general formal science which Boole called Mathematics in the wider sense of the word [see 7.1 and 8.2]. We may-perhaps, we must-call it General Formal Logic; but General Mathematics would be better. The name does not, however, affect Boole's contention that mathematical form is a true type of logical form in general, and that mathematical language as already established and highly organized is the natural language of symbol generally " [Bryant 1888, 189; 1902, 106]. More recently, Laita and Grattan-Guinness hold that Boole viewed logic and mathematics as particular cases of a "Universal calculus of symbols". Their papers draw from few indicative passages from MAL, Boole's early mathematical papers (mainly his [1844]) and from his religious influences. See [Laita 1977, 174; Grattan-Guinness 1982, 34-38].

(6) I have borrowed these two terms from [Van Evra 1977, 364-365].

(7) In 1848 Boole was to point out certain fallacies into which Bronwin had fallen into in his early attempts in 1847 to apply symbolic methods for the solution of differential equations [see chapter 5]. Having commented upon Bronwin's use of false analogies and erroneous principles, Boole took the opportunity to raise attention to the lack of proper study of the "laws of correct reasoning in connexion with the practical discipline of modern science". He also wrote "Dismissing, however, the more immediate subject of these comments, I cannot but observe upon the fact as somewhat remarkable, that in pure mathematics, controversy and misunderstanding should be so rife, as for some years they appear to have been. It would, I conceive, be interesting to inquire into the causes of a state of things which, judging a priori, we should so little expect. One reason is undoubtedly to be found in the unmeasured capabilities of the modern analysis for the expression of general theorems, and in the practically frequent employment of analogy and induction, especially as suggestive aids, in contradiction to the purely deductive processes and more limited conclusions of the ancient geometry" [1848a,418; 5.2,(16)].

(8) Fallacies in Boole's approach in logic are mainly discussed in [Corcoran 1980 and 1986]. Restoration of Boole's shaky method in logic is attempted in [Hailperin 1976, 1981, 1984; Hooley 1966; Styazhkin 1969, 191-197] on formal ground in the modern sense. The most complete treatment is that in Hailperin's papers [1981, 1984] and book [1976]. More specific reference from these works will be made in due time in the course of our study.

(9) This analogy was for the first time pointed out by Bryant [see (4) and (5) above]. Since then this theme was put forward as a basis for a further study of Boole's work in logic in [Koppelman 1971, 235-7; Laita 1977, 171; 1982,7; 1985, 245; Grattan-Guinness 1982, 34-35. See also Jourdain 1910, 332, 340]. These papers mainly hint at the analogy between the three laws of combination of elective symbols and the respective laws of the differential calculus. Based on the same principles of research we will provide here a further study of Boole's work with illustrations covering most of the mathematical peculiarities of his calculus of logic.

(10) Boole hinted at the analogy between the solution of elective and differential equations in [MAL,70,72]. However, no attempt has been made to my knowledge by historians so far to discuss this analogy and illustrate it with examples. This analogy concerns mainly the first method for the solution of elective equations by substitution [see 7.5,8.8]. The second, more formal, method by development, is discussed in the recent researches cited in (8) above.

### Section 7.3

(1) It was only in [LT,28] that Boole restricted the definition of the

universe to "all beings" [see also Dummett 1959 , 205]. In MAL  $X$  stands either for an individual of a class or for the class itself, as it is seen in text below. From 1848 onwards  $x$  and  $X$  are identified [COL,126] but the concepts of election and elective symbol still keep part of their power. However, these concepts gradually loose their primary emphasis and finally in LT Boole's vocabulary is mainly restricted to the concept of class [see 8.3].

(2) Boole's use of parenthesis is poor and varies in different works. For example in [COL, 126] instead of  $x(1)$  he writes  $x1$ . In MAL 1 plays also the role of a numerical constant. In other words  $1x=x$ . In fact it is easily proved that 1 can be assumed as the unit operator in his logical calculus. It is also pointed out that Cayley first conceived 1 as an identity operator in 1854 [Hailperin 1984, 42, fn2; see also Venn 1881, 58].

(3) It is somehow surprising to notice an absence of any illustration with examples from ordinary language for laws (73.2), (73.5) and (73.9). In [COL,127] he gives only an example for (73.5). About the sign  $+$  he writes that "it represents the aggregation of two classes into a single class" [COL,127] but neither in MAL nor in COL is he explicit about the fact that he allows addition only between disjoint classes. Thus, a formal definition of addition is missing from MAL and only gradually, as we shall see, he comes to define the commutative law of this operation. For laws of both addition and subtraction which are implicitly assumed in MAL see [Hailperin 1984, 41-43; Corcoran 1986, 69, fn 6]. As it will be evident from what will follow in text, addition and subtraction in MAL resemble to union of classes and to relative complement of classes respectively [Corcoran 1980; see also 8.4].

(4) The index law,  $AA=A$ , was first discovered by Leibniz but Boole's discovery was independent. [Venn 1881, xxxi, fn 1; Jourdain 1910, 329-331]. Boole mentioned Leibniz only once in [LT,240]. About his acquaintance with Leibniz's respective law his wife recalls: "Some one wrote to my husband to say that in reading an old treatise by Leibniz (who lived at the same time as Newton) he had come upon the same formula which the Cambridge people call "Boole's Equation". My husband looked up Leibniz and found his equation there, and was perfectly delighted; he felt as if Leibniz had come and shaken hands with him across the centuries" [Laita 1980,58,fn 125]. On the law (73.9) as imposing a restriction on Boole's calculus see also [Venn 1881,56 fn 1,57].

(5) "The office of the elective symbol  $x$ , is to select individuals comprehended in the class  $X$ . Let the class  $X$  be supposed to embrace the universe; then whatever the class  $Y$  may be, we have  $xy=y$ . The office which  $X$  performs is now equivalent to the symbol  $+$  in one at least of its interpretations, and index law [(73.10)] gives  $+^n=+$ , which is the known property of that symbol" [MAL 17, fn; see also comments in Clock 1964, 124].

(6) Analogy between Boole's laws of logic and those of analysis as in his [1844] has been so far pointed out in [Laita 1977, 173-4; Grattan-Guinness 1982, 35; see also 7.2, (5), (9)].

(7) Boole discriminated between successive operations, as in  $xy$ , and aggregation, as in  $x+y$ . When in [MAL, 5-6] he writes "It is, for example, true that the result of two successive acts is unaffected by the order in which they are performed", he simply has in mind the commutative law of combination, as in  $xy$ . According to Corcoran [1980, 617] this statement expresses verbally the commutative law of addition, but from what we have seen so far I doubt that his claim is right.

(8) Implicitly Gregory, and explicitly Hamilton, Graves and Cayley, had all made use of the associativity law before 1847. It is puzzling that though Boole was acquainted with the work of these mathematicians he omitted to state this law; most probably he implied it. See [MacHale 1985, 238; Hailperin 1984, 43, fn 4]. De Morgan's laws are also noticeably absent [Corcoran 1980, 617, 636, fn 10].

#### Sections 7.4

(1) The law of transposition is missing together with other rules according to which Boole implicitly carries out his deductions. In Corcoran's critical study [1980] the missing laws are provided. In LT Boole is more careful in stating rules such as that of transposition [see LT, 35-36 and 8.4].

(2) Important comments made by R.L. Ellis about theorem (74.6) that featured in LT are included in [8.4 text and (8)].

(3) See [(86.15) and 8.6, (7)].

(4) In the text [MAL, 24] there is an error; he says: "Multiply by  $x$ , we have  $vy=vx$ " instead of multiply "by  $v$ ". In [LT, 61] he writes: "It is obvious that  $v$  is a symbol of the same kind as  $x, y$ , etc, and that it is subject to the general law,  $v^2=v$ , or  $v(1-v)=0$ ". When he claims that MAL was written hastily he must have meant the fact that he had omitted certain clarifications, which had he <sup>had</sup> included, his theory would have been more lucid <sup>and</sup> <sup>admitt</sup> fewer reactions [see Cayley's objections in 7.5, 8.2].

(5) In recent papers the controversial role of  $v$  in Boole's system has been an object of discussion. It is in fact regarded as a drawback in his system. Van Evra claims that it is impossible to conceive of a class  $v$ , whose only defining characteristic is that it have members [1977, 370]. Certain logicians regard it as redundant [Dummett 1959, 205; Corcoran 1980, 614]. Others pay attention to the fact that  $v$  is to be regarded as an operator [Laita 1980, 7-8; 1985, 245; Hailperin 1976, 78]. The latter modified Boole's system in modern terms establishing a sound foundation for  $v$ . For example  $vx=vy$  is written in

the form:  $(\exists v)(vy=vx \text{ and } vx \neq 0)$  [Hailperin 1976, 97-98].

## Section 7.5

(1) As we have mentioned before, Boole omitted in MAL a detailed discussion of all his processes. In [LT, 70-71], the "division" or "dichotomization" of a class  $y$  in respect to a quality  $x$  to two disjoint parts, as in (75.2), is given prior to theorem (75.1) but this discussion is absent in MAL [see 8.4]. At this point, interested in his methodology of solving elective equations, and, in particular, in illustrating the analogies he perceived between elective and differential equations, we take (75.2) for granted and proceed to his own observations and examples. However, I would like to refer to Venn's book, Symbolic Logic [1881] in which many of Boole's obscure points are explained in detail in a way similar to Boole's points of view. On (75.2) and (75.1) see [Venn 1881, 192-200].

(2) When Boole refers to "the very nature of elective symbols" he is a bit vague. Various illustrations are provided for an explanation of their function as in his letter to Cayley or the notes [N<sub>7</sub>-N<sub>27</sub>]. However, what he must have meant at this point is the property  $x^2=x$  which renders the equations linear. On this point see also [Venn 1881, 56]. From what follows in the text elective symbols are clearly viewed at certain instances as operators.

(3) In the collection of Boole's manuscripts there exist seven letters exchanged between Cayley and Boole late in 1847. These letters are transcribed in a note-book with classification mark E<sub>6</sub>-E<sub>13</sub>. The code number attached to the letters is E<sub>13</sub>. The first letter of this series belongs to Cayley and is dated Dec. 2 1847. On Cayley see also 5.10.

(4) Boole's answer dates Dec. 6 1847 and is the second of the letters exchanged. Prior to the second quotation cited above he wrote "when you ask what is the interpretation of  $x/2$  you forget that in my system  $x$  is not a quantity, but represents a mental operation. [...the above quotation]. The equation  $x/2=0$  is the same as  $x=0$  [...] and indicates the proposition "There are no Xs" ". Further to this matter we will refer in 8.2.

(5) This sketch is drawn from the C<sub>66</sub>-5 Logic notebook that bears the following inscription on the first page: "This appears to be an early exercise book on An.Logic V some parts of the author's Math<sup>1</sup>. speculations. a Relic - but not available" This sketch throws light on how Boole first conceived his general method. By  $*$  we denote multiplication. Notice also that parenthesis is missing from  $a+b+c+d$ . Parenthesis, as in the case of  $v'-v$ , is often denoted by mathematicians of that period by a bar "——". My insertions are in square brackets.

## Section 7.6



(1) Corcoran deduced (76.5) formally by means of Boole's laws mentioned in 7.3 enriched with certain laws only implicitly assumed by him. Corcoran regarded scheme (76.4) as weak on the following grounds. From  $a'y+b'=0$  we deduce  $b'=0$  (for, multiply with  $y-1$ . Then  $a'y(y-1)+b'(y-1)=0$ , hence  $b'(y-1)=0$  independently of  $y$ , hence  $b'=0$ ). Thus  $a'y=0$  and for the same reason  $ab'-a'b=0$ . Thus, elimination is effective only by the second premise [1980, 625-6].

(2) Next he went on to comment upon the essential structure of a syllogism and the limitations imposed upon the nature and the order of the terms of a conclusion [MAL, 34]. Boole intended to enlarge Aristotelian logic<sup>65</sup>, suggesting ways, as De Morgan did, to treat problems that are beyond the scope of traditional logic. For a comparison between Boole's treatment of syllogism with Aristotelian syllogism see [Corcoran 1980; Laita 1978, 61-2].

(3) In the first case,  $t=x$ , the conclusion is (a)  $vv'x=vv'z$ . In the list (74.18) form (6)' is accompanied by the auxilliary equations  $v(1-x)=0$ ,  $v'(1-z)=0$ . Thus (b)  $v'=v'z$ ,  $v=vx$  and substituting the values of  $v'z$  and  $vx$  from (b) in (a) we have  $vv'=vv' \longrightarrow 0=0$ . In the second case,  $t=x-1$ , we have (c)  $vv'(1-x)=vv'z$ . The auxilliaries are now  $v'x=0$ ,  $v'(1-z)=0$ . Thus  $v'z=v'$  and  $v'x=0$ , therefore (c) is written as  $vv'-vv'x=vv'z \longrightarrow vv'-v=vv' \longrightarrow v=0$  and hence (c) becomes  $0=0$ .

(4) "The mathematical condition in question, therefore, -the irreducibility of the final question to the form  $0=0$ , -adequately represents the logical condition of there being no middle term, or common medium of comparison, in the given premises". Boole thought that this had passed unnoticed by logicians so far, and attributed his discovery to the "peculiarity of its  $[0=0]$  mathematical expression" [MAL, 41]. However, in the postscript of MAL Boole acknowledged De Morgan's earlier work on this subject [MAL,82; see also Laita 1980, 62].

(5) "Perhaps the system we have actually employed is better, as distinguishing the cases in which  $v$  only may be employed, from those in which it must. But for the demonstration of certain general properties of the syllogism, the above system [(76.10)] is, from its simplicity, and from the mutual analogy of its forms, very convenient" [MAL, 44-5]. Boole also wrote in a footnote, "Professor Graves suggests the employment of the equation  $x=vy$  for the primary expression of the Proposition All Xs are Ys, and remarks that on multiplying both members by  $1-y$ , we obtain  $x(1-y)=0$ , the equation from which we set out in the text, and of which the previous one is solution" [MAL, 45 fn].

(6) "This elegant theorem was communicated by the Rev. Charles Graves, Fellow and Professor of Mathematics in Trinity College, Dublin, to whom the Author desires further to record his grateful acknowledgements for a very judicious examination of the former portion of this work, and for some new applications of the method" [MAL,45 fn]. We omit Boole's proof of (76.11) as it is beyond

the scope of our study. For comments upon Graves's suggestions to him see [Jourdain 1910, 337 fn; Kneale 1948, 152; MacHale 1985, 70; Smith 1983, 31-32].

(7) Among these writers is Latham [7.2.(2)]. Boole wrote in the footnote: "The Proposition, Every animal is either rational or irrational, cannot be resolved into, Either every animal is rational, or every animal is irrational. The former belongs to pure categoricals, the latter to hypotheticals. This animal is either rational or irrational, is equivalent to, Either this animal is rational, or it is irrational. This peculiarity of singular Propositions would almost justify our ranking them, though truly universals, in a separate class, as Ramus and his followers did" [MAL, 59 fn]. On other critical remarks, drawn in his system of logic by analogy with the rules of ordinary language we will comment in 8.2.

## Section 7.7

- (1) In fact, Boole had suggested in the course of chapters 3-5, verification of the solution only by substitution. See [(74.16), (76.2)].
- (2) This is the case with Boole's examples in MAL. However, in LT, other fractional coefficients appear but again, by means of theorem (77.29), the terms with which they are multiplied are equated to zero. See text below and 8.4-8.6.
- (3) For this reason we postpone any comments upon Boole's conception of division in his system until 8.5.
- (4) See particularly 8.2, 8.5 and 8.7.
- (5) For comments upon the proof of (77.9) see [Grattan-Guinness 1982, 34-37; Styazhkin 1969, 181 fn]. Rigorous demonstration of this theorem is provided by [Hailperin 1976, 95; Hooley 1966, 114].
- (6) Formulae (77.11) are particular cases of the development of the binomial function  $f(n;p)$ , where  $n, p$  operate upon a subject  $u$  combining according to the law  $pf(n)u = f(p(n))pu$  [1845d, 214; see also 4.7].
- (7) This is indeed true, for, whatever the objection can be about Boole's non-rigorous, for our standards, procedures, it is evident that he did care about consistency and this fact challenged historians to discover the "secret" of his approach [see (77.28) below and references in 8.5 text and (3)].
- (8) In [LT, 78-9], (77.16)-(77.17) are presented in the form of a theorem. However, once more demonstration is based upon observation and no reproduction of it will be included in 8.4-8.5.
- (9) Styazhkin argues that it is erroneous to conclude  $t=0$  from  $at=0$  and  $a \neq 0$ . "It is clear, however, that such a deduction holds only in systems where there are no divisors of zero, that is, where the equation  $xy=0$  legitimately does imply that at least one of the multipliers must be equal to zero. It is also

clear that in systems with divisors of zero, Boole's proof is simply untrue; in fact, the equation  $xy=0$  can occur in the algebra of logic, with  $x \neq 0$  and  $y \neq 0$  simultaneously" [1969, 186]. Respective comments about his procedure as in the proof of (77.22) were provided also in 7.5. Hailperin, on lines similar to those of Styazhkin's reasoning, adds property (77.24) in the fundamental laws of Boole's system [1976, 70-71; 1981, 178].

(10) He then went on: "If among the infinite number of different values which we are thus permitted to give to the moduli which do not vanish in a proposed equation, any one value should be preferred, it is unity, for when the moduli of a function are all either 0 or 1, the function itself satisfies the condition ((77.14))" [MAL, 65-66].

(11) On theorem (77.29) see also (5) above and [8.5, (3)]. The two theorems (77.20), (77.29) will not be reproduced in chapter 8. At this point I would like to mention a careless and incomplete reproduction of Boole's main results of his formal method by Feys in his [1955]. Particularly, at page 109 the writer calls  $a, a', \dots$  "coefficients" and  $t, t', \dots$  "the corresponding moduli" in the expansion (77.15).

(12) As an example of limitation take  $y=vx$ . Multiplication by  $v$  will give  $vy=vx$  which does not express as much as the first. For,  $y=vx$  stands for  $A$  and  $vy=vx$  for  $I$ . See list (74.18) and (74.10), (74.11).

(13) Accordingly "all hypothetical Propositions may be resolved into denials of the coexistence of the truth or falsity of certain assertions" [MAL, 77]. Both quotations are given in italics. In [LT, 84-85] Boole calls the forms  $t_1=0$  in which conclusions are exhibited as "Single or Conjoint Denial".

(14) The respective discussion in [LT, 84-85] is less emphatic than that in MAL and the claim that the theory of propositions rests upon "a positive and upon a negative foundation" is surprisingly omitted.

(15) Another analogy we can draw is between the theory of vectors and logic, in the sense that  $x$ , and  $1-x$  in  $\varphi(x)=\varphi(1)x+\varphi(0)(1-x)$  resemble unit vectors since  $x(1-x)=0$  and  $x+(1-x)=1$ . Closely related to the theory of vector spaces is also formula (77.30).

## Chapter 8

### Section 8.1

(1) First in [COL, 140] and consequently in the paper cited above, Boole distinguished Kant's foundation of logic, as falling within the domain of metaphysics, from his own exposition in which he tries to show that the laws of thought are mathematical and "to establish upon them a perfect and formal

## Section 8.2

(1) This is quoted from pages N<sub>10</sub>-N<sub>12</sub> of Boole's notes N<sub>7</sub>-N<sub>27</sub> [7.2, (2)]. Occasionally a reference of a footnote will serve as a means of recalling the quotation given in text.

(2) R.G.Latham (1812-1888) was an ethnologist and philologist. He graduated from Cambridge in 1832. Boole drew his [1847] [see 7.2, (2)] and referred to him in both MAL and LT [7.1, (3)]. On an objection of Boole's towards Latham's conception of ordinary language see also [7.6, (7)]. The letter cited below in text belongs to the archives of the Royal Society and bears code number C<sub>7-e</sub>.

(3) In this letter Boole argued on similar lines, as in the case of "Men", about the plural of "I". "We", he wrote, is not "I+I+...". For, "the plurality of "We" has no reference to the personality of the ego as its singular root or idea - its reference is to some quality or circumstance or position in which the ego is placed or regarded".

(4) In the postscript of the letter he wrote: "In [If] the expression of the plural as a collection of singulars in the language of the calculus of Logic were required we should have to proceed first as in ordinary language in expressing these singular[s] to consider by what qualities they differ from each other while agreeing in that common quality to which the plural has reference. Thus if x represent the common quality and s,t,u,... the special qualities [...] respectively in conjunction with x to mark the separate individuals we should have  $x=xs+xt+xu...$  and the consequences deduced from this as a logical  $\Rightarrow$  by the calculus of Logic would be correct consequences  $\Rightarrow$  as [based] upon the data assumed".

(5) When Jevons first insisted in 1863 that (82.1) should be included in Boole's system as the equivalent of the index law  $x^2=x$  in addition, Boole strongly objected to it. If  $x+x=x$  is a fundamental law, this would above all mean that if x is taken for the universe then  $1+1=1$  or  $2=1$ , which is absurd. Or, if  $x+x=x$ , then  $x+x$  is interpretable in logic, hence the index law  $(x+x)^2=x+x$  holds true. But it follows easily that the above implies  $x+x=0$ . Thus

$$(1) \quad x+x=0 \longleftrightarrow x=0$$

and only in such equations  $x+x$  can appear, wrote Boole. The letters from which we drew our arguments are held in the Royal Society with code numbers C<sub>21</sub>, C<sub>22</sub>. Further on Jevons see chapter 9. Among contemporary logicians only Corcoran insisted on including (82.1) among the laws of Boole's system, but this suggestion is erroneous since, according to what we stated so far, this law

is inconsistent with the rest of Boole's calculus [Corcoran 1980, 617]. Notice that axiom (1) is a case of (77.23) [see also 7.7, (9)].

(6) On Boole's correspondence with Cayley see [7.5, (4)]. By  $\phi(xyz)$  Boole denoted  $\phi(x,y,z)$ .

(7) We first referred to Cayley's objections in [7.5, (4)]. Boole's first argument on the analogy between  $\frac{1}{2}x + \frac{1}{2}x = x$  and  $\sqrt{-1}\sqrt{-1}=-1$  appeared in a letter dated 6 December 1847. Cayley's response above in text dates 7 December 1847. In this letter Cayley raised various objections towards the "English theory" of the geometrical interpretation of  $\sqrt{-1}$ . He added that he would "much more easily admit witchcraft on the philosopher's stone" than believe in this theory. An abundance of similar objections are to be found in his letters which followed.

(8) In fact Cayley had started corresponding with Boole on invariants as early as 1844 [7.1; MacHale 1985, 56-8]. The characterization of Boole's correspondences in text is borrowed from Corcoran's review of the De Morgan - Boole correspondence [1986; 7.1, (8)]. The characterization may sound general and absolute but in large it may be applied to most of Boole's correspondences.

(9) What Boole wrote exactly has as follows: "Do you think it necessary in order to our employing the symbol  $\sqrt{-1}$  in analysis that we should be able to interpret it [?]. Is it not in the science of quantity (apart from direction) sufficient to consider it as a symbol (i) which satisfies particular laws and especially this,  $i^2=-1$ , and which disappears from the final result whenever a solution is real in virtue of the principle that if  $a+bi=0$  and  $a$  and  $b$  are real then  $a=0$   $b=0$  and is not this so far as it goes analogous with that I have proved in Prop. 2 [(77.20)] [?]. I contend for analogy in the nature of the things themselves and I only give this as an illustration which does not at all affect the truth of my system".

(10) Venn did include Boole's argument on imaginaries in his book [1881, 201]. However, quoting from Boole's LT in a footnote - "The employment of the uninterpretable symbol  $\sqrt{-1}$  in the intermediate processes of Trigonometry furnishes an illustration of what has been said"- he wrote: "I need hardly say that I do not here accept the uninterpretability of the symbol  $\sqrt{-1}$ " [1881, 73]. By "what has been said" Boole referred to the conditions of general symbolic reasoning quoted in 7.2 [see also 7.2, (7); LT, 69 and Hailperin 1976, 68].

(11) ["Sketch", 165-166]. Once more it follows from the last paragraph that had  $x+x=x$  had been included as a law, then it would have had to be satisfied by both 1 and 0. In the former case the contradiction  $2=1$  would follow. See also (5) above.

(12) It is interesting to notice the way Boole discriminates between "the mathematics of quantity" and "the mathematics of logic". The concept

"mathematics" is taken to mean general reasoning by symbols other than words. See also [7.2, (5)].  
 (13) [1851, 208-209].

### Section 8.3

(1) Boole wrote  $x^2=x$  also in the form  $x(1-x)(-1-x)=0$ . He wrote that  $1+x$  "is not interpretable, because we cannot conceive of the addition of any class  $x$  to the universe 1". Moreover,  $-1-x$  is not interpretable because it does not obey (83.2). See [LT, 50 fn; Hailperin 1976, 64-65 and 8.4 text and (5)].

(2) At page N<sub>13</sub> Boole wrote "A good illustration of the real nature of an elective symbol is afforded by the discontinuous integrals which are employed as multipliers, in the calculation of the values of definite multiple integrals according to the method of M. Dirichlet. For these multipliers are equal to unity for certain points in space and to 0 for all others. The product of two such multipliers would therefore represent all the points in space which are common to the representation of each".

(3) The first to notice the fading of the operational character of Boole's logic in LT in comparison with MAL, was S. Bryant [7.2, (4)].

(4) In [COL, 128] Boole wrote: "The expression All Ys represents the class Y and will therefore be expressed by  $y$ , the copula are by the sign  $=$ , the indefinite term, Xs, is equivalent to Some Xs. It is a convention of language, the word some is expressed in the subject but not in the predicate of a proposition. The term Some Xs will be expressed by  $vx$ , in which  $v$  is an elective symbol appropriate to a class V, some members of which are Xs, but which is in other respects arbitrary. Thus the proposition A will be expressed by the equation  $y=vx$ " [whereas in MAL it was expressed by  $x(1-y)=0$  which was justified in a more intuitive way; see (74.3)]. Boole seems to be influenced here both by C. Graves's suggestion and by the quantification of the predicate [see 7.5 and (5) below]. The same approach was taken in [LT, 61] where Boole stated the above in the form of a rule.

(5) Boole's former formulation of A as  $x(1-y)=0$  is less complicated. But the main objection concerns the way  $v$  is introduced. See [Jourdain 1910, 338 fn; Lewis 1918, 56-57 and further discussion on  $v$  in 8.4-8.5].

(6) When (77.40), or  $x(1-y)=0$ , is solved and  $y$  is given by the formula (77.41),  $y$  is in fact the result of the operation  $x/x$ .

(7) [Page N<sub>14</sub>]. At the next page a certain change is observed. Under the title "Introduction" there is a list of subjects such as: "signs", "division", "development", "elimination". These notes are a very rough sketch of something to be written probably in the future. They end with few remarks on the equation  $x(1-x)=0$  very similar to those in [LT, 49-50; see also (74.6)].

(8) This letter belongs to the Royal Society archives and bears the code number B<sub>363</sub>. It appears in two forms. One is dated and in manuscript form, the other is an undated extract of the original letter. It appears that this extract was most probably included in a volume of Boole's manuscripts which belonged to Harley. This conclusion is derived from a commentary of Jourdain on this volume which contained letters to and from Gregory, Boole, Jevons, Lubbock and others [Jourdain 1910, 332, fn. 335-336].

(9) Viewing Boole's system as an algebra of operators, Hailperin formalized Boole's propositional logic in MAL proving that his system of hypotheticals is isomorphic to what we nowadays call Boolean algebra <sup>more exactly,</sup> Boolean ring [Hailperin 1984].

#### Section 8.4

(1) See Boole's letter to Cayley of December 1847 copied in [8.2 text, (6)].

(2) He argued that "whether we regard signs as the representatives of things and of their relations, or as the representatives of the conceptions and operations of the human intellect, in studying the laws of signs, we are in effect studying the manifested laws of reasoning". He concluded his arguments by saying that we could not easily conceive that "the unnumbered tongues and dialects of the earth should have preserved through a long succession of ages so much that is common and universal, were we not assured of the existence of some deep foundation of their agreement in the laws of the mind itself" [LT, 24-25].

(3) I wrote above "in a formal way" for, Boole presented this list (in italics) in the form of a proposition which lays down the symbolic language of his calculus [LT, 25]. This list strongly resembles De Morgan's respective definition of the language of algebra [De Morgan 1849c, 101-5].

(4) In order to justify cases such as  $yx$ , when  $y$  denotes "sheep" and  $x$  "white things", Boole drew from poetry: "The rising world of waters dark and deep" [LT, 30]. This tendency to draw directly from the structure of ordinary language is absent in MAL.

(5) Formulae (84.4) and (84.5) were also missing in MAL. For further comments see [Corcoran 1986, 69, 71; Lewis 1918, 52-3, 56]. Corcoran draws a comparison between Boole's own version of the union of classes and De Morgan's reproduction of it in his article on logic [1860c].

(6) As it has been stressed more recently, Boole's algebra is not Boolean algebra [Kneale 1948, 166; 1962, 413; Hailperin 1976, 62; 1982]. A.J.Ellis's interesting brief paper has not been cited by historians so far. I discovered it in the bibliography of [Venn 1881].

(7) In [Thomas 1955, 92] there are selected various instances from Boole's

writings, including the above quotation, on the notion of limit in his system. Formula (77.34) is also recalled in this context. The notion of limit was also included in Gratry's Logique but it was studied under its mathematical context missing in Boole [see 8.9].

(8) Harley, based upon Ellis's remarks, dealt in detail with how  $x-x^2=0$  is derived from  $x^2=x$ . "Further", he wrote, "in the final interpretation not only is the principle of contradiction (or non-contradiction) employed, as Leslie Ellis points out in the latter part of his "Observations", but the principle of excluded middle is also employed. For in interpreting  $1-x$  to mean not- $x$ , it is tacitly assumed that every one of the things of which the universe, represented by unity, is made up, is either  $x$  or not  $x$ " [Harley 1870, 14]. In an unknown paper on LT, [Young 1865, 166-167], we see an independent argument against the deduction of (84.10) as a "theorem". The author refutes Boole's philosophical arguments calling attention to Boole's "definite mathematical processes" as the principle merit of LT [ibid, 182].

(9) Boole's last remark in the quotation above seems prophetic in concern with the development of three valued logic [Haiperin 1976, 64-65; Grattan-Guinness 1989, 8]. The latter mentions Peirce's extension of Boole's law (84.10) in the form  $(x-f)(x-v)(x-z)=0$  where " $v$ " and " $f$ " are true and false respectively, and " $z$ " is a new available case.

(10) References on the rigorous demonstration of theorem (84.13) on modern grounds are given in [7.7, (5)].

## Section 8.5

(1) Boole did not bother to mention this assumption. Moreover, he did not distinguish between interpretation in extension and in comprehension as Hamilton and De Morgan had done [see however, 8.7, (5)]. As J.J. Murphy pointed out in his [1884, 35-36], addition and subtraction <sup>are</sup> interpreted in Boole's logic in extension whereas combination and division <sub>in</sub> comprehension. Hence, quality  $y$  is part of the attribute  $x$ . In books of modern set theory equation  $x=y \cap z$ ,  $x, y, z$  sets, is solved under the assumption that  $x \subseteq y$  and the result is that  $z=x \cup c$ ,  $c$  arbitrary set such that  $c \cap y = \emptyset$ . Compare this result with (85.11)-(85.12) and (85.13) below in text.

(2) This essay is marked E2 in the Royal Society manuscripts. As Rhees has pointed out, Boole revised in this essay his treatment of certain questions yielded in LT [Rhees 1952, 211 fn].

(3) Among the earliest modern approaches towards a verification of Boole's method is [Hooley 1966]. Based upon elementary Boolean algebra, Hooley reformulated and proved theorem (77.29) and then showed that every equation  $\phi(x, a, b)$  has a solution [1966, 116-118]. Independently, Styazhkin proved for-



mula (85.13), working in a spirit closer to that of Boole's than to modern Boolean algebra. Based upon (85.13), he defined  $1/0$  as  $1+v(1-0)=1+v$  and called it the complement of  $v$ . He also examined whether one can establish rules for operating with logical "fractions",  $x/y$ , such that a maximal analogy is obtained with respect to arithmetic rules for operation with fractions [1969, 190-202]. Finally, Hailperin used the notion of "heap" or "signed multiset" in order to keep Boole's fundamental laws intact and regard addition, such as  $x+x$ , as valid. "A multiset is like an ordinary set except that multiple occurrences of elements are allowed, and by a signed multiset we mean one in which negative multiplicities are allowed. When the multiplicities are restricted to 0 or 1, then multisets become ordinary sets" [1981, 178].

(4) Not all commentators on Boole's division as in his LT had difficulties in accepting the interpretation provided by him for the formula (85.10). In [Young 1865, 177-8] we read straight after the introduction of (85.10): "Here we have two symbols,  $0/0$  and  $1/0$ , the meaning of which has not yet been determined. Our author shows that the former, which in Algebra denotes an indefinite numerical quantity, denotes in the logical system an indefinite class. In Algebra  $1/0$  denotes infinity; and, as is well known, when it occurs as the co-efficient in a term in an equation all of whose other terms are finite, this indicates that the quantity of which it is the co-efficient is zero. So, in the logical system if, in any term of an equation obtained in the manner in which equation [(85.10)] has been obtained, the co-efficient be  $1/0$ , the corresponding constituent must be 0. These are certainly very remarkable analogies".

## Section 8.6

(1) Notice that (86.1) is a necessary and sufficient condition for  $f(x)=0$  to have a solution. Take for instance  $f(z)=0$ , where  $f(z)=yz-x$ . Then  $f(1)=y-x$ ,  $f(0)=x$  and thus we have  $(y-x)x=0$  equivalent to  $x(1-y)=0$ . The latter coincides with condition (85.12) in the solution  $z=y/x$ . Boole omitted to notice that as well as to mention that  $f(0) \neq f(1)$ . The latter is rather implicitly assumed, as we will see below in text when he comments upon the interpretation of the classes  $f(0)$ ,  $f(1)$ . For discussion on  $f(0)$ ,  $f(1)$  see [Venn 1881, chapter 14]. This method for elimination was first introduced by Boole in "Sketch" around 1848-9 [see 8.1].

(2) This method was applied in few cases only and so we omitted details in text. In brief Boole showed that the system  $V_1=0$ ,  $V_2=0$  is equivalent to  $V_1+cV_2=0$ ,  $c$  constant. In fact, he proved that if  $At$  is any term in the development of  $V_1$  and  $Bt$  the corresponding term in  $V_2$  then it follows that  $A+cB=0 \longleftrightarrow A=B=0$  [LT, 116-117; see also 7.7 and MAL, 78-81].

- (3) It is pointed out that Boole's argument based on squaring may fail if for  $A \neq 0$  it happens that  $A^2 = 0$ . Hence formal justification requires the principle  $A^2 = 0 \rightarrow A = 0$  [Hailperin 1976, 76; see also Lewis 1918, 64].
- (4) Styazhkin holds that the algebraic deduction of  $X \cdot Y = 0$  from  $X \cdot Y$  is logically unjustified [1969, 183].
- (5) Smith points out that the technique of writing  $V$  in the form (86.13) is exactly what we use in set theory when we wish to express the union of a denumerable collection of sets as a union of pairwise disjoint sets [Smith 1982a, 58-9].
- (6) The only work on Boole so far that deals in depth with his applications on philosophical arguments is [Diagne 1989, 158-169]. This is the most recent book on Boole so far, in large <sup>part</sup> based upon Mac Hale [1985]. Though Diagne goes deeper than Mac Hale in certain aspects of logic, the book is far from satisfactory. The author ignored many recent papers on Boole's work, and the order in which the material is arranged is very confusing.
- (7) Venn defended Boole in connection with the quantification of the predicate, saying that he was always to provide alternative forms for  $A$  and  $O$  devoid of  $v$ . He went on: "There is no attempt to distinguish whether we mean "some only" or "all" the predicate to be taken; and this I have always considered to be the whole point of the Quantification doctrine. Moreover his whole treatment of these forms is antagonistic to this doctrine... If any one will point out to me a passage in which Boole has admitted the distinctive propositions "All  $X$  is some  $Y$ ", "Some  $X$  is not some  $Y$ ", I shall admit, not that his system is founded on the Quantification of predicates, but that he has there used expressions inconsistent with his system of symbols" [Venn 1881, 324-5, fn ].
- (8) In fact there exist two copies. One is included in the Royal Society manuscript collection and is written <sup>by</sup> Boole himself, dated 1848. The other copy belonged to Dr. Hinton, a descendant of Boole, and seems to be a transcription taken principally from the former copy of MAL. However, the second copy is incomplete. For details see "Introduction" in [Smith 1982b].
- (9) Smith used mainly the Hinton copy for his edition, but as its appendix was incomplete he made use of the Royal Society copy for what follows in text.

## Section 8.7

- (1) In a letter to De Morgan, dated 21 March 1859, Boole wrote that the material written on the philosophy of logic could make two or three books and that he was indecisive as to what he would finally include in them. Moreover, he stressed that he was not going to set aside anything in the LT [Smith 1982a, 77]. On the reasons why his book remained incomplete see also [Neil 1865, 172; Rhees 1952, 11-12]. On De Morgan's advice against the publication

of Boole's manuscripts in 1867 see [Hesse 1952, 61-2; Rhees 1952, 11-12].

(2) Edition and discussion of extracts from Boole's manuscripts of logic is provided by [Hesse 1952; Rhees 1952].

(3) On the full title of the paper cited as "Extracts" in text, together with comments, see [Rhees 1952, 10; 230].

(4) Boole did not omit entirely such comments in his presentation in LT. For example see (84.1). However, he makes no reference explicitly as to the formal conditions of the possibility of symbolical expressions in the way he does in later manuscripts.

(5) It is only in manuscript notes that Boole defines division (abstraction) as the inverse operation of multiplication (composition). Three instances are noticed altogether. Beside this mentioned in text above, we have the notes  $N_{27}$  [(83.3)-(83.4)] and [LR, 221; 8.5].

(6) Once more we notice the absence of verification of the result of division, as in (87.9), which amounts to multiplication of (87.9) with  $z$ . In 8.5 we mentioned Venn's remedy to this omission.

(7) As a complement to our brief account see also [Hesse 1952, 73-81; Rhees 1952, 17-40].

## Section 8.8

(1) We will cite from the 4th edition of Boole's textbook dated 1877 (see 4.5(5)).

(2) Compare the above citation with Boole's statements in [MAL, 2; LT, 12]. See further comments on these statements and on Boole's educational concerns in [7.1, (15), (16), (18), (23); 7.2, (7)].

(3) We have dealt with this matter earlier in [7.2, 8.2, 8.7]. See particularly [(87.5)-(87.6); LT, 6,68].

(4) Monge's method for the solution of partial differential equations of the second order is included in chapter 15 of Boole's textbook. He noticed there that there are cases where Monge's method fails and mentioned also Laplace's own method. However all these methods were of limited generality. His commentary ends as follows: "But there are probably no instances in which this method has been applied in which the solution may not be effected with far greater elegance, and with far greater simplicity, by the symbolical methods of the following Chapters. And even Laplace's method is better exhibited in a symbolical form" [1877, 374-375; see also 205].

(5) See also comments in [(71.1); 7.4, (2), (4); (84.10); LT, 411-14].

(6) See [Laita 1980, 45; Mac Hale 1985, 222; Grattan-Guinness 1982, 39-40].

(7) See passages in [M.Boole 1972, 67-68; 1879, 49-52; 1931, 252-253]. See also comments in [Laita 1980]. On Mary Boole's work see the translator's in-

roduction in [Gratry 1944, 11-13].

## Section 8.9

(1) On M.Boole's claim see 8.8. Lack of reference in their work show that neither Boole nor Gratry were acquainted with each other's work. Most probably Boole read Gratry early in the 1860's. In text we will quote from the fifth edition of Gratry's Logic dated as [1944].

(2) Gratry's discussion of induction covers [1944, 35-266;351-465]. M.Boole was the first commentator to perceive the similarity of doctrines between the two men. See particularly her [1897, 44-55]. Further comments are traced in the translator's introduction [1944, 11-13] as well as in [Grattan-Guinness 1982, 41; Laita 1980, 58; Mac Hale 1985, 203-4; Thomas 1955, 91, 95-96]. The latter's article on Boole's concept of science is the only instance where contrasts are also noticed.

(3) The quotation in text is from [Mac Hale 1985, 203; see also 7.1, (4)]. On Gratry's optimism and Boole's scepticisism compare [1944, 96-7, 109, 594] with [LT, 424].

(4) Discussing the nature of syllogism as compared to that of induction, Gratry wrote:"the syllogism would be a task for minds in their infancy" [1944, 322]. For further comments see [1944, 36, 175].

(5) This quotation is from Boole's manuscript notes with code number C<sub>57</sub>. See [Rhees 1952, 16-6].

(6) Boole's and Gratry's common Platonistic views are pointed out in [Thomas 1955, 91, 96].

(7) Gratry's comments are as follows: "The theory of Analytic Functions [by Lagrange], containing the principles of the differential calculus (infinitesimal calculus) free from any consideration of the infinitely small or vanishing element, of limits or fluxions, are reduced to the algebraic analysis of finite quantities". More than one disciple of Condillac thrilled with joy on reading this title. "So we are", he thought, "free at last from this geometrical mysticism, from the infinitesimal mystery, from all that alleged need of the infinite" [1944, 424-5]. For further comments on Lagrange's work see [1944, 86, 425-7, 565].

(8) We have omitted Boole's account of the form-matter issue as illustrated in his work, regarding it as known [see 8.7-8.8]. In LT we notice an absence of examples from analysis. This fact was noticed by Thomas [see (6) above].

(9) The two examples of infinite series with singular properties given by Gratry are  $1-1/2+1/3-1/4+\dots$  and  $1/4+1/4^2+1/4^3+\dots$  [1944, 65, 65 fn 10]. Such singular cases were noticed by Dirichlet in 1825.

(10) The polygon-curve example was extensively studied by Carnot in his [1813]. On Carnot's discussion see interesting comments in [Gillispie 1971,

134-141]. It is quite probable that Gratry was aware of Carnot's work.

(11) [1944, 557]. For further comments on Gratry's "comparative study" see [1944, 100-1, 552-7].

(12) We remind the reader of De Morgan's unique application of syllogism to Pythagoras' theorem [see 6.5, (2)].

## Chapter 9

### Section 9.1

(1) Valéry [1977, 39-40]. See also [1.1, (1); 7.2, (2)].

(2) Many modern textbooks on differential equations apply operator methods for the solution of linear equations with constant coefficients on lines closely similar to Gregory's. Most of these books either provide no references as to the inventors of such symbolical methods [for example see Blakey 1949, 473-486; Sneddon 1957, 96-105] or draw on more recent researchers in this branch like C.A. Hutchinson in the 1930's [see Rainville 1974, 90, 148]. However, there are some rare exceptions such as [Ince 1927, 138; Rota 1975, 62] where Boole is merely acknowledged for what is now erroneously known as the "Heaviside expansion theorem" [on the latter see details in Cooper 1952; Petrova 1987; 1.7; 4.4-4.5]. Boole's method for the solution of equations with variable coefficients is absent from textbooks on differential equations in our century; in fact it is even omitted by his successor textbook writer Forsyth [4.6, (11); 9.4, (17)]. The case of finite difference equations nevertheless is less dissappointing [see 9.4, (18)].

(3) Our account follows a not too strict chronological order. According to the demands of the text, occasionally certain instances may be stressed and discussed in the context of an earlier or later period.

### Section 9.2

(1) The abbreviations COO, COF, EFE, SDM and FNL stand respectively for the calculi of operations and functions, the earth-figure equation and De Morgan [1831; 1839]. See also [9.1, (3)].

(2) Cayley was to stress the transform  $\varphi^{-1}f\varphi$  in his encyclopedia article on functions as a most basic form of the calculus of functions [1879, 823-4]. He added (like R. Ball [see 3.9 text above (1)]) that "The so-called Calculus of Functions, as considered chiefly by Herschel, Babbage and De Morgan, is not so much a theory of functions as a theory of the solution of functional equations". Recall Herschel [1860] on a formula by Brinkley [text below (23.24)] and note that Cayley [1860b] elaborated on it using symbolical

methods. Brinkley's  $\Delta^n O^m$  numbers and Herschel's symbolical treatment of the calculus of finite differences survived in our century under improved reformulations [see Milne-Thomson 1951, chapter 2].

(3) At least in Murphy's case the problem was purely financial. See [Smith 1984a, 1-7].

### Section 9.3

(1) According to Condillac algebra furnished the paradigmatic example of a well-made language [1.8, (10)]. We would like to stress the linguistic influences in the cases of Babbage [2.9], Boole [7.1-7.3; 8.2; 8.4; 8.6], Sylvester [5.10; 7.1, (2)], Kirwan and Whately [6.2] and Solly [6.3].

(2) On De Morgan's and Boole's educational concerns see respectively [3.4; 3.5; 6.5; 6.7, (4); 6.8, (4)] and [7.1, (15), (16), (23); 8.8].

(3) The abbreviations LE, COV, COL, MAL,  $S_1$  and FL stand respectively for the Laplace equation, the calculus of variations, Boole [1848b; 1847a] and De Morgan [1847a; 1847b]. On those of table 5 read further Boole [1854] for LT and De Morgan [1850; 1858; 1860b; 1860a; 1860c] for  $S_2$ ,  $S_3$ ,  $S$ ,  $S_4$  and "Logic" respectively.

(4) The quotation from Boole's notes  $N_7-N_{13}$  [see 7.2, (2)] is cited in text [7.3, below (6)]. On the Boole-Cayley correspondence see [7.3, (3)-(4); 8.2].

(5) Boole's choice of illustrative examples from symbolical algebra was not a successful one. His arguments in his manuscript notes, as well as his background in operator solutions of differential equations suggest that he was able to provide much better ones [see details in 8.2, (7)-(10)].

(6) On elective equations and on the theorem (93.11) in LT see details respectively [7.5; 8.3 below (5); 8.6] and [(84.13)-(84.15)].

(7) On De Morgan's comparison see 6.7. Boole delved into a deep epistemological study in manuscript notes between 1848 and 1855 which were not published [see (4) above; 8.2; 8.7].

(8) On De Morgan see [text below (69.11)] and on Boole [7.1, (24); 8.7 text and (1)-(2)].

(9) On De Morgan's lack of rigour and clarity see [6.4; 6.6, (3), (7), (15); 6.8; 6.9, (16), (17), (20)]. On Boole's fallacies and weakness of his system see [7.2, (8); 7.3, (2), (3), (8); 7.4, (1), (4), (5); 7.5, (1); 7.6, (1), (4); 7.7, (9); 8.2, (10); 8.2, (10); 8.4, (6), (8); 8.5, (1); 8.6, (3), (4); 8.7, (5)-(6)].

### Section 9.4

(1) [Spottiswoode 1865, 5]. By that time both Spottiswoode and Russell had

effected some progress on Boole's algebra of non-commutative symbols [5.9 text and (1)-(4), (8)].

(2) On Cayley's and Sylvester's work on invariants after Boole and on what came to be called as "new algebra" in the 1860's see [5.7, (12); 5.9, (1)-(2); 5.10, text and (8)-(11)]. More specifically on the definition of "invariant", "covariant", "form" and "quantic" see Cayley's detailed vocabulary in his English Cyclopaedia article [1860a]. Boole was in contact with Cayley since the 1840's and he did not approve of the term "quantic" which the latter applied to a rational and integral function of any order [see Boole's letter to De Morgan in January 1855 in Smith 1982, 68]. Further on Sylvester's "form" see also (4) below.

(3) We had several occasions to see the vital role played by the dual concepts "form" and "matter" in De Morgan's [3.9, (9), (16); 6.7, (8), (10); 6.9, (19)] and Boole's work [8.8 text above (4); 8.9, (8)].

(4) [Sylvester 1853, 544-47]. See also Cayley [1860a] and on Darwin's influence [5.9, (2)]. See also (5) below.

(5) On Boole's influence from grammar see [7.2, (2), (3); 7.6, (7); 8.2, (2)-(4); 8.4, (4)]. On Sylvester's linguistic and pertinent epistemological comments see [5.10, (11)-(12); 6.9, (10); 7.1, (2)]. These references reveal their common passion for poetry and beauty on rather independent lines. In fact only Sylvester appears to share De Morgan's and Boole's complementary studies on the nature of symbolic reasoning and the importance of a symbolic language and metalanguage afforded by the separation of "form" from "matter", or symbols of operation from those of quantity [see 5.10, (12)].

(6) [Spottiswoode 1878, 25]. Spottiswoode declared consequently that he would not dwell upon "such technical points", thus omitting any specific references.

(7) On Bronwin's and Cayley's stimuli and apparent consequences see respectively [5.2, (11)-(18); 5.4, (6); 5.10; 7.2, (7)] and [7.5, (3); 8.2]. Further on Boole's views on symbolical methods see [8.7-8.8].

(8) This is part of an interesting passage from Boole [1859] quoted in full in [8.8, (3)].

(9) [Airy 1866, 38]. Notice the part on the "little value" of operator methods in physical astronomy in connection with text below and (15)-(16).

(10) [Airy 1866, 32]. Airy's switch from series solutions -as favoured by Whewell- to finite ones is interesting [see 3.2].

(11) [Earnshaw 1871, vii]. Earnshaw's account is in comparison with Airy's far from elementary and devoted solely to symbolical methods. In this respect his statement is very bizarre given his knowledge of all the prevailing textbooks on such methods. On the treatment of the LE in the form (94.3), see Boole on (58.12) and Carmichael on (58.50). By a technique closely similar to that used

by Gregory and Bronwin [5.2,(6)], Earnshaw solved (94.3) by reducing it to the form (94.2) [1871,140-43]. We omit his final result as too complicated in form and of apparently no impact.

(12) Most papers by Ellis, Bronwin and other mid-19<sup>th</sup>-century analysts on the EFE and allied second order equations were published under this title [see 4.3;5.2 text above (1)].

(13) [Cayley 1869,77]. We recall that Ellis, Bronwin, Hargreave and Boole devoted a good deal of their work to series solutions [see 4.3;4.5;5.2-5.4].

(14) Boole devoted chapter 6 of his [1859] to the solution of the RE by the method of continued fractions. In chapter 7 on symbolical methods he confined in giving its symbolic solution as an exercise by reducing it, like the EFE [4.6] in binomial form [see 1877,451].

(15) [Pratt 1871,20,fn]. Pratt's book is the fourth edition of his [1860] cited in 1.3 and 3.2. It is, in fact, an improved version of his [1836] which followed on Poisson's lines [see 1.3,(4),(5),(7),(8),(12),(15);3.2, text below (32.24);5.8, text below (12)].

(16) [Todhunter 1875,154]. On Todhunter's treatment see [3.2;5.8 below (12)].

(17) [Forsyth 1830,ix].

(18) In the "Preface" to his book The calculus of finite differences [1951,v-vii] Milne-Thomson wrote: "The only comprehensive English treatise, namely Boole's Finite Differences, is long since out of print, and in most respects out of date. My first idea was to revise Boole's book, but on looking into the matter it appeared that such a course would be unsatisfactory, if not impracticable. I therefore decided to write a completely new work in which not only the useful material of Boole should find a place, but in which room should also be found for the more modern developments of the finite calculus [... ] Chapters XIV and XVI develop the solution of linear difference equations with variable coefficients by means of Boole's operators, which I have generalized in order to render the treatment more complete". On Boole's textbook on finite differences [1860] see also [4.5,(8);7.1, (25)].

## Section 9.5

(1) In the case of operator methods see our discussion in [5.10;93-9.4;9.8]. On the few exceptions to this rule see [9.4 text and (18)]. As far as logic is concerned recent historians have tried to reformulate upon rigorous standards Boole's method of solution of elective equations [Hailperin 1981; Hooley 1966; see 8.5,(3)]. Among his 19<sup>th</sup>-century followers apparently only Venn [1881] and Schröder made use of that method [on the latter see Styazhkin 1969,208-211; 9.6,(7)].

(2) We have compared MAL with LT in [7.3;8.3-8.6]. Boole had good reasons



for not approving of MAL. However on MAL's merits over LT see comments in [7.1, (20); 7.2, (4); 8.3, (9)].

(3) On Brodie's application of Boolean techniques to chemistry see 9.7. Solly's peculiar application of the differential calculus to logical concepts in his [1839] is omitted here [see 6.3, (7)–(8)]. The proximity between Solly's concerns and Gratry's and Boole's work on singular solutions [8.8–8.9] is an interesting topic for further research.

(4) [Harley 1870, 15].

(5) [Harley 1866b, 5–6].

(6) [Young 1865, 182]. We recall that while almost two thirds of MAL was devoted to syllogistic logic, only one chapter of LT tackled syllogism on the grounds that traditional logic was insufficient and badly presented [8.4; 8.6]. A biography of Young is missing, but as he was acquainted with Boole's friend Kelland [5.10, (1); 7.1, (11)] it is not surprising that he was interested in LT [see also 8.4, (6)].

(7) [Nidditch 1962, 48]. See also [Lewis 1918, 72–78; Styazhkin 1969, 203–207] and references in (8) below.

(8) The citations that follow in text are from Jevons's criticism as cited in [Harley 1866a, 42–3]. Further on Jevons's four objections see [Jourdain 1913, 120–1; Hailperin 1976, 82; Grattan-Guinness 1991, 17–19; MacHale 1985, 236–238].

(9) [Venn 1881, 355].

(10) [Venn 1881, 206].

(11) For example he pointed out a test of correctness for the result of the division  $x/y$  [1881, 70–73; see (85.13)]. Further see [7.3, (2), (4); 7.5, (1), (2); 8.2, (10); 8.5; 8.6, (1), (7); 8.7, (6)] for instances where Venn's book proved useful in our study of Boole's logic.

## Section 9.6

(1) G.B. Halsted (1853–1922) graduated from Princeton University in 1875, receiving four years later his Ph.D. on a "Basis for a dual logic" from Johns Hopkins University where he was the first student of Sylvester. On his commentaries on De Morgan's and Boole's work see [6.6, (2)] and [Venn 1881, 214, fn1; Grattan-Guinness 1991a, 20] respectively. On Halsted's suggestion to Murphy see also [Jourdain 1913, 127, fn].

(2) Our account of MacColl's work together with citations from his writings is based on the unique historical study of his logical contributions by Jourdain [1912]. Jourdain's account include details from Russell's critical review of Symbolic logic in 1906. According to Kneale [1962, 427, 549] MacColl influenced Lewis [1918] in connection with modal logic. Like Peirce and

- Schröder. MacColl contributed to probability logic [Hailperin 1988,186-7]. A biography of Mac Coll (1837-1909) is wanting, though he is being studied by T.Christie (Erlangen).
- (3) C.S.Peirce (1839-1914), son of the American mathematician B.Peirce (1809-1880), spent much of his life in seclusion, devoted to the study of philosophy. He is known as a father of semiotics. He took interest in inductive logic admiring the writings of Pèrè Gratry [Gratry 1944,1-3]. On his contributions to philosophy see [Passmore 1968,136-144; Styazhkin 1969,257-263]. On his logical inquiries his main novelties are stressed in [Bochenski 1970, 267,302,348,375-380; Grattan-Guinness 1991b; Hailperin 1988,184-185; Kneale 1962,427-434; Lewis 1918,79-85; Merrill 1978; Nidditch 1962,48-56].
- (4) Peirce admired profoundly the geniuses of Boole and De Morgan; he called the latter the father of the logic of relations. On his appraisals of Boole's and De Morgan's studies see passages cited in [Grattan-Guinness 1991b; Harley 1870,15; Hawkins 1979,32].
- (5) On (96.10) see Bochenski [1970,375-8]. Bochenski's account illustrates briefly the proximity between Boolean laws and those governing Peirce's logic of relations. Boole's early influence on Peirce is studied in Lewis [1918]. On Peirce's extension of Boole's law (84.10) see [8.4.(9)].
- (6) E.Schröder (1841-1902) attended the University of Heidelberg, passing his doctoral examination in 1862. He was a prolific mathematician and logician, particularly noteworthy being his support of Cantor's set theory which he was among the first to accept. Like Peirce he worked in isolation and as a result his contributions were not widely acknowledged by his contemporaries. He adopted Peano's postulates of arithmetic and the abstract conception of mathematical operations set forth by Grassmann and Hankel. Now primarily remembered for the term "logical calculus".
- (7) On Schröder's principle of duality for logical expressions in the logic of classes, as well as on his solution of the logical equation  $Ax+B(1-x)=0$  on Boolean lines see Styazhkin [1969,208-211]. Illuminating annotated instances from Schröder's axiomatization of propositional logic and logic of relations are traced in [Grattan-Guinness 1975,109-120]. This paper concerns Wiener's comparative study of the logics of Russell and Schröder. Further, an elementary comparative study of the symbolical procedures used by MacColl, Peirce, Schröder, Grassmann and other logicians is carried out in the last two chapters of Venn [1881]. A concise account of Schröder's work is included in Nidditch [1962,56-58].
- (8) Peirce's work was taken over by a group in the United States, his influence on Continental logicians exercised mainly on Schröder. The latter was of some slight influence on Russell and Peano. On the mathematical logic which

succeeded algebraic logic see [Grattan-Guinness 1988b].

(9) Frege is erroneously considered as having developed Boole's ideas [see Corcoran 1978,81]. However Bochenski [1970,271] and Jourdain [1912,250-55] hint at a comparison between the two logicians, the latter citing Frege's own comments on Boole's work.

(10) On Johnson, semi-follower of Boole who was against Jevons's work, see [Grattan-Guinness 1991a,21, ; Passmore 1968,135-6]. Huntington's axiomatic reformulation of Boolean algebra in 1904 is stressed in [Kneale 1962,423-27; Nidditch 1962,58-9]. Interesting comments with a partial reproduction of his axioms are found in [Grattan-Guinness 1975,113,118; Tarski 1941,77-78,fn3]. On Whitehead's work -and on Principia Mathematica which drew on Huntington's work (which, like Schröder's, was significant in the development of lattice theory)- see [Grattan-Guinness 1975,113;Nidditch 1962,58,77-79].

## Section 9.7

(1) A concise account of Brodie's life and work, with special focus on his calculus of chemical operations, is given by Farrar [1964]. A comprehensive account of the atomic debate, including a portion of Brodie's correspondence in 1867, is the topic of Brock [1967].

(2) [M.Boole 1972,49]. Not mentioning his name, M.Boole alluded to Brodie while discussing the power of signs in the investigation of the laws of numbers, chemistry and human thought, linking him appropriately with Degérando, Babbage and Boole. Harley contacted Brodie in October 1867, letting him know that he was chiefly interested in Brodie's researches "because of their obvious connexion with Boole's logical system: and I am very curious to see how you will construct an Algebra in which the symbols  $x, y, \dots$  obey all the rules of common algebra and yet are subject to the chemical law expressed by  $xy=x+y$  [(97.4)]". He sent to Brodie his biographical essay [1866a] on Boole [Brock 1967,133], but apart from his account given above in text, there is no evidence that he considered Brodie's calculus any further.

(3) It is true that Brodie's calculus bears a striking similarity with Boole's calculus of elective symbols as we shall see in text below. This similarity (unnoticed by Harley) is recently raised in Brock [1967,82-83]. Nevertheless, since Brodie did not refer to MAL in his paper, it is exaggerated to assert that "the most cursory glance at the Calculus reveals how much Brodie owed to [MAL] and Brodie himself recognized this" [Brock 1967,82]. On Brodie's most prominent source of influence - besides LT - see (10)-(11) below.

(4) [M.Boole 1931,v;7.1,(4)]. Solly was influenced from Kant's distinction between "analysis" and "synthesis". Notice that in Lacroix [1828] it was

geometry which offered a heuristic example of discussing the multidimensional notions of "analysis" and "synthesis" after Condillac [1.8, text below (23)]. In Solly it was chemistry which formed the basis for a relevant distinction. We might further draw an analogy between these two notions and the logical terms "aggregation" and "composition" as employed by De Morgan and Brodie.

(5) [S<sub>3</sub>, 120]. De Morgan repeated these views in a letter to Brodie in May 1867 [Brock 1967, 102; see (13) below]. Notice his reference to lawyers and his statement in [S, 155, fn2]: "Lawyers ought to be much of logicians and something of mathematicians" which recalls Kirwan's dedication of his Logick to students of law [6.2, (3)].

(6) The symbolic equivalence between  $x+y$  and  $xy$  is expressed by (97.4). Brodie did not mention explicitly that in formulating this law he drew on the equivalence between " $H_2+O$ " and " $H_2O$ ". See, however, his references in [1866, 795, fns] as well as De Morgan's very pertinent remark in a letter cited in (13) below.

(7) In his arguments against the views of Berthelot on the insignificance of scientific languages [1866, 784, fn] Brodie recalls of Babbage [1827] on the influence of signs [2.9]. In text he mentioned Herschel's opposition to the abbreviated expression " $H_2O$ " for " $H+H+O$ ", which was due to Berzelius, and his preference for the notation  $2H+O$ . It is of interest to note that Herschel's point of objection was just diametrically opposed to De Morgan's own given below (5); in other words, Herschel claimed that "the apposition of letters, being the algebraic sign of multiplication, cannot, consistently with the conventional principles of algebra, be employed to express the sum of two weights" [1866, 784]. On similar grounds Herschel reacted towards Brodie's memoir in 1867 [Brock 1967, 121-126] as we shall see in (16) below.

(8) [Brodie 1866, 781-2]. On the proximity between Brodie's and De Morgan's historical approaches see also [6.7, text and (17)].

(9) [1866, 787]. Brodie stressed again this point in [1866, 855] writing "we are not free to arrange and to interpret [symbols] according to the dictates of caprice, but of which each has a specific meaning assigned to it in the calculus from which the laws are deduced according to which it is permitted to operate upon it". These passages recall of Boole's list of the laws that render symbolical reasoning lawful in [LT, 68] cited in [7.2, text below (7)]. Brodie cited LT on numerous occasions in [1866, 795, 800-802, fns] and was additionally acquainted with Gregory [1840] -see (11) below. He might have been also influenced by De Morgan's algebra [1849c], a work referred to only in his second memoir [see (17) below].

(10) [1866, 787]. Though no reference is provided here, there is strong evidence [see (11) below] that Brodie drew on Gregory's paper "On the real na-

ture of symbolical algebra" [1840]. Gregory's paper discussed distributive and commutative algebraic operations paying special emphasis to geometrical examples influencing a generation of mathematicians in the direction of symbolic geometry [4.4;5.10,(4)-(7)]. Further, on our claim of Brodie's slight acquaintance with the calculus of operations, see text below on his correspondence with Donkin and De Morgan. Our comments should be seen in combination with those raised in (3) and (9) above.

(11) [1866,801,fn]. A parallelism between geometrical and chemical operations as in Gregory [1840] and Brodie [1867] respectively was carried out by Jevons in a letter to Brodie in 1867 [see Brock 1967,55-56].

(12) Jevons's arguments are closely similar to those applied by him four years earlier against Boole's algebraic calculus [see 9.5]. <sup>for</sup> details of the Jevons-Brodie communication and of the exaggerated character of the former's objections see (Brock 1967,54-57,115-119) and (15) below.

(13) From De Morgan's letter to Brodie of 19 May 1867 [Brock 1967,101-104]. See also our comments in (6) above.

(14) De Morgan had been interested in the properties of the operator E (introduced by Français (16.6)) since 1843 [see his letter to Boole in Smith 1982a,14]. Few instances from his inquiries were incorporated in his textbook on the calculus [1842c], while further inquiries are traced in his manuscripts written in the 1850's [see 3.9,(2),(6),(13)]. Notice that De Morgan's inquiries in the calculus of operations were directly influenced from those in the calculus of functions -see particularly the analogy between (97.12) and Babbage's transform (25.1).

(15) See Donkin's undated letter to Brodie cited in [Brock 1967, 115-16]. The theorem (97.17) is misprinted at page 116; read  $n/x(d/dx)^{n-1}$  for  $n/x d/dx$ . Donkin had earlier communicated to Brodie his inquiries on the necessary and sufficient conditions for  $U=f(x,y,\dots)=0$  to be a chemical equation, developing U by Taylor's theorem [Brock 1967,104-106].

(16) See Herschel's letter to Brodie on 13 June 1867 and Brodie's reply on 26 June 1867 in (Brock 1967,121-124,124-126). See also our comments in (7) above.

(17) [1877,74,fn]. Brodie's reference from De Morgan [1849,114] is checked

De Morgan's assertion has <sup>been</sup> traced at [1849,118-119]. It should be mentioned, however, that De Morgan's statement concerns addition of vectors, and in this sense it is not paradoxical. We would like to add that Brodie is unclear about the notation (1), here standing for the chemical operation while in [1867,803] for the arithmetical symbol 1. The citation in text reveals Brodie's partial influence from De Morgan's letters in 1867 [see text above and (14)].

(18) [Brock 1967,47-8; Brodie 1877,105-116; Farrar 1964,177-178]. It seems rather plausible that Brodie was under Gregory's influence when he wrote [1877,110] that the proof of Taylor's theorem of expansion depends solely upon the commutative and distributive laws, and therefore it can be applied for symbols of chemical operations [see (10) above and (44.7),(97.6)]. Further on his discussion of Taylor's theorem with De Morgan and Brodie see [text and (14)-(15) above].

(19) Sylvester wrote in his paper "On an application of a new atomic theory" [1878,64-65] "The factors of any algebraical form may be regarded as in some sense the analogues of the rays of atomicity in the equivalent chemical area.....".

### Section 9.8

(1) This passage from De Morgan's writings is cited by Boole's biographer Neil [1865,81]. On its original source De Morgan [1872] see [De Morgan 1915,2,78]. On Neil's comparison between the two logicians see [7.1,(1);9.3, text below (6);9.5, text below (3)].

(2) On the reasons 1-5 see details in [Tables 2-5 in 9.2-9.3]. Instances of an evident competitive spirit and of a tendency to note errors and complications are revealed, among other cases, in those of Babbage-Herschel [2.4-2.9;9.2], Boole-Bronwin [5.2;5.4; 9.3] and Boole-Carmichael [5.7-5.8]. While the last reason above listed was often connected with one of the principal ones 1-5, there were papers, e.g. by Bronwin, Curtis, Donkin, Williamson, C.Graves, Spottiswoode and other semi-followers of Boole and Carmichael, produced principally under the motivation to commentate upon one another's work and to reformulate known theorems in a slightly more general or elegant way, symmetry and beauty in form being the ultimate scope [see, for example, 5.2-5.8;5.10,(3);9.3, below (1)].

(3) The difference between generalization by induction and extension in algebra was first hinted at by De Morgan in 1835 [4.3, text and (17)-(22)]. Applying this latter notion in his calculus of functions [3.5-3.6], he further stressed it and employed it in connection with his arithmetical system in logic in  $S_2$  [6.6, text and (1),(9)]. Moreover, the theorems of the calculus of non-commutative operations are genuine extensions and not direct generalizations of the relevant theorems of algebra [see (45.13),(52.45),(57.15) and (93.4) as few representative cases of extension]. Further, 9.3 is devoted specifically to the importance of this issue as linked with the form-matter distinction.

(4) On the reasons 1)-2) see particularly our summary in 5.10. On 3)-6) and 7) see [9.4, text and (9)-(16)] and [5.9-5.10,9.4, (2)] respectively. Few com-

ments relevant to the last reason 8) can be found in [6.9,8.7-8.9;9.4;9.7].

(5) On Heaviside's work and its proximity with Cauchy's, Gregory's and Boole's on operator methods see [Cooper 1952; Petrova 1987,1.7;4.4-4.5;9.1,(2)].

(6) On the misinterpretation of Boole's algebra and its exaggerated connection with computers see [7.1,(21)-(22);8.4,(6);9.1; 9.5-9.6; Grattan-Guinness 1991a].

(7) [Spottiswoode 1878,29-30]. See also citation in text below (1).

(8) Comments and references on topics related to the calculus of operations and its wider applications in mathematics, as well as to Arbogast's influence see [1.7; 4.3,(6); 4.8,(3); 5.7,(10),(12)-(14);5.9,(10);5.10,(4)-(6),(11);9.2,(2);9.3-9.4].

(9) [Glaisher 1890,723]. Glaisher's appeal to the lunar theory brings us back to the mid 1830's when the attention of Cambridge scholars had focused on a more efficient treatment of the theory of attractions and the solution of equations belonging in the class of the EFE. This attention gave rise to the treatises of Murphy and Pratt, and later on to Boole's general method in analysis [1844]. Perhaps Glaisher is appealing here to a new general method which would be less clumsy than the series method which had flourished again with Cayley and himself in the 1860's and 1870's. In a way, Boole's spirit revived in a flash in Glaisher's speech, but his hopes remained unrealised [see also Bronwin's arguments on the impossibility of the existence of one general method in 5.4,(6)].

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